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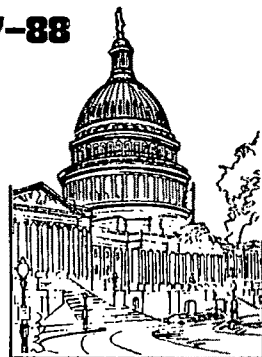
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ARTICLES

THE GROWTH OF DEMOCRATIC LEGITIMACY IN SPAIN

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The political transition in Spain provides a rare opportunity to monitor popular attitudes toward alternative regimes. Through the analysis of national surveys conducted in 1978, 1979–80, and 1984, we first establish that the Spanish public distinguishes not only between successive governments—the Franquist and the center-right and socialist governments of the post-Franco period—but also between Francoism and democracy as political systems. Second, we show that during the post-Franco era the criteria of legitimacy have begun to shift from formal political to social democratic values. These analytical results are achieved by comparing standard with less orthodox measures of political legitimacy and performance, and by revising conventional theories of system support. Third, we estimate the determinants of support for and opposition to the two regimes. The Franquist system remains more polarizing than does the democratic system; the constituencies of the democratic regime are considerably broader and more heterogeneous. However, while the new democratic state is comparatively inclusive and autonomous, low rates of political participation and changes in traditional socialist ideology have made the institutional bases of legitimacy ambiguous.

Spain's break with authoritarianism since the death of Franco in 1975 has been substantial and on occasion dramatic. The democratic regime has survived the onslaughts of regional separatists and of reactionaries in the military. How legitimate, then, is the new political system? The transfer of power from a center-right party, the Union of the Democratic Center (UCD), to the center-left socialists (PSOE) that took place in the general elections of 1982 makes it possible, in principle, to dis-

entangle support for government incumbents—the authorities of the hour—from commitment to the presumably more basic regime or political system. The succession of governments gives us a chance to distinguish between Francoism and democracy.¹

A primary objective of our study is to determine whether the split between Francoism and democracy holds up in the perceptions of Spaniards themselves. After the euphoria of the early days of the transition, the citizenry may have settled

back into its proverbial cynicism about *los de siempre*, the powers that be, without making any meaningful distinction between new and old leadership and institutions. Such an outcome is hardly unthinkable, given the slide in economic performance and the rise in street crime during the post-Franco period. "Against Franco," the wisecrack went in Madrid, "we lived better."

Theoretical Perspectives

Most approaches to political legitimacy make two related distinctions. One is between system support based on affective, diffuse commitment and governmental popularity based on policy effectiveness or performance. This distinction derives from a split in the criteria of support: the former is thought to be deep and slow to change, the latter shallow and more labile. Another, partially separate, distinction concerns the objects rather than the criteria of support. It postulates a threefold division among the authorities, the regime, and the community.

In bare outline, this is the foundation of mainstream theorizing about political legitimacy (Easton, 1975, 1976; Lipset, 1960). Empirical confirmation, however, has been elusive, and in Spain we encounter an anomaly that contradicts the theory.¹ On the one hand, presumed measures of system support vary across governments. The socialists elicit much greater trust than did the center-right government that preceded theirs, yet the regime within which both governments have operated is the same. On the other hand, indicators that supposedly tap evaluations of performance in economic and social as well as political areas separate the Franco and post-Franco regimes from each other without revealing sharp differences between the post-Franco, democratic governments. These results challenge the received wisdom about political

legitimacy. Neither the "criteria" nor the "objects" guidelines work out.

The first step in solving this puzzle entails a recognition that legitimization in Spain may follow an evolutionary dynamic that reflects a shift from political to social standards. The change in priority from formal, or political, democracy to social democracy has been documented elsewhere in Western Europe (Marshall, 1964; Weil, 1985). This movement helps account for the caution with which the Spanish public treated the socialists in the early days of the political transition, and the greater enthusiasm accorded to the socialists once the democratic regime had been consolidated. There are problems with viewing this trend as inexorable: the socialists have come to power when socialism as an economic doctrine has gone out of favor. Nevertheless, it serves to substantiate a crucial theoretical point: the values on which legitimacy is based may shift within the same regime.

The second step begins with the unexceptional idea that in polities such as Spain—or Argentina and Brazil, for that matter—where regime change has occurred within the living memory of most citizens, the public is likely to distinguish between types of political systems, and not just among different incumbents and governments within these systems. Dichotomies between regimes overshadow possibly finer distinctions, based on multiple criteria and dimensions, within regimes. On the contrary, in polities where no regime change has taken place recently, contrasts between the legitimacy of different regimes make almost no sense—alternative polities are virtually impossible to imagine (Sniderman, 1981). In this case citizens are likely to discriminate among different governments according to performance criteria.

This line of argument has its limitations, too. One stems from the possibility that an overarching sense of legitimacy may decay, rather than accumulate, over

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time as the "common enemy" (e.g., Francoism) recedes into the past. Still, it helps resolve the paradox of the failure of supposed indicators of performance and policy effectiveness to distinguish between evidently different governments, when the overriding contrast is between rival political systems, both of which most citizens have lived through.

As the parallels with Brazil and Argentina suggest, the theoretical implications of our analysis are not unique to the Spanish experience; for this reason, the logic of the analysis needs to be kept plainly in view. While we take the tripartite division across political authorities, regime, and community as potentially verifiable, we depart in several ways from the methodological procedures it implies. The most conspicuous is by way of innovation in measurement, but the most fundamental revision is conceptual. Imagine the authorities-regime-community continuum as the column axis in a matrix where the rows are formed by three functional dimensions: political, economic, and social (McDonough, 1981; Walzer, 1983). Suppose, further, that Spaniards evaluate the government, the regime, and possibly the community by these different standards.

Now, the democratic regime (if such an entity exists in the eyes of the Spanish public, apart from the governments that make it up) might be perceived as legitimate in *social* terms—for example, as reducing, or properly trying to reduce, the disparities between rich and poor. In this case the standard would be one of equity or fairness (Tyler, 1984). All the same, it might be regarded as a disaster by *political* standards—e.g., for failing to maintain law and order. Similarly, it might be viewed as efficient along *economic* lines, for promoting economic growth, while failing by one or both of the other criteria. For example—and to complicate matters further—the new regime might be judged as adequate by

both social and economic standards, while it is seen as no more, or perhaps even less, legitimate politically than the Franquist system.

Thus, legitimacy can be understood according to its range, or scope, as well as by depth, or levels. In effect, it is multi-dimensional. The political component of the "system" may be legitimate, while the economic and/or social components are not, or are only partially so, and vice versa (Dahl, 1985; Vidal-Beneyto, 1984). The putative compartmentalization of the dimensions of legitimacy is an empirical as well as a theoretical question, as is the supposed separation of a shallower government from a deeper regime.

In one respect, our focus on the variable dimensions of legitimacy is not particularly novel. It systematizes and makes explicit earlier treatments of the phenomenon that were occasionally casual and abstract. In another sense, however, the approach is controversial. The differentiation and, some might argue, the proliferation of multiple criteria of legitimacy run the risk of confusing indicators of regime affect with measures of government evaluation. It is difficult to settle such matters a priori without falling into inconsequential taxonomical refinements. Our provisional solution is to estimate the overlap or separateness of the presumed system and apparent performance indicators by calculating the associations between them.³

A final question remains to be resolved. Although the foregoing argument might be a plausible addition to the customary authorities-regime split, it does not apply to the level of political community as generally understood. In fact, empirical studies of political legitimacy have ignored this level, and have focused instead on the authorities-regime dichotomy, with uneven results. However, it is unrealistic to ignore the distinctive and, conceivably, alternative political communities or subcultures in Spain. Regional

groups that reject incorporation under a Spanish nation, much less a sovereign national government, provide the most familiar examples; elements on the extreme left and right furnish others.

The possibility of regional and ideological disaffection raises a general question: if differences between regimes, beyond this or that set of authorities, can be detected, what are the social bases of system and antisystem sentiment in the larger community? The first part of the question is a function of theoretical precision and definition, and a successful response to it depends on careful measurement. The second part of the question assumes that the first has been answered, and focuses on the determinants or the demographics of legitimacy.

Estimating the links between legitimacy and its predictors is fairly straightforward. Indicators of system support stand on the left side of an equation in which the right side is made up of indicators of various divisions in the community—for example, region, class, rural/urban, and so on. Still, the analytical implications of differing empirical outcomes need to be anticipated. Two theoretical traditions, one dealing with the relative autonomy of the state, the other with civil religion, help place the quantitative results in perspective.

The notion of civil religion, originating with Rousseau and elaborated most recently by Bellah (1970), is associated with an ideal of political community that is autonomous from parochial and primordial loyalties, such as religion and region. Not exactly a crude patriotism, nor necessarily an ideology of statism, it reflects the evolution of a political culture that grants the public sphere a life and merit of its own. In the European and American experiences, civil religion has been most nearly approximated by the republican virtues represented by the solid citizens of Renaissance city-states and by the founding fathers of the United

States (Bellah and Hammond, 1980).

Civil religion implies a positive political model in the moral sense that the public sphere is looked upon not only as autonomous, but as the setting for education in political virtue and civic responsibility (Almond and Verba, 1963; Fliegelman, 1982). Theories of the autonomy of the state make no such claim. The fundamental notion is that "the state"—an entity virtually identical to "the regime"—enjoys medium- and probably long-term policy discretion vis-à-vis the classes and interests ("the community") supporting as well as opposing it (Evans, Rueschemeyer, and Skocpol, 1985; Nordlinger, 1981). The state apparatus may operate for some time in a practically autonomous manner, whether or not its decision-making discretion is enhanced by cultural hegemony—that is, by a "civil religion."

The relevance of these ideas to the understanding of political legitimacy in Spain is reasonably clearcut.⁴ An important criterion of legitimacy is the extent to which the new regime, as compared to the Franquist system, is autonomous in the sense that it is not beholden to a narrow cluster of class interests or regional or religious communities. Empirically, this would be reflected in a relatively low polarization associated with the democratic regime, as compared to the strong polarization surrounding Francoism. In short, the new regime should not be strongly identified with any particular class, group, or community. It should be seen as more impartial, and its support less narrow, than the system that preceded it.

Such an outcome is mysterious if it is assumed that only authoritarian governments can approach autonomy, by reason of their supposed "strength." Conversely, democratic government, being "responsive," might be characterized as less autonomous, overloaded by "special interests." The trouble with this line of reasoning is that it presupposes a thor-

ough incomparability between the exclusionary, restricted nature of Francoism, and indeed of most authoritarian systems, and the inclusionary and almost certainly more heterogeneous bases of democratic regimes (Epstein, 1984). Our position is simply that inclusiveness as a basis of legitimacy in the democratic case is to autonomy what "strength," or force, is for authoritarian regimes.

Legitimacy in these terms—in effect, as autonomy, whether democratic or authoritarian—is still circumscribed. The fact that the new regime may be more socially encompassing, less politically restrictive, and more autonomous than Francoism is an important component of legitimacy. The distance between the new regime and its social bases constitutes a slack resource that probably gives political leadership room for maneuver, but this flexibility—the reverse of the ideological and dogmatic style long identified with public life in Spain—is not itself a political philosophy, and it is not equivalent to a civil religion (Aumente, 1985b). The legitimacy of democracy in Spain may be constrained not only by the inexperience with mass politics ensuant on 40 years of Francoism, but also by the fact that Spain is renewing its democratic venture at a time when trust in and affiliation with the mainstream institutions of democratic politics, such as parties and trade unions, are stagnant or falling off, and when the social democratic, Keynesian suppositions—the surrogate civil religion—of the postwar period are suffering a crisis of credibility (Berger, 1981; Sotelo, 1984).⁵

In summary, the new regime has broken free of the essentially parochial and sectarian props of Francoism; the scope of its support is wider and more varied. It is less polarized and more diffuse. This inclusiveness is an indispensable feature of democracy, as contrasted with authoritarianism.

However, whether the Spanish demo-

cratic system has developed a political vision or culture of its own to incorporate diverse constituencies into a coherent political community remains problematic. A potential cost of depolarization is a lapse into political privatization, with low rates of involvement in public affairs and an uncertain commitment to democratic procedures (Bellah, Madsen, Sullivan, Swidler, and Tipton, 1985; Habermas, 1984; Keane, 1984). By itself, the ambiguity of political values in Spain is not a symptom of rampant anomie, since much of it reflects a new-found ethic of toleration; in the Spanish experience the alternative to such "relativism" has been composed of variations on "maximalism," "triumphalism," and related crusades that involve killing for an ideal. Yet, when considered alongside levels of political involvement and participation that seem low even after allowances are made for the fledgling nature of Spanish democracy, this inchoate pluralism forces us to contemplate what institutionalized legitimacy means for ordinary Spaniards.

The conceptualization of political legitimacy presented here is unorthodox, and it is well to lay out how the ideas will be tested. The analysis proceeds in two principal stages, with an epilogue. The first task is to define and detect—to "operationalize"—legitimacy. This entails determining whether Spaniards view Francoism and the democratic governments following it as two distinct regimes, as essentially the same, or as nothing more than a series of different governments, without distinguishing genotypes. We accomplish this by examining the extent to which Spaniards perceive the political systems as coherent wholes, differing more from one another than within themselves on grounds of economic, social, and political performance. The basic finding is that the Spanish public discerns a bipartite split between Francoism and democracy.

With the evaluation of regimes sep-

**Table 1. Measures of Political Affect and Institutional Support:
Marginal Distributions (in percentages)**

Year	Trust Government?		Government Favors:		Tax Money Is:		
	Yes	No	Many	Few	Spent Well	Partly Wasted	Wasted
1984	41	59	58	42	23	48	29
1980	26	74	32	68	12	44	43
1978	27	73	35	65	12	47	41

Year	Elections		Congress		Monarchy or Republic? ^a		
	Needed	Not Needed	Needed	Not Needed	Republic	Indifferent	Monarchy
1984	91	9	81	19	—	—	—
1980	94	6	86	14	23	56	21
1978	93	7	87	13	32	42	26

Note: Sampling and question wording information is provided in Note 6.

^aNot asked in 1984.

arated from the perception of mere governments, the second stage is to estimate the inclusiveness of the Franquist and democratic systems. This operation involves assessing the degree of polarization surrounding each regime—that is, the degree to which they are seen as incorporating particular groups and distinctive subcultures at the cost of marginalizing others. As a practical matter, the test means determining the weight of socioeconomic predictors such as class, region, and religion in accounting for variation in support for and opposition to the regimes.

Finally, we explore the possibility that political autonomy, treated as the comparative freedom from ties with distinctive social clusters and subcultures, is only a partial indicator of legitimacy. This exploration is devoted to a tentative analysis of what might be the emerging bases of a positive political framework in Spain.

Standard Measures of Legitimacy

Table 1 presents the univariate distributions of some traditional indicators of

system support across three points in the post-Franco period: in 1978, during the first term of the center-right government led by Adolfo Suárez; in 1980, during the second term of the Suárez government, when what Spaniards call a certain *desencanto* (disenchantment) with the democratic process appeared to have set in; and in 1984, two years into the socialist government headed by Felipe González.⁶

Rudimentary as they are, the data furnish evidence of both stability and change. The consensus favoring democratic institutions—the need for elections and for the preservation of representative politics through the Cortes—is overwhelming and steady. However, variation in political affect—for example, in trust and in perceptions of the majoritarian versus elitist inclinations of the government—is considerable. A sharp break occurs between 1980 and 1984, between the UCD and the PSOE governments. The jump in positive affect coincides with the incumbency of the socialists.

None of these results corroborate expectations about crisp demarcations be-

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Table 2. Political Affect and Institutional Support by Party Identification
(in percentages)

Indicators	Party Identification ^a						
	PSOE	PCE	None	CDS (UCD)	Right	Local	Other
<i>Trust Government</i>							
1984	60	39	35	25	23	23	16
1980	18	13	29	44	25	16	2
1978	20	14	28	53	22	23	16
<i>Government for Many</i>							
1984	75	47	53	55	36	44	39
1980	26	10	34	60	40	16	13
1978	27	14	36	65	41	18	20
<i>Taxes Well Spent</i>							
1984	38	18	17	9	13	13	8
1980	7	5	12	32	21	7	3
1978	6	2	13	29	14	5	9
<i>Elections Needed</i>							
1984	96	93	89	90	84	92	87
1980	97	94	93	95	81	94	85
1978	96	93	93	96	85	97	85
<i>Congress Needed</i>							
1984	85	83	78	84	71	83	79
1980	91	89	84	88	79	81	79
1978	91	85	86	92	69	91	78
<i>Favor Republic</i>							
1980	29	65	14	2	6	42	76
1978	37	74	18	8	6	31	58

Note: Information for question wording and sampling is provided in Note 6.

^aThe CDS is a small centrist party with some resemblance to the once much larger and now defunct UCD. The PCE is the Community party of Spain. The "right" included mainly the Alianza Popular, with a few adherents of the Fuerza Nueva. "Other" indicates the primarily extraparlimentary mini-parties of the left.

tween government and regime or between popularity and legitimacy. Table 2 provides further evidence that shifts in the traditional indicators may be tied closely to alternation in government rather than to regime change. The measures of institutional and affective orientation are broken down by partisan identification.

The partisanship of Spaniards strongly influences their supposedly generic political views. With the socialists in power, 60% of the PSOE supporters indicated trust in the government. When the socialists were in opposition, the corresponding figure among PSOE partisans was about one-third as large, roughly 20%. Con-

versely, under the reign of the UCD, about 6 out of 10 UCD identifiers said that the government was "for the many," while only about 1 out of 4 socialists had the same opinion. Rather than tapping longstanding dispositions, fluctuations in these indicators seem to be quite contingent on specific incumbents.

However, another explanation is also compatible with this evidence, even though it is not plainly superior to the logic of reduction-to-political-circumstance. The sharp increase in esteem for the socialists may reflect a change in the bases of support, from liberal-political to social-egalitarian values. The fact that

commitment to the institutions of pluralism holds up at a constantly high level, while trust and the other indicators evidently tied to the fortunes of the socialists soar, contravenes zero-sum interpretations of the trade-offs between the versions of democracy. Whether "support" entails legitimacy, popularity, or some mixture of the two, remains indeterminate.⁷

The inferential ramifications of the standard measures are not always negative or ambiguous. The political orientations of those who identify with conservative parties—mainly, the Alianza Popular—are very stable, relative to those of Spaniards in the center and toward the left. It would be misleading to conclude that the Spanish right maintains a uniformly dim view of democratic institutions and values. Rather, they do not distinguish as sharply as others between the center-right and the socialist left. Hence, their trust in and assessments of the fairness and efficacy of the government remain for the most part unaffected by the changes in government that have occurred so far.

Nevertheless, the broader lesson of these first-cut results is that we are not going to get very far with the analysis of system support by sticking to conventional indicators of the construct or, indeed, by leaving standard theories of legitimacy intact.

A Revisionist View of Political Performance and Legitimacy

Table 3 traces the movement in several indicators of political orientation, all of them scored between 1 and 10, across the time points plotted earlier for the traditional indicators. The new measures are of three types. Toward the top of the table are generic indicators, such as satisfaction with "the way democracy is working nowadays in Spain." The next set taps perceptions of how well different govern-

ments, including the Franco government "during its last ten years," did or have been doing along three dimensions: economic ("improving the standard of living"), social ("treating Spaniards from different social classes equally"), and political ("maintaining law and order"). A third group of indicators is composed of popularity ratings for major figures in Spanish politics, including the Pope and Juan Carlos, the king; the reasons for introducing these variables will be set forth momentarily.⁸

Two trends should be noted at the outset. The first is the recovery of assessments favoring democracy and the governments and leaders associated with it, after a downturn at the beginning of the 1980s. One of the most striking patterns is the recuperation of perceptions regarding how well democracy is working. They reach a high of 5.5 in 1984 after having fallen to 4.1 in 1980. A similar upswing can be detected in the popularity of leaders whose fortunes have been associated with the new regime—notably, Suárez and González.

It is not difficult to come up with a plausible explanation for these curvilinear trends. In the face of a deteriorating economy, mounting terrorism, and increasing crime, Spaniards began to exhibit a certain fatigue with democracy toward the beginning of the 1980s. Their spirits lifted with the coming-to-power of the socialists, with the implicit hope of better economic as well as political times, and, even earlier, with signs that terrorism was waning (Cebrián, 1985).

A second set of trends is linear but more complex. There has been a steady decline in the popularity and the fortunes of the communist left. Santiago Carrillo resigned as leader of the party, to be replaced by a younger militant, Gerdardo Iglesias, whose following seems no greater than that of his mentor (Paramio, 1985a). An almost obsessive moderation has marked the transition to democracy in

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**Table 3. Average Scores for Selected Indicators of Political Orientation,
1978-1984**

Indicators	1978	1980	1984
<i>Satisfaction:</i>			
With Democracy	4.45	4.19	5.54
With PSOE Government ^b	—	—	5.04
With UCD Government	4.52	4.17	4.42
With Franco Government	3.58	3.74	4.13
<i>Living Standards:</i>			
Under PSOE Government	—	—	4.97
Under UCD Government	4.26	4.04	4.24
Under Franco Government	3.94	4.31	4.47
<i>Law and Order:</i>			
Under PSOE Government ^b	—	—	4.70
Under UCD Government	4.24	3.18	4.25
Under Franco Government	4.94	5.15	5.31
<i>Social Equity:</i>			
Under PSOE Government ^b	—	—	5.04
Under UCD Government	3.82	3.62	4.27
Under Franco Government	2.82	2.93	3.77
<i>Popularity:</i>			
The King	6.44	6.50	7.97
John Paul II ^c	—	6.79	6.92
González	5.60	5.45	6.50
Suárez	5.42	5.08	5.37
Carrillo	4.30	3.94	3.26
Iglesias ^d	—	—	3.32
Fraga	3.27	2.94	3.68

Note: Information regarding survey is provided in Note 6.

^aResponses were scored on a scale of 1-10, with 1 being the lowest score and 10 being the highest.

^bThe PSOE (Socialist Party) did not assume power until 1982; its governmental ratings were therefore not elicited until the 1984 survey.

^cOmitted in 1978.

^dGeraldo Iglesias succeeded Santiago Carrillo as secretary-general of the Communist Party of Spain in 1984.

Spain. Despite its protestations of Eurocommunism, the PCE has been tainted by a simultaneously extremist and manipulative history, and has been unable to shake its image as the repository of the anti-democratic left (Mujal-Leon, 1983).

By contrast, memories of Franco have become rosier, the farther the dictator fades into the past. The absence-makes-the-heart-grow-fonder effect is most obvious when it comes to assessments of law-and-order accomplishments. Neither does Franco's absolutely low, but still rising, grade with respect to social equity require

much explanation. The rudiments of a welfare state were built during the last decade of Francoism; to many, the job tenure and medical benefits of that period compare favorably to post-Franco unemployment rates and the hazards of the underground economy (Flora and Heidenheimer, 1981; Lancaster, 1979). Furthermore, if recent economic conditions in Spain are set against the boom times of the later Franco period, his prosperity rating is also understandable.

For all this, with the exception of the law-and-order criterion, the ratings of the

socialists still exceed those given to Franco, even if his image continues to improve with age.⁹ The two regimes are rated differently by Spaniards, as are the center-right and center-left governments of the democratic regime itself.

Nevertheless, the creeping nostalgia for the good times, if not the politics, associated with the Franco period, and the fluctuating esteem in which post-Franco governments have been held place the legitimacy of the new political system at issue. What do the measures reviewed so far reveal about the solidity of democracy in Spain?

The simplest way to proceed is to see how the indicators hang together—the degree to which they are correlated—in light of theoretical expectations about the links between the areas of governmental performance and the dimensions of regime legitimacy. Consider the standards used for evaluating each of the governments—Franco's, the center-right UCD, and the center-left PSOE. In the present case, four main outcomes of a data-reduction technique such as factor analysis are possible. The least likely is that no significant differences emerge across either the three governments or the three criteria of evaluation. The outcome is improbable in view of the evidence presented in Table 3, which shows that the governments and their functions are perceived differently.

Another, more intriguing and perhaps likelier outcome would be reflected in the emergence of two or three distinct functional factors that do not, however, discriminate among the different governments. Such a configuration might lead to the inference that while Spaniards are quite capable of placing greater or lesser emphasis on, say, social equity as compared to economic growth, they see virtually no difference between governments in the implementation of these goals. This pattern would also imply that the governments—and regimes—are epiphenomenal,

and that it is differences in the priority they attach to economic and social goals, together with their success in reaching them, rather than their democratic or authoritarian "processes of decision making," that determine their legitimacy. System support, such as it is, is driven by performance and effectiveness (Anderson, 1970).

There is a third, quite different pattern that seems at least equally probable. This would occur if Spaniards distinguished sharply among the three governments, with the implication that they perceived each of them serially, as separate policy and leadership packages. The pattern would demolish notions of regime legitimacy as something separable from support for particular governments, at least with the indicators at hand. Finally, results along these lines would reinforce the suspicion that the indicators themselves are unlikely measures of system support, but instead simply register perceptions of efficiency in different policy spheres.

By now the contours of the fourth major outcome should be evident. This would entail a division not among the three governments, but between the two, "qualitatively different" regimes, with the Franco regime on one side and the combined UCD/PSOE regime on the other. Only under this configuration would the typological separation of authoritarianism and democracy be substantiated empirically.

The data confirm the fourth outcome. Table 4 displays the results of a factor analysis of perceptions of economic, social, and political performance for the Franco regime and the two post-Franco governments.¹⁰

The key result is that two, and only two, dimensions emerge: one for Franco and one for the combined democratic governments. What does not happen is equally significant. Three separate factors for the different governments do not

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**Table 4. Oblique (Promax) Factor Matrix with Standardized Loadings:
Franco, UCD, and PSOE Evaluations, 1984**

Evaluation Indicators	Factors ^a		Communalities
	Authoritarianism	Democracy	
<i>Franco:</i>			
Standard of Living	1.01	-.11	1.02
Social Equity	1.00	-.05	1.01
Law and Order	1.01	-.11	1.02
<i>UCD:</i>			
Standard of Living	.52	.80	.91
Social Equity	.49	.82	.91
Law and Order	.39	.88	.92
<i>PSOE:</i>			
Standard of Living	-.24	.96	1.09
Social Equity	-.25	1.00	1.05
Law and Order	-.43	.95	1.09
Sum of Squares	4.11	4.92	

Note: Information regarding survey is provided in Note 6.

^aCorrelation between factors = .11.

appear, nor do we pick up stray factors associated with one or another of the functional dimensions. The authoritarian and democratic regimes are viewed "in the round," as not only more different from each other than particular governments might be, but also than any similarities in specific functions or policy areas might suggest.

All the same, it is important to recognize that the UCD government, while tied, together with the PSOE government, to the democratic regime, is perceived as being genuinely transitional. As the loadings of the UCD government on the Franco factor indicate, it leans a bit toward the authoritarian dimension, despite the fact that its center of gravity is with "democracy"—in effect, coalescing with the PSOE. Thus, even though the center, at least as incarnated in the UCD, has disappeared in Spain, the public understands that the UCD carried out a crucial role in the early days of the transition.

The data also allow us to view the

nature of the transition, still from the viewpoint of the Spanish public, from a more synoptic perspective. We can chart the correlations between the authoritarian and democratic dimensions that emerge from factor analyses conducted on the 1978 and 1980 as well as the 1984 data (McDonough and López Pina, 1984). The purpose is to see how separate or close the two dimensions become over time and, by extension, to estimate how smoothly or abruptly the transition from Francoism to democracy unfolded in the eyes of ordinary Spaniards.

For 1978 the figure was .49, and in 1980 .38. The 1984 correlation, reported in Table 4 is .11. The steady decline in the correlations suggests (a) that, certainly in its initial stages, democratization was seen as genuinely transitional by many Spaniards, rather than as a sharp break with the past, and (b) that over time the democratic regime has taken on a life of its own, becoming progressively dissociated from the Franquist past.

In summary, it appears that Francoism

and democracy are perceived not only as distinct regimes but, reflecting the fairly smooth transition, as regimes that become progressively differentiated with the passage of time. To be sure, the Spanish public distinguishes among actual governments—for example, between the UCD and the PSOE incumbents. Nevertheless, Spaniards also discriminate, at a deeper level, between the Franquist and democratic regimes. This is what the factor analysis, which seeks underlying dimensions, evidently succeeds in demonstrating.

The obvious objection to this conclusion is that the indicators do not measure legitimacy at all, but continue to reflect mere performance criteria. In some quarters factor analysis has a faintly alchemical air. Whatever the statistical results, an interpretation that opts for a system-support reading of the data depends to a great extent on the validity of the measures used as input to the analysis. In the following section we shall argue that this objection becomes difficult to sustain to the degree that reified divisions between political legitimacy and efficiency are relaxed.

The essential point, however, is that historical circumstances surrounding the Spanish transition strongly condition the analytical results. The proximity in time of the two regimes tends to submerge differences between the post-Franco governments that would otherwise be more prominent. In another setting—say, Spain 20 years hence, or almost any of the present-day polities of Western Europe that reestablished their democratic institutions after World War II—differences across governments, based on the evaluation of performance, might come through unequivocally. The appearance of differences between regimes, however, would be most improbable. Such a possibility seems to require the experience of, or the opportunity to imagine, alternative political systems (Aumante, 1985a).

The Phases of Legitimacy

The common thread running through the analysis presented so far is that the data cannot be interpreted in a narrowly positivistic manner, as if "system support" and "legitimacy" were timeless Platonic abstractions or ideal types impervious to reasonable measurement or, indeed, beyond the grasp of Spanish citizens. The fact that the context of our study of regime support is a transition from authoritarianism to democracy, rather than a setting in which democracy has been long established, shapes the meaning of the empirical results.

Our argument rests on two developments: the historical contrast between the Franco and post-Franco regimes, and an evolution, within the democratic regime, in the bases of system support. The inter-regime contrast and the intraregime dynamics can be understood as successive phases of legitimization. Table 5 documents some of the links between these phases. It presents the correlations, for each of the three surveys, between the standard measures of political assessment used at the outset of the analysis and the summary indicators (factor scores) of perceptions of the Franco and post-Franco systems. The coefficients for satisfaction with the way democracy is working provide a validity check on the use of the "democracy dimension" constructed from multiple measures.

The democracy factor and the satisfaction with democracy indicator parallel each other in their size and stability. The magnitude of the correlations with the standard indicators is about equal, and the correlations hold steady over time. The correlations are positive, since the indicators all tap aspects, whether of the legitimacy or the efficiency, of democratic governments. The correlations with the institutional measures (the need for elections and congress) are uniformly modest, in large part because there is great consen-

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Table 5. Correlations between Authoritarianism Factor Scores, Democracy Factor Scores, and Satisfaction with Democracy, and Indicators of Political Affect and Institutional Support

Affect and Institutional Indicators	Authoritarianism			Democracy			Satisfaction with Democracy		
	1978	1980	1984	1978	1980	1984	1978	1980	1984
Trust in Government	.18	.25	-.15	.45	.41	.47	.37	.40	.38
Government favors the many	.25	.29	-.11	.45	.41	.44	.36	.36	.36
Tax revenues spent well	.22	.23	-.17	.44	.42	.45	.37	.32	.36
Elections needed	-.06	-.09	-.12	.17	.17	.20	.15	.19	.20
Congress needed	-.09	-.09	-.14	.19	.22	.19	.18	.23	.21
Preference for Monarchy over Republic	.43	.38	—	.24	.18	—	.26	.16	—

Note: Description of survey is provided in Note 6.

sus—that is, little variation—surrounding these questions.¹¹

The pattern displayed by the Franco factor is more complex. In 1978 and 1980, while the correlations of the Franco factor with the institutional measures are mildly negative, the coefficients related to such measures as trust in the democratic government are in fact positive, though not robustly so. This differential interaction between orientations toward Francoism and orientations toward democracy echoes what was suggested earlier: that the new regime was viewed as genuinely transitional, with links to the past, rather than as a venture in swift, radical change. However, by 1984, when the socialists had been in power for two years, the relations of the Franco factor with all the measures connected to democracy have become inverse. The coefficients are small, but their signs are consistently negative.

The change in the connection between orientations toward Francoism and toward democracy, as conventionally measured, must be understood in conjunction with the stability of the connection between orientations toward the democratic regime and the traditional measures of system support. The accession of the socialists to power reversed

whatever links existed between Francoism and democracy. But the change of government did not alter the commitment of anti-Franquists to the democratic regime. On the one hand, the stability of the results buttresses the conviction that the data reveal orientations toward a political system that is in fact larger than specific governments, and that differs from the preceding regime. On the other hand, the change in the results, signified by the progressive separation between orientations toward Francoism and democracy during the later phase of the transition, suggests that the basis of legitimacy has expanded from liberal democratic toward social democratic norms. The perceived opening toward social democracy, or toward a variant of populism, is more threatening to Franquists than the earlier stage of the transition toward democratic capitalism under the aegis of the UCD.¹²

Table 6 presents the same data in a disaggregated form that makes it easier to spot the specific contrasts between and within regimes. It is composed of a series of analyses of variance in which the dependent variables are standardized scores on the authoritarianism and democracy factors.

One remarkable feature of these data is the confirmation they provide of the long-

Table 6. Analyses of Variance: Standardized Authoritarianism and Democracy Factor Scores by Political Affect and Institutional Support

Affect and Institutional Indicators	Authoritarianism			Democracy		
	1978	1980	1984	1978	1980	1984
<i>Trust in Government</i>						
Never	-.22	-.28	.21	-.66	-.49	-.78
Rarely	-.05	-.02	.04	.09	.06	-.09
Sometimes	.31	.24	-.14	.59	.46	.41
Always	.17	.47	-.21	.42	.57	.55
Eta ²	.04	.06	.02	.24	.18	.25
<i>Government of Many</i>						
Few	-.19	-.20	.10	-.33	-.26	-.46
Many	.30	.36	-.10	.58	.51	.35
Eta ²	.06	.08	.01	.20	.16	.19
<i>Tax Revenues</i>						
Wasted	-.27	.45	.21	-.49	.74	-.55
Partly wasted	.08	.00	-.04	.18	.14	.05
Spent well	.34	-.24	-.24	.75	-.36	.56
Eta ²	.05	.06	.03	.20	.17	.20
<i>Monarchy vs. Republic</i>						
Monarchy	.51	.46	—	.24	.21	—
Indifferent	.03	.03	—	.10	.03	—
Republic	-.53	-.54	—	-.31	-.25	—
Eta ²	.19	.15	—	.06	.03	—
<i>Elections</i>						
Need	-.04	-.05	-.08	.05	.04	.08
No need	.17	.26	.24	-.55	-.57	-.35
Eta ²	.06	.01	.02	.03	.03	.04
<i>Congress</i>						
Need	-.09	-.08	-.28	.06	.08	.13
No need	.14	.14	.33	-.45	-.45	-.22
Eta ²	.01	.01	.10	.04	.05	.04

Note: Description of survey is provided in Note 6.

standing hypothesis that Francoism was a rather nonideological and opportunistic cast of mind or "mentality," not as a full-blown set of political beliefs (Linz, 1964). While the measures of association between the democratic indicators and the Franco factor are significant, and while the form of the relationships is often in a positive direction, the covariance is generally very modest. Many of the Spaniards nostalgic over Francoism do not seem to be unduly exercised about the onset and settling in of democracy, even if their judgment of socialist democracy is on

balance negative. While the two regimes are viewed as distinct, they are not seen as utterly discontinuous, either by Franquists or by Spaniards with warmer feelings toward the new system.

The single area in which Franquist Spaniards show polarizing inclinations is with respect to the monarchy vs. republic issue. At least in the abstract, they tend to be monarchists. Democratically oriented Spaniards are comparatively lukewarm on the question, although they lean toward the republican side. This asymmetry serves to highlight the pivotal role

of the king in providing an assurance of continuity during the transition for a reluctantly democratizing portion of the public (McDonough, Barnes, and López Pina, 1981).¹³

Finally, it is possible to separate out one indicator that has at most a vestigial link with regime legitimacy. This is the item tapping perceptions of governmental spending efficiency. In 1978, both the authoritarian and the democratic factors tied in with positive assessments of the government's role in this area. By 1980, in the depths of the *desencanto*, both turned negative: not even democratically oriented Spaniards looked favorably on the government's budgetary performance. By 1984, the Franquists were also set against the government, now led by the socialists, while the "democrats" adopted a much more supportive attitude.

The flip-flopping of the tax indicator, which is clearly a measure of governmental performance, contrasts with the remaining measures that, in varying ways and degrees, distinguish between the larger political systems. A principal lesson of the analysis of the links between the "old" and "new" indicators is that rigid dichotomies between "legitimacy" and "performance," as well as analogous divisions between "qualitative" and "quantitative" observation, can be artificial. In the first place, the historical proximity of Francoism and democracy tends to overshadow niceties of evaluation based on performance. Second, given the basic demarcation between regimes, the amplification within the democratic system from political to social norms of legitimacy reflects a natural tension and ambiguity in the definition of democracy, rather than merely a preference for this or that set of incumbents. Third, either/or categorization of regimes, like the dichotomy between legitimacy and efficiency, is particularly suspect if applied to political change that happens to be transitional, not revolutionary.

Determinants of Legitimacy

Authoritarianism and democracy are separate regimes, and not just discrete governments, in the eyes of the Spanish public. What remains to be explored is the social bases of the legitimacy of the democratic regime.

Table 7 represents a first attempt to resolve this question. It assembles the correlations, for the three successive surveys, between the series of indicators introduced earlier and the factor scores tapping orientations toward the authoritarian and democratic regimes. The correlations convey a sense of the relative polarization surrounding the political systems. For example, if feelings of religiosity correlate strongly with positive assessments of Francoism, but only weakly with evaluations of the democratic regime, then one possibly crucial determinant of legitimacy from within the Spanish community, religious vs. secular sentiments, can be said to be polarizing for the Franquist system, but less problematic for the new system.

In fact, this turns out to be consistently the case for the religiosity indicator. Starting in 1978, the association between religiosity and Franco is .39, in 1980 .41, and in 1984 .38. By contrast, the corresponding coefficients for the democratic regime are .19, .10, and .06. Again, it is important to note what does not happen. While the correlations of religiosity with Franco are strongly positive (and the left-right/Franco coefficients are even higher), the figures associated with the UCD/PSOE regime are not reversed—that is, they are not strongly negative. Instead, they are simply washed out.

The results are much the same for the other indicators. The Franco regime elicits stronger passions, both positive and negative, than does the democratic system. In this sense Franco was a polarizing figure. But the phenomenon is not purely or even mainly psychological. Spanish authori-

**Table 7. Correlations Between Scores on Democracy and Authoritarianism
Factors and Selected Socio-political Indicators**

Indicators	Authoritarianism			Democracy		
	1978	1980	1984	1978	1980	1984
<i>Left/Right Placement</i>	.60	.60	.57	.18	.13	-.12
<i>Religiosity</i>	.39	.41	.38	.19	.10	.06
<i>Satisfaction:</i>						
With Democracy	.13	.10	-.17	.81	.68	.57
With PSOE Government	—	—	-.26	—	—	.68
With UCD Government	.25	.22	.37	.91	.72	.57
With Franco Government	.94	.80	.85	.21	.16	-.05
<i>Popularity:</i>						
The King	.35	.33	.17	.37	.30	.31
John Paul II	—	.46	.43	—	.17	.09
González	-.10	.01	-.25	.30	.32	.52
Suárez	.32	.39	.14	.48	.42	.33
Carrillo	-.24	-.26	-.34	.15	.20	.18
Iglesias	—	—	-.33	—	—	.19
Fraga	.49	.51	.55	.14	.12	-.05

Note: Information regarding survey is provided in Note 6.

tarianism was demobilizing—that is, strongly exclusionary. Although it was probably not ideological in the fullest sense, it did evoke powerful defensive loyalties from its adherents, as well as hatred from those whom it defeated and resentment among those who had become marginalized by it. Once the constitutional roadblocks were overcome in the early days of the transition, political exclusion ceased to be a problem, and much of the combativeness of Spanish politics was defused, its *raison d'être* having diminished (Maravall, 1984; Medhurst, 1984; cf. Eckstein, 1984).

The correlations between the popularity ratings of selected public figures and the summary evaluations of the authoritarian and democratic regimes are given because these personalities are known to express ideological currents, or *tendances*, in Spain (Barnes, McDonough, and López Pina, 1985). The anchoring roles of the king and of Adolfo Suárez are manifest in the positive correlations of their popularity ratings with both the Franquist and the

democratic dimensions. By contrast, the two most polarizing figures are Santiago Carrillo, ex-leader of the communists, and, on the right, Manuel Fraga, leader of the Alianza Popular. These men define the permissible extremes of the competitive establishment in Spain.

Felipe González, the leader of the socialists, is seen largely as the center-left figure he is. The one polarizing chord struck by González occurs in 1984, in relation to the summary scores for the Franco dimension: the authoritarianism-González correlation is $-.25$. The coming to power of the PSOE, beyond its role as an attractive opposition, aroused the potential for countermobilization on the right—a reaction encouraged by the disintegration of the center-right and substantiated by the increase in the vote for Fraga's right-wing forces in the same election (1982) won by the socialists. The sign of distancing between González and the Franquists matches the shift toward norms of socialist democracy as legitimizing symbols, once the political institution-

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**Table 8. Multiple Classification Analyses:
Authoritarianism (Franco) and Democracy (UCD/PSOE) Factors, 1984**

Authoritarianism Factor Scores as Dependent Variable		Democracy Factor Scores as Dependent Variable	
Predictor	Beta	Predictor	Beta
Religiosity	.30	Political interest	.17
Region	.18	Size of city/town	.15
Political interest	.15	Region	.12
Class identification	.13	Education	.11
Age	.09	Age	.10
Size of city/town	.08	Income	.08
Education	.08	Religiosity	.06
Income	.05	Class identification	.02
$R^2 = .24$		$R^2 = .05$	

Note: Description of survey is provided in Note 6.

alization of the new regime had been secured.

There is one trend, however, that surpasses in importance those tied to individual politicians and other public figures. This is the gradual dissociation of support for the democratic regime from satisfaction with what seems to be the mere effectiveness of democracy.

In 1978, the correlation between positive orientations toward the democratic government and satisfaction with "the way democracy is working in Spain" was .81. In 1980 it dipped to .68, and by 1984 it had reached .57. It is unexceptionable that the two variables are positively related. What matters is that the strength of the correlation has dropped over time. This suggests that the victories and defeats experienced by any single government are less and less likely to touch off feelings of alternate triumphalism and despair—the venerable manic-depressive style of Spanish politics—associated with the political system as a whole, the fate of the nation, and other similarly grandiose stakes.¹⁴

The data just presented, while suggestive, are limited to bivariate correlations, and it is difficult to assess the relative impact of, say, religiosity and class iden-

tification on commitment to and disaffection from the political systems. The evidence on display in Table 8 reduces this problem. It summarizes the results of two multiple classification analyses—essentially, multiple regression in which nominal predictors such as region are permitted—for the factor scores that crystallize orientations toward the Franquist and the democratic regimes.

The most important finding is the vastly greater percentage of variance accounted for in the Franco factor than in the democratic factor by the same set of standard socioeconomic predictors. The Franquist regime was tied more closely to distinctive constituencies—for example, to the culture of Counter-Reformation Catholicism (Alvarez Bolado, 1976; Payne, 1984; Tusell, 1984); to the Castilian zone much more than to regions on the periphery, such as the Basque country and Catalonia (García Ferrando, 1982); to the upper class more than to the workers (López Pina and Aranguren, 1976), and so on. It is the two classic cleavages of Spanish politics—religion and region—that most sharply delineate the mass foundations of the authoritarian state.

What is remarkable is not this confirmation of the exclusiveness of the social

boundaries of the Franquist system, but rather the contrast with post-Franco governments. The constituencies of the democratic regime are much more heterogeneous. It is not region or religiosity that tops the list of predictors, but "political interest" and the "small town/big city," or rural-urban continuum. These variables are not the stuff of confrontation; they are not dividing lines that unequivocally demarcate political identities. Even if they were charged with polarization, political interest and the like do not approach the "primordial" or pre-industrial demarcators such as region in accounting statistically for support for and opposition to the UCD/PSOE governments, in the way that these indicators operate with respect to Francoism.¹⁵

Thus, the bases of support for the democratic regime are much more variegated—broader and more ambiguous—than is the case for the exclusionary system that preceded it. The democratic regime is less strictly tied to particular interests; in this respect it enjoys relative autonomy. While this is not legitimacy or consensus pure and simple, and does not guarantee governability, the depolarization of democracy in Spain constitutes a major achievement of the new political system. The nexus of antagonism is Francoism, now a fading memory (Santamaría, 1985). Democracy in the last quarter of the twentieth century does not pit conservative and progressive Spaniards against one another in the way that democracy did in the 1930s, or as the remnants of Francoism continue to do.

We have seen that Spanish politics is less polarized and more permissive than it was during the days of Franco. The "spread" of the democratic system is wide relative to the sharply delineated constituencies underlying the old system. We have also established that the two regimes, Franco's and the post-Franco system of UCD and PSOE governments, are seen as separate by the Spanish public,

thus opening the way for the analysis of legitimacy instead of mere support/opposition with regard to particular governments.

Heterogeneity is not consensus, however, nor can it be equated with legitimacy. The tolerance implicit in the variegated composition of the constituencies of the new regime—the comparative absence of polarization—is not the same as positive legitimacy, although it may be an important prerequisite of civility. By itself, it does not constitute a political vision.

Detecting the positive elements behind the lack of polarization surrounding the new regime is theoretically as well as empirically problematic, and we can only suggest a route of investigation here. The question matters to the degree that civil comity, pluralism, and the like have not been notable virtues of Hispanic public life, compared with the ideological ferocity and winner-take-all style of the past (Sánchez, 1964; Ullman, 1971).

Our suspicion is that, if there are positive grounds for the legitimacy of the new regime, beyond the rejection of Francoism, they have to do with expectations of a deepening of democracy from political liberalization toward social fairness. Strong hints of such an evolution have already appeared in the expectations surrounding the rise of the socialists, and the ideal of neopopulism is not exceptional in Latin societies with traditionally steep stratification hierarchies, or in societies recently emerging from an agrarian heritage. In Spain, the two legacies merge.

Figure 1 furnishes a bit of data—it is only that—to support this line of reasoning. The dashed curve shows the percentage of Spaniards who feel that workers, the middle class, and the upper class have respectively "done better" over the first two years of the socialist government. It reflects the cynicism of the *los-de-siempre* view—the judgment that the workers have not done particularly well

even under the socialists and that, although no one is perceived as doing especially well, the upper class has done least badly of all.

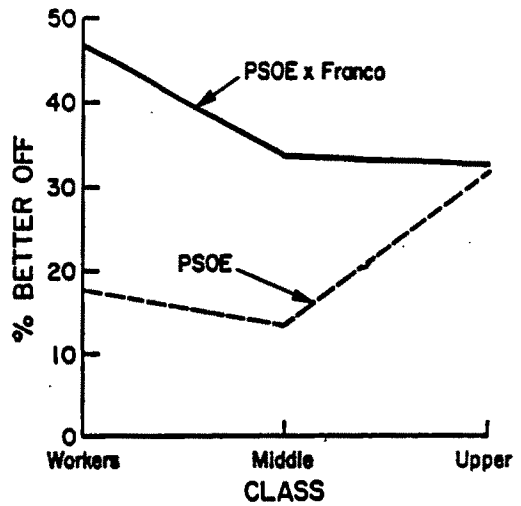
However, as the solid curve shows, when the question becomes how workers, the middle class, and the upper class have done under the PSOE government *compared with how they fared under Franco*, the picture changes substantially. A sharply inverted gradient emerges, with the workers on top, the middle class in the middle, and the upper class at the bottom. Spaniards do indeed distinguish between Francoism and the current political arrangement, and they do so partly in terms of equity considerations.

Another significant facet of the change-over is that it is not zero-sum. The upper class is thought to have done neither better nor worse in the contrast between the PSOE and Franco than it was thought to have done in the contrast within the PSOE. It is the middle class and, especially, the workers who are believed to have done better when the frame of comparison sets the present against the situation under Franco. The variable-sum properties of the interclass comparison reflect the diminished polarization under the new regime.

The lesson is reasonably plain. The Spanish democratic system, and the socialist government in particular, are not immune from substantial criticism. Short-term policy consensus is a poor standard of legitimacy. However, when the point of reference switches from the tribulations of day-to-day democracy to the equity of the system relative to its predecessor, the air of grumbling lifts, and a modicum of satisfaction is discernible. If Spaniards harbor a craving for an alternative to the present political system, that yearning does not imply a resurrection of Francoism.

Spaniards probably have a greater sense than they did under Franco that sacrifices as well as benefits are dis-

Figure 1. Percentages Claiming That "Workers," the "Middle Class," and the "Upper Class" Have Improved Their Situation (a) under the Socialist Government, and (b) under the Socialist Government as Compared with Franco's



tributed more or less equitably. How long this sense of fairness can be maintained in the face of slow or even negative growth—less favorable conditions than prevailed during the rebirth of democracies during the postwar period in Western Europe—remains to be seen. The integration of Spain into the European community and the material advantages associated with this integration seem likely to bolster the democratic regime. In this scenario, political legitimacy would come to depend on, although it might not be reducible to, economic performance (cf. Lane, 1981).¹⁶

Conclusion

The pragmatism of social democracy, its gradual loss of the radicalism and the vision of justice that had inspired it in the beginning, can be seen as a reaction against the excesses and the crimes of authoritarian and dogmatic socialism. This reaction has been salutary; at the same time,

social democracy has been unable to fill the vacuum left by the failure of the great communist hope. Does this mean, as many predict, that the hour of the churches has come? If this should turn out to be the case, I hope that there will be left on the earth at least a small handful of human beings—as at the end of antiquity—who will resist the temptation of divine omniscience, as others, in our day, have resisted that of revolutionary omniscience. (Paz, 1985, p. 191)

The empirical study of political legitimacy has a checkered history. A primary reason for this has been the gulf between ambitious theorizing and the vicissitudes of measurement. The political transition in Spain has eliminated much of this difficulty by providing a rare opportunity to track popular attitudes toward alternative political regimes. Because the Franquist system is still vivid in the minds of most Spaniards, it has been feasible to ask questions about the legitimacy of authoritarianism and democracy that are not only conceptually important but also—and this seems to have been a major challenge in previous studies—meaningful to the man-in-the-street.

We have verified that the two regimes are perceived as separate, above and beyond the specific governments of which they have been composed. The measurement device on which the test was based is a set of items derived from theoretical expectations about the differential bases of legitimacy, and designed to elicit assessments of governments according to their economic, social, and political performance. The multiple indicators evaluating particular governments set up a series of logically distinct analytical outcomes. Only one of these outcomes—that in which the Franco and post-Franco governments were in fact divided into two separate regimes—would substantiate the existence, in the eyes of the Spanish public as well as in those of the theoretical observer, of authoritarian and democratic political systems. This turned out to be the empirical result. We then probed the socio-economic support structure of the

new regime—that is, the demographics of its legitimacy. The natural constituencies of Spanish democracy are broader than the restricted subcultures on which Francoism was built.

The plausibility of our argument, however, depends as much on contextual factors as on statistical elegance. Fixed distinctions between legitimacy and efficiency, and between regimes and governments, turn out to be unhelpful, for three reasons. First, the historical contrast between the opposed political systems of authoritarianism and democracy is so vivid for Spaniards that it would probably remain salient no matter how differences between and within the regime were assessed. The contrast may fade in the future, as Francoism recedes into the past. But for now it is paramount.

Moreover, while the primary transition in Spain has been between authoritarianism and political democracy, the makings of a secondary transition in public opinion—from liberal to social democracy—can also be discerned. The shift is latent. It implies a change in the values by which post-Franco governments are likely to be evaluated, that is, an expansion in the norms of legitimacy. Although tentative, this evolution is visible beneath the movement away from the political restrictions of Francoism.

Third, the change from authoritarianism to democracy in Spain has been genuinely transitional after all, not revolutionary. The discovery of continuities between the regimes is therefore not surprising.

A major feature of the transitional nature of democratization in Spain is that the profile of support for the new regime is not the mirror image of the constituencies of Francoism. The democratic audience is large and diverse. In one sense, this pattern may seem obvious to the point of tautology. After all, in comparison with practically every authoritarian system, industrial democracies are mass democra-

cies.¹⁷ The point, however, is not only that the structural constituencies of democracy expanded with the political opening in Spain and the instantaneous enfranchisement of previously marginalized citizens. It is also the case that long-standing cultural as well as structural demarcators—in particular, religion and regionalism—have eroded as touchstones for mobilization and countermobilization. In this sense, the contours of the legitimacy of the new regime are more diffuse and more heterogeneous than the profile of support for Francoism. Spanish politics has become less polarized and more ambiguous than it used to be.

The variegation in the structural and cultural bases of Spanish democracy is central to an understanding of the political sociology of the post-Franco regime. It also raises questions about the nature of legitimacy under “post-modern” conditions (Anderson, 1984; Martín Santos, 1985; Villalonga, 1984). On the one hand, it is possible to argue that the new regime enjoys considerable autonomy from a narrow constellation of social forces and interests—a flexibility, however, that may be as much a function of the underorganization of secondary associations in Spain, compared to the strength of the state, than of the sheer heterogeneity of the regime’s constituents (McDonough et al., 1984; cf. Useem and Useem, 1979). On the other hand, the multifaceted nature of its support raises the issue of how the regime will hold together, and the low rates of involvement in secondary associations cast doubts on the political socialization of the citizenry. The direction of the regime is further obscured by the timing of the socialists’ ascent to power during a period of disenchantment with the shibboleths of social democracy, yet in the midst of popular expectations regarding economic well being.

The underorganization of Spanish democracy probably reduces the threat of

the country’s becoming ungovernable through a proliferation of interest groups, even if, simultaneously, the constraints imposed by economic hard times reduce the regime’s ability to satisfy such groups. Prolonged economic stagnation may be overrated as an impediment to the consolidation of democracy in Spain. In the least, it is not the only impediment. Of equal importance may be the virtual absence of a normative bond, a political vision, to replace the accustomed “transcendentalism and triumphalism” of Spanish politics. “Principled” confrontation may give way, some analysts fear, to privatization and nihilism (Paniker, 1985; Paramio, 1984, 1985b).

The God-is-dead-Marx-is-dead-and-I-don’t-feel-too-good-myself problem is not unique to Spain. The reformulation of the exhausted or threadbare ideological and material foundations of industrial democracy is also underway in Western Europe and the United States (Cebrián, 1984; Piore and Sabel, 1985). Part of the challenge of Spanish democracy is one of timing: it has been restored more than a generation after its peer nations revived their commitment to democracy. The stock solutions compounded of Keynesianism and party pluralism have been criticized and partially discredited, and a certain demoralization is detectable among those for whom social democracy was linked, in “normal” times, with economic growth (Altares, 1985; Offe, 1984). The increasing complexity of the Spanish electorate and the differentiation of Spanish society that have made political tolerance structurally possible may have rendered difficult the institutionalization of a political vision that can unify such diversity.

Notes

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1. Pluralities in the first two general elections under the democratic dispensation—in 1977 and 1979—were won by the UCD.

2. There is a considerable literature on political legitimacy that we do not utilize, mainly because it is empirically inconclusive and adds little to the formulations by Easton and Lipset. See, however, Hart (1978), Schaar (1981), and Wright (1976).

3. The chronic inconclusiveness of most studies of political legitimacy reflects, we believe, the disparity between elaborateness of theory and crudity of measurement. Furthermore, the fact that many standard indicators of legitimacy are nominal attributes may contribute to a certain petrification of concepts—for example, to mutually exclusive categorizations of values and attitudes that might be rendered more accurately as continua. This tradition may also work to confound “real legitimacy” with nominal classifications that are “fundamental,” “longterm,” “qualitative,” and the like, as compared, invidiously, with scalar, quantitative measures that are assumed to register “efficiency,” “performance evaluations,” et cetera.

4. As with the question of political legitimacy, we omit a review of much of the literature on civil religion and on the autonomy of the state, for two reasons: it is theoretically ambiguous and empirically thin. Since we are not concerned with advancing the study of civil religion or of the autonomy of the state as such, our use of this literature is highly selective. For useful updates of the theoretical literature on the state, see Alford and Friedland (1985) and Benjamin and Elkin (1985).

5. Another factor behind the leeway enjoyed by post-Franco governments, in addition to the social heterogeneity and interclass nature of their bases of support, is itself a relic of Francoism: the continuing low levels of mass participation in voluntary associations (McDonough, Barnes, and López Pina, 1984). The organizational infrastructure for channeling conflicts is comparatively underdeveloped in Spain.

6. The surveys were conducted in June 1973, in December 1979–January 1980, and in November 1984. The number of respondents from the national population 16 years of age and older in each of the surveys is 3004 (1978), 3014 (1980), and 2994 (1984). The first two surveys were based on quota sampling at the individual level, with selection by categories of age and sex. Provinces, and then counties, were selected by probability-proportional-to-size criteria, as were *secciones* (roughly, “polling-booth areas”); then quotas by age and sex were drawn. Respondents for the third survey were drawn from an area probability sample, with random selection of households and individuals within households.

The trust in government, government for the few/many, and tax expenditure items were translated from the by now standard questions originally used in the National Election Studies conducted by the University of Michigan’s Center for Political Studies (Miller, Miller, and Schneider, 1980). The elections question reads: “Do you believe that elections are the best system for choosing the government and the authorities of the country, or do you believe that they are not the best system?” The congress question reads: “Do you believe that we need a congress of deputies and a senate or could we get along without them?” The remaining question reads: “Do you think that Spain should be a monarchy, or that Spain should be a republic?”

7. The question is taken up in detail in the section on “The Phases of Legitimacy.”

8. Except for the popularity items, the questions were introduced in the following manner: “Now I would like to ask you about some things which you might be satisfied or dissatisfied about in our country. As you can see, a 1 on this scale means that you are totally dissatisfied or very displeased, and a 10 means that you are completely satisfied or very pleased.” Then respondents were asked, “Are you totally satisfied or totally dissatisfied with the way in which democracy is working nowadays in Spain?” The interviewer then continued, “Now I would like you to compare, for several things (*en una serie de cosas*), the present government with the previous UCD governments and that of the last ten years of Franco.”

The political evaluation item has provoked controversy among political scientists, even though it does not agitate most Spaniards. The law-and-order slant was chosen, rather than a freedom-and-liberty focus, because on pretesting the former elicited variance, while the latter confirmed only the obvious.

The popularity items were introduced as follows: “We would like to know where you stand (*su postura*) with respect to some prominent figures in Spain today.” The original wording of all questions is available from the authors on request.

9. Franco’s ratings are affected by the demographics of aging. For example, in 1984, the correlation between age and overall satisfaction with the Franco regime is .26. The corresponding figures for the UCD and PSOE governments are, respectively, .15 and .08. Furthermore, it is clear that many younger Spaniards do not have any opinion of Francoism. Of the cohort aged 16–19 years, one-third give no opinion about the Franco regime. For the cohort aged 20–29 years the figure is 15%; for the cohort aged 30–39 years, 14%, and so on, until those 60 years of age and older, for whom non-opinion is only 9%. In short, the steady increase in favorable ratings of Franco can be deceptive unless it is understood that they tend to be concentrated in segments of the population that are dying off. On balance, demographic replacement is working in favor of the democratic regime.

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10. An oblique rather than orthogonal rotation is used. The promax algorithm is a version of oblique rotation, performed after computing a varimax (orthogonal) rotation that gives evidence of correlation between dimensions (Mulaik, 1971, pp. 300-304). Factor analyses of the 1978 and 1980 data, which give essentially the same configuration as that reported here, are presented in McDonough and López Pina (1984).

11. The monarchy-vs.-republic issue is a case apart; it is discussed below.

12. This interpretation does not assume that mass values have shifted in favor of a rigorous socialism since the early days of the transition. No significant further movement toward the left has been detected in the Spanish public. Nor has the public's perception of the PSOE or of Felipe González moved from the extreme left to the center-left; it seems to have been at the center-left for several years, for the most part unaffected by the moderates' and social democrats' internal conquest of the PSOE, and subsequently the government (Gunther, Sani, and Shabad, 1985; Linz, Gómez-Reino, Orizo, and Vila, 1981).

Instead, our interpretation can be boiled down to two hypotheses. First, the timing of PSOE's ascendancy has itself been crucial. Popular ratification of the socialist label at the start of the transition, prior to the consolidation of political democracy, would have been "a provocation." With the passage of time, voting for the socialists became less risky, and in any case the moderate leftism of the PSOE had for several years corresponded to the modal location of the Spanish electorate. Second, while they stand to the left of, for example, the Alianza Popular, and while their position has been on the left since well before the electoral victory of the socialists, the preferences of PSOE supporters with regard to economic and social policy do not constitute a mandate for rapid structural transformation. Rather, they reflect a comparatively uncrystallized package of traditional statist paternalism, bewilderment at tax policy, and upward mobility for "the little guy" (McDonough, Barnes, and López Pina, 1986).

Finally, the disintegration of the center-right UCD into factionalism—in large part because it had no place to go after fulfilling its role in the political transition—contributed to the electability of the already popular center-left socialists (Gunther, 1985). But the socialists did not simply fill a vacuum created by the collapse of the UCD, regardless of ideology; the public has consistently located the PSOE to the left of center.

13. The passion that Franquists bring to the monarchy-vs.-republic issue is probably diagnostic of their attachment to the past, including its controversies. "Asymmetry" results from the tendency of non-Franquists to get more worked up about issues relating to the present and the future (e.g., tax equity).

14. The progressive dissociation between satisfac-

tion with UCD/PSOE governments and satisfaction with democracy may seem to indicate a waning of commitment to the new regime. While the possibility cannot be ruled out, the statistical relation remains very high, so that the indifference interpretation based on these data alone would be alarmist. It is also plain that the connection between satisfaction with democracy and attachment to the Franquist system, never very strong, has become weaker, to the point that by 1984 the association turns negative, corroborating an antipathy between the authoritarian and (social) democratic orientations. The 1978 correlation is .13; in 1980, .10; and in 1984, -.17. This, in turn, suggests that a certain polarization remains in Spanish mass politics and that, given the appropriate conditions, the forces of hypermobilization and countermobilization could return (Sani and Shabad, 1983). Here it suffices to mention the obvious point that disengagement of approval of Francoism from positive evaluations of the new regime substantiates the inference that the Spanish public indeed distinguishes between the two systems, and is not merely accepting of practically any regime—a conclusion that might be drawn if attention were confined to the early period of the transition, when evaluative perceptions of Francoism and democracy were rather strongly correlated.

15. The MCA coefficients alone can be opaque in explaining the direction of relationships. None of the coefficients have signs, since predictors can be nominal attributes. Without going through a predictor-by-predictor account, it may be noted that the relationships work in expected ways. For example, as the beta of .30 indicates, orientations toward the Franquist system vary sharply (and linearly) with religiosity; the corresponding link of religiosity with the new regime is irregular and washed-out (beta = .06). "Region" (of residence) is the only less-than-ordinal predictor. On the whole, Basques and Catalans are the least favorable to Francoism. While it remains important, the center-vs.-periphery conflict is considerably more attenuated for the "democracy factor." For an exposition of the statistical technique, see Andrews, Morgan, Sonquist, and Klem (1973). For a more detailed empirical analysis see McDonough, Barnes, and López Pina (1985).

16. Conventional measures of legitimacy have stressed political trust and participation and the like over imputations of social equity and fairness, with the latter tending to be labeled as performance or policy indicators. The distributive rather than radically redistributive cast to the data examined here raises a question we explore elsewhere: to what extent do populist-paternalistic expectations surround the socialist government and perhaps the democratic regime itself (McDonough et al., 1986)? At least some portion of the distributive cast to Spanish public opinion seems to be traditionally pre-capitalist rather than ideologically anticapitalist.

17. A major exception was Argentina under Perón (Schoultz, 1983).

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TOWARD A POLITICAL EXPLANATION OF GOVERNMENT VOTE LOSSES IN MIDTERM BY-ELECTIONS

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By-elections in parliamentary democracies are remarkable for the almost invariable loss of support experienced by governments relative to the preceding general election. There has been, however, surprisingly little research into this phenomenon. Using Britain as a test case, this article goes some way towards filling this gap. It identifies and tests three political explanations of governments' by-election vote losses, which it labels the turnout, referendum and personal vote models. No simple conclusion emerges, since different results are obtained depending on whether the models are treated as separate entities or their interplay is taken into account by integrating them into a single causal model. This latter strategy is argued to involve a superior conceptualization of the processes at work in bringing about governments' midterm vote losses.

Since the world moved into economic recession in the mid-1970s it has become fashionable to talk of the "ungovernability" of modern democracies. Particular attention has been paid in that debate to the determinants of governments' midterm popularity, since a basic assumption of democracy in practice is that governments are able to carry out their duties and responsibilities more effectively when they enjoy high public esteem: popularity provides a moral and political authority that opponents find difficult to resist. This link has always been apparent in the fragmented

governmental system of the United States, where popular presidents often successfully appeal directly to the public for help in overcoming congressional opposition to their policies (Ostrom and Simon, 1985). Although less obviously so, it is also a link that holds in parliamentary systems characterized by firm party discipline. Because such discipline is not rigid, party leaders find it easier to control their party when they, and by inference their actions and policies, bear the stamp of public approval (Norton, 1975).

Using opinion poll voting intention data, a substantial crossnational literature

has grown up around the question of the economic determinants of short-term fluctuations in the level of government support (good summaries are Paldam, 1981, and Whiteley, 1984; see also Husbands, 1985). An arguably more reliable measure of government popularity, however, is its performance in midterm elections, since this is based on actual rather than hypothetical voting behavior. The best-known such elections are off-year congressional elections in the U.S. presidential system and by-elections in those parliamentary systems where there is no proportional representation electoral system to dictate the automatic filling of a casual vacancy in the legislature by the next person on the list. Yet little is known about the dynamics of government support in this type of election.

An obvious exception to this general observation would be the material on the decline of the president's party in U.S. off-year congressional elections (Campbell, 1966; Hinckley, 1967; Kernell, 1977; and Tufte, 1975). The insights of this literature are not necessarily applicable to parliamentary by-elections, however, since the two types of midterm election lack sufficient similarity to be directly comparable. In particular, off-year congressional elections are national events involving the simultaneous election of all 435 members of the House of Representatives, as well as of 33 senators. They also always carry the possibility of a change of party control in one or both houses of Congress. By-elections, on the other hand, are ad hoc events that rarely involve more than one or two of several hundred constituencies at any one time, while their outcome seldom jeopardizes the government's majority position in the legislature.¹

By-elections can, nonetheless, have very real political consequences. A government's poor performance in them can erode its public credibility, promote disunity in its leadership, and weaken party morale and confidence enough to affect

the timing of elections (Boston, 1980, p. 107; Kay, 1981, p. 37). More seriously, by-election reversals can even cost governments their majority status in Parliament, as happened to the last British Labour government (Butler and Kavanagh, 1981). Consequences like these are the inevitable risk attaching to the tendency for governments almost invariably to lose support in by-elections relative to the preceding general election. A British example will suffice to illustrate this point. Eighty-six percent of the almost 300 by-elections held between 1950 and 1983 registered a drop in the governing party's vote, the mean value of which over all contests is 9.7%. The picture with regard to seat losses is less startling, but governments still lost 42 of them and gained only 3 (see also Butler, 1973, p. 9; Boston, 1980, p. 107; and Scarrow, 1961).

In view of their ever-present potential for making life difficult for the party in power, it is understandable that the question most commonly asked about by-elections is why governments do so badly in them (Boston, 1980; Butler, 1949, 1973; Kay, 1981; King, 1968; Pollock, 1941; Scarrow, 1961; Sparks, 1940; Stray and Silver, 1982, 1983; Taylor and Payne, 1973). What is less understandable is the lack of any serious attempt to formulate and test empirically hypotheses addressing this question. Using Britain as a case study, this article goes some way toward remedying this neglect. Since attitudinal data from the constituencies in which by-elections have taken place are simply not available in the public domain, the analysis of government vote losses undertaken here is necessarily based on aggregate data alone. The undoubtedly important effects of individual perceptions on this aggregate-level phenomenon cannot be assessed until suitable data are available (as, for example, in Kernell, 1977).

The basic strategy of this analysis is to identify three separate political explanations of why governments do so badly in

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midterm elections: the *turnout*, *referendum*, and *personal vote* models. Following existing practice, these models will first be tested individually. It will then be argued, however, that this methodology represents an inadequate conceptualization of the individual and collective effect of these models, since it does not allow for the interplay between them. Integrating them into a single causal model, it will be shown, provides an account of their role in explaining governments' by-election performance that is more sensitive and sophisticated than any of the models affords individually.

By-Elections in Britain

Postwar Britain is perhaps the best available research site for an analysis of the dynamics of government support in by-elections, for three reasons. The first is that the combination of a large House of Commons and a long, five-year parliamentary term means that by-elections are far more numerous there than in, for example, the other Anglo-American parliamentary democracies, where the size of the lower house tends to be smaller and the parliaments of shorter duration (Inter-Parliamentary Union, 1976). In the years between 1919 and 1970, for example, there were 795 by-elections in Britain (Butler, 1973, p. 6), whereas only 94 took place in New Zealand over a period 10 years longer (Boston, 1980, p. 104). With 110 contests between 1901 and 1982, Australia is very similar to New Zealand, while Canada, with 152 contests in the years from 1940 to 1980, scores higher than both of them but still falls far short of Britain (Australian Electoral Office, 1983; Kay, 1981, p. 39).

Britain's second advantage is that its two major parties have spent more or less equal periods of time in office in the postwar period: 21 years for the Conservatives, and 17 for Labour. In consequence, swings against the government cannot be

construed simply as swings against a party perceived as having dominated the national political process for too long. Finally, the political importance of by-elections in Britain has ranged from the slight to the serious. To be sure, most by-elections have represented no immediate threat to the incumbent government, but it is worth bearing in mind that 4 of the 11 governments elected since 1950 enjoyed small majorities. The 1950 government had a majority of five seats, the 1964 one a majority of four; the government of February 1974 was a minority government, and that of October the same year enjoyed a majority of only three seats at its election. The very fact that governments do as badly in by-elections under these conditions as when they have a large majority means that the forces shaping their performance should be particularly apparent in Britain.

This analysis begins with the 1950 general election because it was the first postwar election not characterized by multimember constituencies and plural voting; it ends with the 1983 general election because it is the most recent. A total of 296 by-elections were held in Britain over this period. Excluded from their number, however, are three that were not contested by the government in either the preceding general election or the by-election. This leaves the 293 constituencies that form the data base for this analysis, a full list of which can be found in Mughan (1986, appendix A).

Three Models of Government Vote Losses

Although never tested empirically, there has been informed speculation about why by-elections go against the party in power. This speculation can be distilled into three distinct models, the rationales for which follow.

The Turnout Model

This model is dealt with first because it apparently enjoys the status of conventional wisdom among observers of by-elections. Its focus is voting turnout, since, as with the governing party's vote, this is almost always lower in midterm than in general elections. In Britain, for example, this was the case in all but 15 of the relevant 293 by-election contests. With a range stretching from an increase of 11.4% to a decrease of 39.4%, the drop in turnout averaged 14.7%; it correlates with the change in the government's vote at -0.33 . In other words, the bigger the drop in turnout, the larger the drop in the government's share of the vote.

It is commonplace to assume a causal link between these trends: people who previously voted for the government stay away from the polls in by-elections in disproportionate numbers. Their reason might be dissatisfaction with their party's performance in office, or that they were not strong supporters of the government in the first place and simply cannot be bothered to turn out for an election where the encouragement to vote is weak and the political stakes low (Boston, 1980, pp. 117-22; Campbell, 1966; King, 1968). Whatever its cause, the explanatory importance of the decline in turnout seems self-evident to many, journalists as well as academics; for example, "The feature of the Walthamstow and Brighton by-elections was the abnormally low polls, mainly to be explained by the massive abstentions of Labour voters who have lost faith and hope in the [Labour] Government" (Wood, 1969, p. 1).

Before the turnout model can be adequately specified, however, it must be recognized that the outcome of any by-election will not be a function solely of change in the value of a number of variables, like turnout, from the preceding general election. Some contribution to the changing pattern of party competition

will also likely be made by the cross-sectional value of these same variables at the time of the by-election. One hypothesis, for example, is that high turnout in a by-election will favor the government because it means that "the swing against [it] will be damped down" (King, 1968, p. 415). Thus, both the cross-sectional and change versions of pertinent variables must be included in the specified model if a reliable estimate of their independent effect on the dependent variable is to be obtained. The turnout model, then, is

$$\Delta GPV = a + b_1 T + b_2 \Delta T + e_i, \quad (1)$$

where ΔGPV is the governing party's percentage of the poll in the preceding general election minus its share in the by-election; T is the percentage of by-election turnout; ΔT is the percentage of turnout in the preceding general election minus the same figure in the by-election; and e_i represents the stochastic error. The expectation is that the partial regression coefficient, b_1 , will significantly reduce government vote losses, and b_2 significantly increase them.

The Referendum Model

The second model has its origins in the long-standing and unresolved debate over whether by-election outcomes in single constituencies can be taken as a signal of anything about the nation's assessment of the incumbent government's performance in office. In Britain, this debate was at its height in the throes of the country's democratization, when electoral patterns were still being set. In 1904, for example, Prime Minister Arthur Balfour roundly rejected the view that by-elections are referendums on the national government:

I do not for one instant admit that by-elections are a test, or ought to be regarded as a test, of public feeling. They are, of course, a test of the feelings of a particular constituency at the time the by-election takes place. They are not, and

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they cannot be made, the index and test of what the feeling of the people of the country is as a whole. (quoted in Pollock, 1941, p. 528)

Subsequent empirical studies added weight to this verdict (Pollock, 1941; Sparks, 1940).

Evidence is always questionable, however, and Balfour's interpretation was unanimously accepted neither then nor in more recent times. Assessing the same interwar contests, Herman Finer, for example, confidently declared by-elections to function as "invaluable political referenda on the action of the government" (quoted in Pollock, 1941, p. 519). This interpretation is based on two assumptions that seem hardly contentious in the contemporary British context. The first is that governments will decline in popularity simply by taking and implementing decisions that are not always acceptable to all elements of the support coalition that brought them to office in the first place. The second is that because the British electorate is relatively unmoved by local forces (Stokes, 1967), a government's decline in popularity will manifest itself in by-election outcomes, since these will accurately reflect trends in national public opinion; nor is this an interpretation to be taken lightly. The imputed role of individual by-elections as a referendum on the government's performance is the essential reason for the considerable media coverage of them, even when their outcome does not threaten the distribution of parliamentary power. It is also one reason why governments dislike doing badly in them, even if their public posture is to shrug off poor performances as routine, midterm reversals.

But before the referendum model can be tested, it must be noted that a full specification of this model must take into account that governments do not operate in a political vacuum; there are always other parties waiting in the wings to take up the reins of office. In an adversarial polity

like Britain, the government's loss of popularity might automatically be thought to be the opposition's gain. If this were the case, there would be no need to ascertain whether changes in the opposition party's national standing affect government vote losses, since the relationship would be the exact inverse of the governing party's. Things are not so simple, however, since support for the government or support for the opposition are not the only alternatives open to electors, even in perfect two-party systems. Another is for electors not to express a voting preference at all. In addition to the matching measures for the governing party, therefore, a complete specification of the referendum model dictates the incorporation of cross-sectional and change measures of support for the opposition party and of the proportion of the electorate not knowing which party to support.² The model thus takes the following form:

$$\begin{aligned}\Delta GPV = & a + b_3GA + b_4\Delta GA \\ & + b_5OVI + b_6\Delta OVI + b_7DK \\ & + b_8\Delta DK + e_i,\end{aligned}\quad (2)$$

where GA is the proportion of Gallup poll respondents expressing approval of the government's record in the month of the by-election; ΔGA is the change in this proportionate figure from general to by-election; OVI is the proportion of respondents intending to vote for the major opposition party; ΔOVI is the change in this figure between the two types of election; DK is the proportion volunteering a "don't know" voting intention; and ΔDK is the change in this proportion.³

It is expected that the more popular the government is at by-election time (b_3), the lower will be its vote losses; conversely, the more popular the opposition (b_5) is, the higher will be these losses. As for the change measures on these two variables, the direction of their impact will depend

Table 1. Variables, Scoring, and Means

Variables	Scoring	Means
<i>Control Variables</i>		
Partisanship	10.9% to 21.0%	18.07
Δ Partisanship	6.7% to -4.9%	0.34
Country	1 = Scotland/Wales, 0 = England	0.18
Season	1 = Summer/Spring, 0 = Winter/Autumn	0.43
Register age	0.5 to 16 months	9.45
Δ Register age	-12.0 to 10.5 months	-0.30
Electoral marginality	0.20% to 73.0%	22.34
Δ Electoral marginality	58.3% to -36.3%	1.81
Parliamentary marginality	-33 to 102 seats	46.85
Δ Parliamentary marginality	30 to -2 seats	4.21
Number of by-elections	1 to 6	1.90
Timing of by-elections	1 = within 6 months of a general election, 0 = not within 6 months	0.18
Number of candidates	2 to 16	3.79
Δ Candidate number	3 to -11	-1.10
<i>Turnout Model</i>		
Turnout	24.9% to 85.4%	61.21
Δ Turnout	39.4% to -11.4%	14.71
<i>Personal Vote Model</i>		
Δ Constituency service	40.3 to 0 years	7.89
Δ Privy Council membership	25.8 to 0 years	3.67
<i>Referendum Model</i>		
Government approval	18.0% to 60.0%	39.27
Δ Government approval	35.0% to -22.0%	6.65
Opposition voting intention	25.0% to 55.0%	45.60
Δ Opposition voting intention	16.0% to -19.0%	-1.72
Don't know voting intention	5.0% to 25.0%	12.35
Δ Don't know voting intention	8.0% to -12.0%	-1.89

Note: The symbol Δ denotes change, and the change measures are arrived at by subtracting the by-election score from the preceding general election score.

on the direction of their change. Table 1 shows governments to become on average more unpopular between general and by-elections (b_4), and the larger the drop in their support, the larger should be their vote losses. The opposition, in contrast, gains support, so that the change in its popularity (b_5) can also be expected to be positively related to the dependent variable. Finally, on the assumption that governments are the principal midterm reference points for voters, the higher the proportion of voters volunteering a "don't know" response (b_7) and the larger the increase in this figure (b_8), the greater should be the drop in the government's share of the vote.

The Personal Vote Model

The final model to be tested shifts the focus of attention from national party politics to what might loosely be called personality politics. By-elections are caused by the death or resignation of individuals who have been the member of Parliament (M.P.) for their constituency for some years. In the particular context of Britain in the period from 1950 to 1983, the length of such service for departing government M.P.s ranges from less than 1 year to over 40 years, and averages a little more than 10 years. The point of this observation is that evidence is now accumulating that M.P.s are able to build

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up a "personal vote" as a direct result of the public visibility and opportunities for constituency service that tenure of office gives them (Cain, Ferejohn, and Fiorina, 1984; Williams, 1966-67). One advantage of a personal following is that it provides the individual M.P. with some insulation against the worst effects of downturns in the popularity of his party. His departure from office, however, should mean the loss of this insulation for the by-election candidate nominated to replace him, and lead to an intensification of the vote swing against the government.

Nonpartisan service to both constituents and constituency is the commonly recognized basis of the personal vote (Cain et al., 1984, p. 113). But this unidimensional conception is incomplete in parliamentary systems in which the executive and legislative branches of government are fused. This is because parliamentarians can be members of both branches, and may consequently develop a personal following through their service to the nation instead of, or as well as, to their constituency. The most salient badge of national political prominence is the title *Right Honorable* that membership in the Privy Council brings, and there is no reason why the visibility and status accruing to those so honored should not be translated into a nonpartisan support base among constituents proud of their representative's achievements. This being the case, the longer the departing government M.P. has enjoyed this prominence, the greater should be the vote swing against his party in the by-election.⁴

No governing party candidate had previously represented the by-election constituency in the Commons, which means that none of them had had the opportunity to build up a personal following there. The cross-sectional personal vote variables are therefore redundant, and the model to be tested is

$$\Delta GPV = .a + b_9 \Delta CS + b_{10} \Delta PC + e_i, \quad (3)$$

where ΔCS is the number of years the outgoing governing party member had served as parliamentary representative of the constituency, and ΔPC is the number of years the same individual had been a member of the Privy Council. Both variables are expected to be positively related to the size of the governing party's vote losses.

Testing Models of Government Vote Losses

These models, then, represent common explanations of the by-election performance of governments in the short term. Voting patterns in both general and by-elections, however, are also prey to two other sets of forces whose influence has to be controlled if the turnout, referendum, and personal vote models are to be tested properly. These are, first, long-term forces whose effect is more or less stable across elections, and, second, contextual forces specific to the by-election contest.

When taken together, these long-term and contextual forces are numerous; and reasons of space dictate that the theoretical grounds for their inclusion as control variables not be spelled out in detail here. This detail is given in Mughan (1986) and a summary identification of their measurement and values can be found in Table 1. To start with the long-term electoral forces, the first of them is aggregate partisanship, by which is meant the degree to which individuals' party preferences can be predicted from a knowledge of their sociodemographic characteristics. Following Powell (1980), the degree of partisanship in the electorate is measured through election-by-election estimates of the variance explained in party choice by an unchanging set of social structural variables (Rose and McAllister, 1986, p. 92).⁵ Then comes the country—England on the one hand, Scotland and Wales on the other—in which the by-election is held (Mughan, 1986). The season in which elections take place is

also often held to influence voting patterns (Alderman and Cross, 1975, p. 388; King, 1968, p. 415).

The contextual variables are more numerous. The first of them is marginality, and it takes two forms: electoral and parliamentary. Electoral marginality is best known for its positive effect on turnout (Denver and Hands, 1974), but it also needs to be controlled in case the swing against the government is, other things being equal, less marked in more marginal constituencies. As for parliamentary marginality, it would seem reasonable to hypothesize that the smaller the government's overall majority in Parliament, the greater will be its efforts to dampen, or even reverse, its vote losses in by-elections. The timing of by-elections has a threefold potential to affect their outcome. In the first place, the larger the number of by-elections scheduled on the same day, the higher their stimulus and turnout levels are likely to be, and the lower the vote swing against the government. Second, the closer they are held to a general election, the lower the government's vote losses (Taylor and Payne, 1973, pp. 345-49). Third, if turnout does indeed affect the size of the government's by-election vote, then the age of the electoral register should be controlled, since deaths, removals, and so on mean that turnout gets lower as the register ages. Finally, account must be taken that minor parties, especially the Liberals, have shown themselves more likely to contest by-elections than general elections. The larger number of candidates in the former, therefore, is likely to accentuate government's vote losses by providing an alternative for voters who might otherwise have supported the government.

This array of electoral forces will act as control variables when each of the models—turnout, referendum, and personal vote—is tested for its ability to explain governments' by-election vote losses. The analysis relies on ordinary least squares

regression methods, which assume that variables are, to a reasonable approximation, linear and additive (Hanushek and Jackson, 1977).⁶ Missing data, being very rarely a problem, are treated by pairwise deletion. Since a directional effect has been hypothesized for each variable in the models, one-tailed tests of significance will be reported.

Table 2 presents the regression results for each model.⁷ The immediately striking feature of the table is its clear indication that only the referendum model has an unambiguous role to play in the explanation of governmental performance in by-elections. The other two models produce disappointing results at best. The worst performance comes from the personal vote model, in the sense that the table provides no evidence that the loss of a long-serving elected representative has any direct effect on the pattern of constituency vote losses for governments. Moreover, this observation holds whether or not this representative also enjoyed national political prominence. Thus, the evidence would seem to vindicate the traditional supposition in British politics that "electors vote for parties and not for candidates" (Jennings, 1961, pp. 260-61). It may be, of course, that this disregard for candidates is less pronounced in the contemporary era of partisan dealignment, but in the longer term it would appear that any personal following that departing governing party candidates may have had is not sufficiently strong to have made itself felt over the generalized anti-government swing characteristic of by-elections.

The explanatory status of the turnout model is a little more ambiguous, since the effect of change in turnout on the dependent variable is significant at the .05 level only. The relationship is certainly not of the strength that would have been predicted, in view of the relative ease with which relationships of this level of significance are achieved with aggregate

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Table 2. The Effects of the Turnout, Personal Vote, and Referendum Models on Governments' By-Election Vote Losses

Variables	Turnout Model			Personal Vote Model			Referendum Model		
	<i>b</i>	beta	<i>t</i> statistic	<i>b</i>	beta	<i>t</i> statistic	<i>b</i>	beta	<i>t</i> statistic
<i>Control</i>									
Partisanship	-0.36	-0.10	1.69*	-0.34	-0.10	1.60	0.55	0.16	1.72*
Δ Partisanship	0.13	0.04	0.64	0.16	0.05	0.80	0.23	0.07	0.85
Country	0.76	0.03	0.74	1.21	0.05	1.21	2.59	0.11	2.85**
Season	2.05	0.12	2.39**	2.20	0.13	2.55**	0.45	0.03	0.55
Δ Register age	-0.12	-0.06	1.24	-0.10	-0.05	1.00	0.05	0.02	0.50
Electoral marginality	-0.02	-0.03	0.50	0.01	0.02	0.31	0.00	0.00	0.00
Δ Electoral marginality	0.10	0.18	3.26**	0.09	0.16	2.87**	0.11	0.19	3.86**
Parliamentary marginality	0.10	0.43	7.38***	0.11	0.44	7.44***	0.08	0.35	4.74***
Δ Parliamentary marginality	0.12	0.10	1.54	0.13	0.10	1.58	0.10	0.08	1.28
Number of by-elections	0.05	0.01	0.13	0.00	0.00	0.00	0.27	0.04	0.88
Timing of by-elections	-2.35	-0.10	2.34**	-2.17	-0.10	2.16*	-1.19	-0.05	1.26
Δ Candidate number	1.66	0.31	6.65***	1.71	0.32	6.88***	1.38	0.26	5.73***
<i>Turnout</i>									
Δ Turnout	-0.10	-0.11	1.96*	—	—	—	—	—	—
<i>Referendum</i>									
Government approval	—	—	—	—	—	—	-0.58	-0.66	7.26***
Δ Government approval	—	—	—	—	—	—	0.16	0.24	2.08*
Δ Opposition voting intention	—	—	—	—	—	—	0.01	0.01	0.09
Don't know voting intention	—	—	—	—	—	—	0.36	0.14	1.97*
Δ Don't know voting intention	—	—	—	—	—	—	0.25	0.10	1.42
<i>Personal Vote</i>									
Δ Constituency service	—	—	—	0.07	0.08	1.47	—	—	—
Δ Privy Council membership	—	—	—	-0.03	-0.02	0.32	—	—	—
Intercept	16.4			11.5			18.8		
Adjusted R ²	46.4			45.9			59.1		

Note: The symbol Δ denotes change. Dashes indicate variables not figuring in the model.

**p* ≤ .05 (one-tailed test).

***p* ≤ .01 (one-tailed test).

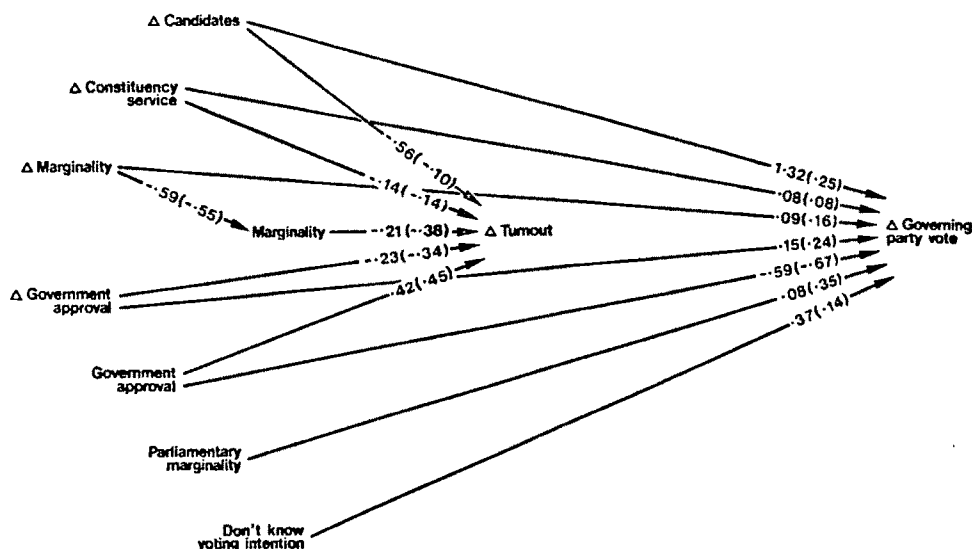
****p* ≤ .001 (one-tailed test).

data, and of turnout's bivariate correlation of -0.33 with change in the governing party's vote, and of the prominent explanatory role commonly attributed to it by observers of by-elections. Under these circumstances, the question inevitably arises as to whether it can be considered a "real" relationship.

Two features of Table 2 suggest that the best way out of this interpretational dilemma is to reconceptualize the basic relationship rather than to opt for some statistical determinant of substantive importance that is itself somewhat arbitrary anyway: First is the fact that a number of background variables signifi-

cantly moderate the relationship of turnout to vote, which suggests that the relationship itself may be spurious; second is the impact of the cross-sectional and change versions of the government approval variable. If these aspects of the government's standing in the polls have a substantial impact on the change in its vote, there is no immediately apparent reason why they should not have a similar influence on turnout change. After all, if a government is unpopular nationally, its supporters are at least as likely to "vote with their feet" and abstain as they are to vote for another party. Again, therefore, the relationship of turnout to vote could

Figure 1. A Causal Model of Governing Parties' By-Election Vote Losses



Note: The model shows metric partial regression coefficients (*bs*) and standardized regression coefficients (*betas*) in parentheses. Only paths at $p < .05$ (one-tailed test) are shown. The model also controls for all the variables shown in Table 2 and not figuring herein. See Table 1 for the definition of the variables.

be spurious, and the basic model needs to be respecified to take account of this possibility.

Figure 1 depicts a path model of government by-election vote losses. Its advantage over the previous treatment of the three models separately is that it allows turnout change to function as both an independent and a dependent variable. That is, it admits of the possibility that while turnout change may influence vote losses, it in turn may be influenced by the government approval and personal vote models, with the result that the direct relationship between turnout and the vote becomes spurious. In testing this possibility, a number of steps have been taken to simplify the model in Figure 1. First, the long-term forces of partisanship, country, and season were included in the estimation of the model, but are excluded from its presentation. Second, the model pre-

sents only the election-specific variables that either directly influence the change in the government's vote or influence the change in both turnout and the vote. Variables affecting turnout change alone are excluded, because their failure to influence the vote loss variable either directly or indirectly means they are irrelevant to the primary concern of this analysis. Finally, even though the change variables will inevitably influence some of the cross-sectional ones, both are treated as background variables occupying the same position in the causal sequence of the model as a whole. Their interrelationship is irrelevant to the question of change in the governing party's vote.

Bearing in mind the previous discussion of the separate models, the main feature of Figure 1 is its striking confirmation that the bivariate relationship between inter-election change in turnout and in the gov-

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erning party's vote is indeed spurious. Once variables comprising the government approval and personal vote models are conceptualized as "causing" turnout change, this relationship disappears. Its nonexistence is clearly illustrated in its partial regression coefficient of -0.01 , its standard error of 0.05 , and its t statistic of 0.21 . The four variables responsible for the spurious relationship are government approval, change in the level of government approval, change in the number of candidates, and change in the length of the governing party candidate's constituency service. All four are related in one direction to turnout change and in the other to change in the government's vote share. The effect is to produce the original, negatively signed bivariate relationship.

Further inspection of Figure 1 shows that the path model is heretical not only for its denial of the commonly assumed importance of the turnout model, but also for its affirmation of the commonly dismissed importance of the personal vote model, or at least its constituency service aspect. The effect of controlling for the nation's assessment of the governing party's record in office is to allow the personal following of its departing M.P.s to surface as a significant factor in the explanation of their former party's by-election misfortunes. As would be expected in a party-dominated British electorate, of course, the effect of this variable is not strong; measured by length of constituency service, it has in fact the weakest impact of all the predictors in the model on the vote change variable, and the second weakest impact on turnout change. Nonetheless, its effect is undeniable.

The limited contribution of the departure of its sitting M.P. to the governing party's by-election misfortunes falls into perspective when compared with the structuring effect of the referendum model. In terms of its expected explana-

tory importance, this model enjoys mixed fortunes. On the debit side, there is the absence of the hypothesized stimulant effect for opposition party support. On the credit side, there is the overwhelming preeminence of the cross-sectional government approval variable; its (depressant) effect on government vote losses is almost twice as great as that of any other single predictor, and more than eight times as great as that of the constituency service variable. Each of these themes will now be elaborated.

A common characterization of the British political process is that it is an adversarial one with government and opposition preying on each other's misfortunes, and locked into a continuous struggle to convince the electorate of the superiority of the claim of each to the right to govern. With a disciplined opposition always sniping at and harassing an equally disciplined government, this dialectic obviously structures the British parliamentary process, but Figure 1 indicates that it exercises no comparable effect on the behavior of the electorate. Be it measured in change or cross-sectional terms, voting intention for the major party in opposition (or, indeed, for the Liberal party separately) simply does not affect the governing party's by-election performance.

The reason for its irrelevance at the electoral level is that the major party in opposition shares with the Liberal party in particular the role of beneficiary of popular discontent with the government. Indeed, it finds itself subordinated to the Liberal party in this role, as is evident from the correlation of government voting intention with opposition voting intention at -0.29 , and with Liberal voting intention at -0.48 . The corresponding correlations for the change measures on these variables are 0.33 and 0.43 respectively. It is only when voting intention for the two parties is aggregated to form a single measure of opposition popularity that such an impact manifests

itself in a reestimated version of the path model in Figure 1.⁸

The fragmentation of the opposition at the electoral level leads logically to the most salient feature of Figure 1, which is its demonstration that governments, to the extent they are able to shape the public's evaluation of their record in office, are in considerable control of their own fate in by-elections. By far the most potent predictor of government performance is precisely the proportion of the electorate satisfied with this record in the month of the by-election. This satisfaction may be based on the country's macroeconomic performance, the popularity of the Prime Minister, or any number of factors; the important point is the higher it is, the lower the governing party's vote losses in the by-election.⁹ Moreover, the less that the proportion satisfied has dropped relative to the figure prevailing when it took office, the lower its vote losses as well.

These relationships are all the more robust because the model has taken account of the governing party's standing in the House of Commons. The strong effect for the parliamentary marginality variable in Figure 1 shows that the by-election vote swing is more pronounced against governments with large majorities, regardless of how popular or unpopular they may be with the electorate. This suggests that disgruntled supporters are less reluctant to desert their party when it has a safe majority in Parliament, since desertion entails no risk of its losing office. When, in contrast, government majorities are small, supporters are more likely to remain loyal in by-elections, so as not to jeopardize their party's control of government. This observation does not, however, gainsay the overriding explanatory importance of the public's satisfaction with the government's performance in office. The simple fact of the matter is that government vote losses in by-elections are primarily a function of the

pattern of such satisfaction at the time the by-election is held. The parliamentary situation may mediate this relationship, but it does not overshadow it.

Conclusion

In highlighting the reasons for government vote losses in by-elections relative to the preceding general election—a matter at best imperfectly understood in the past—this analysis has served to illustrate sharply the need to put conventional wisdom to the empirical test. Turnout change, the factor that has been commonly accepted by academics, journalists, and party activists alike to be responsible in large part for these losses, has been shown in fact to be no more than spuriously related to them. Moreover, even at the bivariate level, the relationship between turnout change and vote change runs counter to that posited by proponents of its importance. Ironically, what turns out to have greater value is the erstwhile incumbent's personal vote, based on constituency service—a factor that has probably been as widely dismissed as turnout change has been embraced. The longer the departing government M.P. has represented the constituency, the worse will be his party's midterm performance.

Pride of explanatory place, however, goes to the variable whose actual role in bringing about a drop in the government's by-election vote has been subject to the most disagreement and liveliest debate, namely, the government's national standing. Moreover, by far the stronger effect is exercised by the level of such standing in the month of the by-election, and not by the extent of the change in it since the last general election. In other words, what determines how well governments do in by-elections is less the change in their popularity from the time of their election than how high they stand in national public opinion at the time of the by-election. It follows that by-elections can

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quite legitimately be interpreted as national referendums on government performance. Indeed, what emphasizes this aspect of their character is the fact that national support for the major opposition party or for the Liberal party separately plays no role in determining the size of the swing against the government. Insofar as the electorate reacts principally to the current standing of the government and disregards stasis and change in the popularity of the other major parties offering themselves as a potential alternative government, by-election voting patterns would seem to represent an investment in the present rather than in the future.

A word of caution is nevertheless in order. It must be stressed that the role of by-elections as barometers of national public opinion about the government does not mean that their outcome can be taken as a faithful indicator of how that government would do in a general election, even if it were held the same day. This is because approval of its record in office will not always mean a vote for the government, and disapproval a vote against it. In both cases, a critical threshold will have to be crossed before government supporters desert their party or opposition supporters defect to it (Hudson, 1985; Kernell and Hibbs, 1981). It also stands to reason that if by-election outcomes are poor predictors of proximate general elections, they will be even worse predictors of more distant ones. Thus, compounding the problem of the discrepancy between approval and vote is the impossibility of predicting how popular governments will be when the next general election is called. The normal pattern is for satisfaction to be high when governments are first elected, for a dip in midterm and for a recovery in the run-up to the next general election. But there remains considerable variability within this broad pattern. Some governments (e.g., the 1966-70 Labour one) end up being less popular than at the outset

despite this recovery, whereas others (e.g., the 1979-83 Conservative one) finish up being more popular. In short, by-elections may be useful barometers of the nation's evaluation of the incumbent government at the time they are fought, but they ought not to be used to predict general election outcomes in the near or distant future.

Notes

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1. The inappropriateness of these comparisons emphasizes the point that the true U.S. equivalent to the parliamentary by-election is not the off-year congressional election, but the "special election" to the House of Representatives. As with by-elections, however, this type of election remains *terra incognita* (Sigelman, 1981).

2. Another option, of course, is to express an intention to vote for a minor party. In Britain, the only such party for which polling data are available is the Liberal party; its role in the explanation of government vote losses in by-elections will be dealt with later in this article.

3. The Gallup poll questions and monthly figures are taken from Webb and Wybrow (1981, 1982). The 1982 and 1983 figures were given to me in private correspondence by Bob Wybrow of the Gallup Organization.

4. The number of years served as a member of Parliament or of the Privy Council is not an altogether satisfactory measure of the personal standing of M.P.s., since it does not allow for differences in such individual characteristics as vivacity, assiduity in "nursing" the constituency, and general devotion to the job of being an M.P. Nor does it allow for differences in the public prominence of the positions of responsibility held by members of the Privy Council. It might be pointed out, however, that the very blandness of the measures used means that the personal following of the genuinely popular M.P. will be underestimated.

5. Since Rose and McAllister (1986, p. 92) provide partisanship figures for 1959 to 1983 only, the equivalent figures for the general elections in 1950, 1951, and 1955 are necessarily estimates. These were derived by equating Rose and McAllister's 1959 figures with Alford's (1963, p. 103) index of class voting value for that same year. The result of dividing the former by the latter is a figure of 0.53%. In

other words, one point on Alford's index for 1959 was the equivalent of 0.53% of the variance explained by social structure in the same year. Assuming a stable relationship between the two measures over the course of the 1950s, Alford's 1950, 1951, and 1955 index values were then multiplied by 0.53 to give the general election partisanship values for these years. Their by-election counterparts were taken to be a linear function of the change in partisanship's value between the general elections immediately preceding and succeeding a by-election. Thus, if partisanship increases by a value of, say, 2% between two general elections, and a general election is held halfway between them, then the by-election partisanship score will be the variable's value at the preceding general election plus 1%.

6. In the exploratory data analysis, a turnout change quadratic term was included in the regression equation to determine whether there was a significant element of curvilinearity in this variable's relationship to the dependent variable. There proved to be none at all, so the quadratic term was not used subsequently.

7. Missing from Table 2 are the cross-sectional versions of the register age, candidate number, turnout, and opposition voting intention variables. These were excluded from the analysis because each is correlated at an unacceptable level (-0.80 , -0.92 , -0.78 , and -0.80 , respectively) with its corresponding change measure (Hanushak and Jackson, 1977, pp. 87-88). To avoid the unstable estimates that usually result from such high levels of collinearity, the cross-sectional measures were excluded from further consideration, on the grounds that change, and especially change in turnout, plays a more central explanatory role in the models to be tested. It might be noted that including the cross-sectional measures instead hardly alters the pattern of influences on government vote losses depicted in Figure 1. Details are available from the author on request.

8. The path model had to be reestimated to ascertain the effect of the aggregated opposition variable, because the Liberal party contested only 179 constituencies in both the general and the by-election. The reestimated model is not presented in the text partly because it departs little from Figure 1, and partly for reasons of space. Details are available from the author on request.

9. In the exploratory analyses, lags of up to three months on the independent voting intention and government approval variables were tried. The best predictions, however, came from the scores on these variables in the month of the by-election.

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TRAPPING THE PRINCE: MACHIAVELLI AND THE POLITICS OF DECEPTION

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Machiavelli's most famous political work, *The Prince*, was a masterful act of political deception. I argue that Machiavelli's intention was a republican one: to undo Lorenzo de Medici by giving him advice that would jeopardize his power, hasten his overthrow, and allow for the resurgence of the Florentine republic. This interpretation returns *The Prince* to its specific historical context. It considers Machiavelli's advice to Lorenzo on where to reside, how to behave, and whom to arm in light of the political reality of sixteenth-century Florence. Evidence external to *The Prince*, including Machiavelli's other writings and his own political biography, confirms his anti-Medicean sentiments, his republican convictions, and his proclivity for deception. Understanding *The Prince* as an act of political deception continues a tradition of reading Machiavelli as a radical republican. Moreover, it overcomes the difficulties of previous republican interpretations, and provides new insight into the strategic perspective and Renaissance artistry Machiavelli employed as a theoretician.

[F]or some time I have never said what I believed and never believed what I said, and if I do sometimes happen to say what I think, I always hide it among so many lies that it is hard to recover.

—Machiavelli

Realism is generally considered a necessary first move in the effort to make the study of politics scientific, and when political scientists turn to the history of ideas, they tend to acknowledge Machiavelli as the champion of realism, and *The Prince*, in particular, as the first treatise in political thought to infuse the contemplation of political affairs with a spirit of empiricism, *realpolitik*, and *raison d'état*. By

now it has become commonplace for political scientists in fields as diverse as international relations, comparative politics, organization theory, and political psychology to construct explanations about political life and political conduct that rely in part on the "axioms" that evolved from Machiavelli's little treatise. These include the necessities of "naked self-interest," the maintenance of rulership at all costs, the utility of unethical and manipulative behavior, and the centrality of power as an end in and of itself.¹ Accordingly, in many areas of political inquiry Machiavelli has come to be regarded as the theorist of "Machiavellianism," and Machiavellianism itself en-

tails understanding politics primarily in terms of who dominates whom and how successfully.

As far as it goes, this understanding of politics as power is true to Machiavelli's purposes in *The Prince*, but, in the end, I think it does not go far enough. What is missing from the political scientists' rendition of Machiavelli is another vision of politics, a republican one rooted in love of liberty and respect for self-governance, which political theorists have long considered as vital to Machiavelli's thought as power and *realpolitik*.² This second vision of politics as participation is one theorists tend to associate with Machiavelli's later works, particularly *The Discourses* and *The History of Florence*. Thus, like political scientists, they too often read *The Prince* in terms of the politics of power and domination.

I suggest that neither political scientists nor political theorists have realized the full force of the Florentine's intentions in *The Prince*. By offering a different perspective on the treatise I hope to show that both groups have underestimated Machiavelli, and in ironic ways—the theorists by not seeing the republican goals that guide the treatise, the political scientists by not recognizing the full force of the "Machiavellian" intentions that inform it. My purpose, then, is not only to present a new interpretation of this most famous of Machiavelli's works, but also to encourage a reconsideration of the adjective that bears his name and the vision of politics it represents.

Republicans and *The Prince*

No political thinker was more aware of how crafty assault by deceit could serve as a substitute for brute assault by violence than Niccolò Machiavelli. The theme of deception weaves through all of his work—his drama, his military writings, his history, his political theory. *Mandragola* is a tale of crafty assault

practiced by the wily Ligurio, who helps a young rake bed the beautiful wife of a pompous old doctor. *The Art of War* argues that a commander who vanquishes an enemy by stratagem is as praiseworthy as one who gains victory by force. *The History of Florence* tells the story of a city where deceit and guile secure power, while honesty and blind trust ruin it. Nowhere, of course, is Machiavelli's love for the art of deception more vividly unmasked than in *The Prince*. There the subject of crafty assault takes its notorious form in his advice to a ruler on how to play the fox, "confuse men's brains," and employ cunning in the political world. In short, whether the subject is love, war, or politics, Machiavelli recognizes the advantages of crafty assault in any form, be it trickery, stratagem, or artifice.

For those who believe that Machiavelli was a republican and a Florentine patriot, this view of him as the master theorist of deceit poses difficulties. As Hanna Pitkin (1984) reminds us, foxes make poor citizens—their deceit undermines civic *virtù*—and *The Prince* abounds with foxes and advice on deception. Furthermore, in Machiavelli's infamous tract to Lorenzo de Medici we find no defense of the Florentine republic, no call for popular liberty, no praises of republican Rome. Far from denouncing tyranny, as would any bold republican, Machiavelli appears to content himself with forging the absolute and ruthless power of an autocrat. How, then, is it possible to hail him as a defender of liberty, self-government, and civic *virtù*, when these appear to be the very values he teaches his Medici protégé to subvert?

Proponents of the thesis that Machiavelli was a republican, despite his authorship of *The Prince*, fall into two camps. According to the "weak republican" thesis, *The Prince* is an aberration. Despairing of the future of Florence, much less its republican government, Machiavelli saw the Medici as the only

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alternative to total chaos, and so wrote his advice book in reaction to an impending crisis (Baron, 1961; Hale, 1961; Pocock, 1975). This thesis maintains that after 1513 Machiavelli simply abandoned the idea of the prince as a "political innovator"; he renewed his commitment to republicanism, developed an admiration for antiquity, and refueled his antipathy for the Medici. The result of this renewal was *The Discourses*, complete with its dedication to republican sympathizers and its repudiation of the entire genre of princely advice books. Thus, these scholars view *The Prince* as a tract that reflects both Machiavelli's acceptance (reluctant though it might have been at the time) of Medicean domination and his clear-sighted, if opportunistic, attempt to ingratiate himself with the new rulers of Florence.

Among other things, the weak republican thesis strains credulity. To read *The Prince* as a sudden capitulation to Medici rule, or as a tool to curry favor is, arguably, to underestimate Machiavelli as a citizen and a theorist. Though I find Garrett Mattingly's (1958) ultimate assessment of *The Prince* unconvincing (for reasons I will explain shortly), I think he poses in dramatic terms the correct riposte to the weak republican thesis:

I suppose it is possible to imagine that a man who has seen his country enslaved, his life's work wrecked and his own career with it, and has, for good measure, been tortured within an inch of his life, should thereupon go home and write a book intended to teach his enemies the proper way to maintain themselves. . . . But it is a little difficult for the ordinary mind to encompass. (1958, p. 485)

The other camp advances a "strong republican" thesis, arguing that even as Machiavelli writes *The Prince*, he remains a defender of republican liberty and an opponent of the Medici (Gentili, 1924; Mattingly, 1958; Rousseau, 1978; Spinoza, 1945; Wolin, 1960). However, if this is indeed the case, and if republican

sympathies abound in *The Prince*, then something else must be moving beneath the surface of the text; some drama that the protagonist-prince does not see must be taking shape. This is precisely what the strong republican camp wants to argue, but as is so often the case with Machiavelli, there are differences of opinion on the matter of the subtext of *The Prince*. Three main views emerge.

The first is Rousseau's (1978, p. 88) claim that *The Prince* is a book for republicans. Rousseau argues that the advice book was not intended for the Medici at all, but rather to expose to the people the brutal ruthlessness of princes and lay bare their methods and madness. There is a paradoxical quality to this interpretation of the sort we have come to expect from Rousseau, namely, that even as Machiavelli is fashioning masks for a prince, he is unmasking him. By exposing the prince's stock-in-trade, Machiavelli is arming republicans with all the knowledge they need to avoid being deceived. A simple and telling criticism can, however, be lodged against this Rousseauian opinion: Machiavelli could not be writing a book for republicans, because he never intended that they read it. Interpretative accuracy often hangs on matters of practical political action, not to mention the intention of where to publish and for whom, and in this case we find no evidence that Machiavelli did or attempted to do anything with his treatise but send it to Francesco Vettori, his contact in the Medici Palace.

A second version of Machiavelli as a strong republican hardly takes *The Prince* seriously, for it assumes that the treatise was not intended seriously. Garrett Mattingly (1958) holds that Machiavelli's advice book is nothing more than a joke, a "diabolical burlesque" of the mirror-of-princes literature prevalent in the Renaissance. Nominally agreeing with Rousseau, Mattingly argues that Machiavelli wrote *The Prince* as an alarm, a "tocsin"

to the people of Florence. However, this agreement is surely more literary than political, for, unlike Rousseau, Mattingly does not read the treatise as revealing certain truths about princely power that republicans should know. He sees *The Prince* primarily as a fine example of Machiavelli's dramatic temperament, a reflection of his ability simultaneously to shock and amuse his audience. Machiavelli surely intended to shock and perhaps to amuse, and *Mandragola* fully displays his dramatic skills, but this does not suffice to make *The Prince* a burlesque, even if a diabolical one. The most obvious problem with Mattingly's reading is that it fails to take seriously Machiavelli's desire to reconstitute the political world. In *The Prince*, Machiavelli (P, p. 56) declares his intention "to write something of use to those who understand."³ He wishes to reveal reality, not ridicule it, and his repeated instruction concerning princely *virtù* sounds less like a satire on the corruption of power than an attempt to determine how those in power might use guile and deceit to mitigate the corruption of the state.

The third strong republican perspective reads *The Prince* literally, as an advice book for a founder. Machiavelli's prince is to be the restorer of order, the man of *virtù* who will lay the foundations from which the republic will emerge. Thus, *The Prince* is "phase one" of a series of events that will lead to liberty and republican government in "phase two." On this account, *The Discourses* takes its point of departure from the (hoped-for) realization of the prince's plans. Put another way, *The Prince* has to do with "heroic" politics, *The Discourses* with mass politics of a republican sort, made possible by the heroism of the virtuoso leader. Thus Sheldon Wolin (1960, p. 231) contends that the prince will "render himself superfluous" and therefore "give way" to the rise of mass politics and the republic. Exactly how the founder renders himself

superfluous or how he gives way to the republic, Wolin does not say; the implication, roughly, is that he creates institutions that will subsume and outlive him. Hence the major issue, for Wolin (1960, p. 231), is whether or not the state will be capable of "generating its own momentum" after the founder is dead.⁴

Two problems beset Wolin's attempt to solve the puzzle of the transition from *The Prince* to *The Discourses*, and so to shore up the third version of the strong republican thesis. The first problem is textual: in *The Prince*, Machiavelli gives no specific advice concerning the foundation of republican institutions. Indeed, he does not deal with republics at all. On this score, we should compare *The Prince* with a later work, Machiavelli's advice to Pope Leo X, "On Reforming the State of Florence" (Pansini, 1969, pp. 633-34). There he does offer lengthy and detailed directives on the organization of a republic, and advises, among other things, the reopening of the hall of the Council of the Thousand, the redistribution of offices to the general public, and the return of a gonfalonier. Nothing even remotely similar to this appears in *The Prince*, where Machiavelli seems content to develop Lorenzo's knowledge of historical examples and his appreciation of deceit and violence, rather than his familiarity with republican *ordini*.

The second problem with Wolin's thesis is more political in nature: Would a theorist as cognizant of the vicissitudes of *fortuna* as Machiavelli content himself with the notion that "heroic politics" will somehow "give way" to mass politics, that the death of the prince will lead to the rise of the republic? Moreover, would a Florentine who knew well the personalities of the Medici princes—Giuliano (a man of little ambition, with a lack of aptitude for dealing with Florentine affairs) and Lorenzo (an unapproachable autocrat with Spanish pretensions) (Gilbert, 1984, p. 135; Hale, 1977, p.

99)—truly imagine them fit subjects for heroic politics, much less men great-hearted enough to relinquish their power after creating the conditions for a new republic?

Surely these rhetorical questions answer themselves. The history of the government of Florence, with its unpredictable oscillations between various forms of princely and republican rule, taught Machiavelli at least one lesson: that to wait (as was the Florentine habit) and expect to benefit from circumstances brings disaster. What is necessary is to act and to act boldly to change circumstances. This, in fact, is the very lesson Machiavelli teaches his prince, so it seems odd, to say the least, to assume that Machiavelli himself is willing to wait for the prince's retirement or death and expect that circumstances will, in due course, eventuate in a republic.

These last observations give us the needed purchase, I believe, to understand Machiavelli's strong republican intentions and to solve the puzzle of *The Prince*. Given the Florentine's commitment to boldness and his conviction that successful political action requires the mastery of circumstances, does it not seem plausible that *The Prince* could be read as a political act in itself, a bold attempt to change existing conditions? Despite its main fault, the Rousseauian interpretation is compelling precisely because of this—it approaches *The Prince* as praxis and renders Machiavelli a political actor as well as a political theorist. Again, however, it must be remembered that Machiavelli sent his tract to the prince and not to Piero Soderini, the deposed gonfalonier, or to his republican friends in the castle at Volterra. So the puzzle remains. If we are to be committed to a reading of Machiavelli as a strong republican and of *The Prince* as praxis—as I propose we should—then in what sense is this little book a bold attempt to alter circumstances? How is the author of *The*

Prince to be reconciled with the author of *The Discourses*?

These questions can be answered only if we remember Machiavelli's awareness of the advantages of crafty assault, and consider another, arguably more plausible, interpretative possibility: that *The Prince* is not simply about deception, but is itself an act of deception, and that this theorist of deceit is at the same time a practitioner of that very art. In other words, *The Prince* is a tract that in fact aims to restore a republic, though in appearance it dedicates itself to maintaining a principdom. Machiavelli indeed intends this book for the Medici. Thus, his deception resides not in exposing princely tricks to republicans, but in something far more crafty: he intends for a gullible and vain-glorious prince to heed the duplicitous advice of *The Prince*, and thereby take actions that will jeopardize his power and bring about his demise. Thus, even as Machiavelli (*P*, p. 65) tells Lorenzo that "one who deceives will always find those who allow themselves to be deceived," he is deceiving Lorenzo. Even as he presses upon the prince the need to establish a relationship to others that is unknown to them, Machiavelli places Lorenzo in exactly this relation to himself. Even as he offers his Medici a "humble testimony of devotion," Machiavelli devises a plot, a series of moves that, if followed, will lead Lorenzo to disaster.

This reasoning presumes, of course, that Machiavelli was a decided enemy of the Medici, and that he intended his advice to be followed to its damning letter. We will turn to that shortly. First we need to consider how, in the course of *The Prince*, this master of political deception sets his trap, disguises his own true aims, and makes Lorenzo his *mannerino*.

Trapping the Prince

Machiavelli's conception of politics in *The Prince* is quite clearly drawn from his

understanding of and experience in the art of war. Politics, like warfare, is a vicious struggle to gain control, to dominate and conquer opposing forces, to battle one's way to victory over the enemy. Machiavelli (*P*, p. 53) thus advises his prince to "have no other aim or thought . . . but war and its organization and discipline." That art alone is necessary for glory in politics. The organizer of the Florentine civilian militia also knows that though there is "no comparison" between the armed and the disarmed man, success in war depends upon more than brute assault by sheer force of arms (*P*, p. 54). Machiavelli (1965) recommends another sort of assault in *The Art of War*:

Where the nature of the terrain is such that you cannot draw the enemy into an ambush easily, you may, however, dig ditches and pitfalls in the plains, cover them over lightly with brushwood and clods and leave areas of solid ground through which you may retire in the heat of battle; if the enemy pursues, he is undone. (1965, p. 118)

Thus, for the general and the prince, the art of war and the art of politics require a knowledge of crafty assault as well as of armed combat. The political actor must be as skilled at setting traps as he is at bold, ferocious attack, for when one is foiled by "terrain" and unable to ambush easily, it may be necessary to deceive.

The political terrain of Florence in 1512 was not advantageous for Machiavelli. No "easy ambush" of the Medici lords was possible, and therefore we might remember Machiavelli's advice to generals in such situations: employ strategy and deception and your enemy will be undone. *The Prince* is Machiavelli's stratagem, an act of assault in the form of deception. As has been recognized for centuries, the text itself provides areas of "solid ground," or firm advice a new prince in a new territory can rely upon to gain and maintain his power. What has been missed, however, is Machiavelli's deceit. Amidst this solid advice he prepares "ditches and pitfalls" in

the form of subversive directives for his Medici lord, which he then covers over with promises of power, glory, and popular support. This deceptive advice to Lorenzo concerns three decisive matters for a prince: where to live, how to behave, and whom to arm. If we read Machiavelli's counsel to the prince with historical information at hand, its subversive character begins to appear. If we read it with a complete understanding of Lorenzo de Medici's circumstances in Florence, the conclusion seems obvious: Machiavelli is out to undo this enemy of the republic. Let us consider what he says.

Where to Live

Machiavelli begins *The Prince* by stating that his subject matter concerns monarchies, not republics (*P*, p. 5). This is surely disingenuous, for although his discussion of kinds of principalities and how they are acquired and kept (*P*, pp. 5-41) focuses primarily on princely power and not popular governance, one of his main categories of principality is the former republic. Machiavelli says that of all the new princes, the one who becomes ruler of a once-free city faces the most overwhelming difficulties, and he devotes a short chapter (*P*, pp. 18-19) to explaining princely options in such a situation. We, like Machiavelli, might expect that Lorenzo, a new prince in a former republic, would be particularly interested in this chapter, so we should note what Machiavelli prescribes when he addresses "the way to govern cities or dominions that, previous to being occupied, lived under their own laws" (*P*, p. 18). Machiavelli offers the prince in a formerly free state three choices: he may either despoil, live within, or restore the freedoms of the occupied city. He then discounts the third alternative by appealing to history: The Romans unsuccessfully tried to hold Greece and, at the same time, allow her freedom; hence, their only recourse was

to lay waste and despoil the country as they had done in Capua, Carthage, and Numantia. The paradox is not lost on Machiavelli—in order to maintain power in a former republic, one must destroy it. What renders such extreme measures necessary is the character of the citizenry of a subjugated republic: "They do not and cannot cast aside the memory of their ancient liberty," Machiavelli warns, but then he concludes his chapter by offering the second option as another resort. If the prince can neither "lay waste" to the city nor restore its freedoms, then he must reside in it (*P*, p. 19).

Upon first reading, Machiavelli's advice in chapter 5 seems solid. Indeed, he does not even bother to defend it. In chapter 3, on "mixed monarchies," he argues that a prince's residence within his conquered territory renders possession "more secure and durable" (*P*, p. 8) and allows for the immediate remedy of disorder, and in chapter 6, he reiterates that the maintenance of power is facilitated by a prince "being obliged to reside personally in his own territory" (*P*, p. 21). Yet there is something curious, even contradictory, about his advice in chapter 5. Before he counsels residence, Machiavelli unequivocally states that "whoever becomes the ruler of a free city and does not destroy it, can be expected to be destroyed by it." A motive for rebellion against the prince, he argues, can always be found in "the name of liberty," which republican citizens cannot cast aside (*P*, p. 18).

If this is the case, then what should we make of his advice? It seems that a prince who lives within a conquered republic would stand to lose rather than benefit, particularly if the people have not forgotten "the name of liberty." A prince within a city is easier to find and destroy than one who lives in a country villa, as had been the habit of the Medici family; they maintained a palace within Florence but spent much of their time in their villas—Careggi, Cafaggiolo, Castello, Fiesole,

Poggio a Caiano—outside the city (Burkhardt, 1958, p. 399; Wackernagel, 1981). Machiavelli's advice seems designed to change the residential practice of the family by strategically placing the prince inside the city's walls. Yet at the same time it seems to run counter to his warning about the vengeful nature of former republicans—why should a prince live in their midst?

Perhaps our perspective is not yet complete. Machiavelli may be determined to assure Lorenzo's power by offering further advice on how to neutralize the "desire for vengeance" and the love of liberty that inflame republican hearts, so that even though the prince resides within the city, he will be secure. Machiavelli does give advice on this score, but what he says is curious indeed.

How to Behave

If any one piece of advice occurs repeatedly in *The Prince*, it is Machiavelli's dictum that the ruler should always strive to gain the favor of the people. In chapter 9, "Of the civic principality," Machiavelli tells Lorenzo to reject the "trite proverb" that, "He who builds on the people builds on mud," for the prince who animates the masses will find "he has laid his foundations well" (*P*, p. 38). Machiavelli reiterates this point in various ways throughout the treatise (*P*, pp. 60, 61, 63, 67, 71, 75, 76, 80). Because the friendship of the people is the prince's "main resource" in times of adversity, he must avoid incurring their hatred in order to insure against conspiracy or ruin; this is "one of the most important matters a prince has to confront" (*P*, p. 67). Machiavelli acknowledges that princes cannot always avoid being hated by someone, but it is best if those who hate him not be the people (*P*, p. 67). In a new age, then, all princes (except the Turk and the Sultan) ought to aim at satisfying the *popolani*. Implicit is a corollary Machia-

velli makes explicit in his chapter on civic principalities: be wary of the nobility. The nobles are portrayed as untrustworthy, dishonest, greedy, and dangerous. Machiavelli writes:

[F]rom hostile nobles [the prince] has to fear not only desertion but their active opposition, and as they are always more far-seeing and more cunning, they are always in time to save themselves and take sides with one who they expect to conquer. (*P*, pp. 36-37)

Hence the wise prince will, when possible, esteem his nobles, but more often he will be suspicious of them. The people provide a far firmer foundation for power.

Before we accept the astuteness of Machiavelli's advice to Lorenzo, we should recall this Medici's situation. As we shall see, the Florentine people were not well inclined toward the new Medici. The mood of the city had changed markedly since the days of *il Magnifico*. The Florentines had become accustomed to a republic; what opposition there was to it came primarily from the aristocracy. It seems, then, that an astute advisor would have told Lorenzo to turn for support to the very class Machiavelli tells him to suspect—the *ottimati*. All the more curious is Machiavelli's own acknowledgement of this elsewhere (though not to Lorenzo in *The Prince*). In his document to Pope Leo X, "On Reforming the State of Florence," Machiavelli tells the Pope what would have to be done if a prince wished to turn the city into a monarchy:

[I]n Florence where there is a great sense of equality, one would first have to introduce inequality and create nobles with castles and villas, who would join the prince in suppressing the city and the whole province with their armies and factions. A prince alone, without the nobles, cannot bear the weight of a monarchy. (Pansini, 1969, p. 20)

Despite the fact that *The Prince* is (ostensibly) dedicated to a prince who wishes to maintain his power, nowhere in the treatise does Machiavelli offer Lorenzo the advice he gives Pope Leo. In

fact, he says exactly the opposite and issues warnings about trusting nobles and alienating the people. Of course, it may be that between 1512 and 1521 Machiavelli simply changed his mind on the subject of whose favor the prince should seek, but before we draw this conclusion we might look at another aspect of his advice on how to behave.

Machiavelli takes up the subject "Of liberality and niggardliness" in chapter 16. In advising the prince on how to behave, he again reminds him of the importance of not being hated by the people, observing that

one who wishes to obtain the reputation of liberality among men, must not omit every kind of sumptuous display, and to such an extent that a prince of this character will consume by such means all his resources and will be at last compelled, if he wishes to maintain his name for liberality, to impose heavy taxes on his people, become extortionate, and do everything possible to obtain money. This will make his subjects begin to hate him. (*P*, p. 58)

To underscore his warning against liberality (*liberalita*), Machiavelli concludes the chapter by saying, "of all things that a prince must guard against, the most important are being despicable or hated, and liberality will lead you to one or other of these conditions" (*P*, p. 60). Thus, the prince (who has already noted the danger of alienating the people) will agree to practice "niggardliness" (*parsimonia*)—it is one of the vices that secures his power. However, we have evidence to suggest that Machiavelli's warning against liberality in *The Prince* is more a matter of republican sympathies than helpful advice, for in *The History of Florence* he reveals how the Medici benefitted from liberality.

Book 8 of Machiavelli's *History* relates, among other things, the famous tale of the Pazzi conspiracy against *il Magnifico*, Lorenzo de Medici, in 1478. The details of the attempted but unsuccessful plot against Lorenzo's life need not concern us,

but what Machiavelli says about the outcome of the conspiracy is instructive. One of the elements of the plan called for Francesco Pazzi to ride to the gates of the city in the aftermath of Lorenzo's assassination, calling the people to liberty and to arms. However, events took a different turn. Francesco was wounded in the attempt to kill the prince, and it fell to his uncle, Iacopo, to sound the alarm. Machiavelli explains and analyzes the failure of this final effort by the Pazzi in the following way:

Iacopo rode out with perhaps a hundred men who had been prepared for this job, to make this last trial of their fortune, and he went to the Palace square calling the people and liberty to his aid. *But the first had been deafened by fortune and the liberality of the Medici* and the second was unknown in Florence, so there was no response. (1970, p. 273, emphasis added)

It is impossible to miss Machiavelli's conclusion—the liberality of the Medici had garnered the people's support. Indeed, the family's practiced munificence and heavy public spending had deafened the Florentines to the *cri de coeur* of the republic—that is, to liberty.

Armed with Machiavelli's analysis of this event, we can now read his advice in chapter 16 of *The Prince* in a different light. His injunction against liberality is intended to deprive Lorenzo of a tactic that had worked exceedingly well for the Medici in the past. As Machiavelli sees it, a liberal prince can spend, then depend on the people's goodwill, a miserly one cannot. It seems, then, that even as he assures Lorenzo that miserliness will win him public support and keep him from being hated, Machiavelli takes steps to guarantee that the name of liberty is not forgotten in Florence—and it is in liberty that citizens of a former republic "can always find a motive for rebellion" (P, p. 19). Yet, as Machiavelli knows, rebellion may come to naught unless there are good arms to strengthen it. We must now consider his advice on arms.

Whom to Arm

In chapter 20 of *The Prince*, Machiavelli takes up the matter of "whether fortresses and other things which princes often contrive are useful or injurious." Allan Gilbert (1938, p. 162) has noted this chapter's "un-Machiavellian" advice to the ruler about how to gain the support of the people, but let us take another look. The chapter begins with a review of what princes who want "to hold their possessions securely" have done, and Machiavelli notes that no "definitive judgment" is possible on these matters (P, p. 77). Conditions and circumstances vary and require different responses. But, then he issues a most definitive statement:

A new prince has never been known to disarm his subjects, on the contrary, when he has found them disarmed he has always armed them, for by arming them these arms become your own, those that you suspected become faithful and those that were faithful remain so, and from being merely subjects become your partisans. (P, p. 77)⁵

As if to leave no further doubt on the matter, Machiavelli goes on to argue that the new prince who disarms his subjects offends them and generates hatred toward himself. As we have seen, he has taken care to prepare the solid ground for this advice by telling the prince that the hatred of the people is precisely what he must avoid; one of his most potent remedies against conspiracy is not being hated by the masses. Therefore, Machiavelli concludes, "a new prince in a new dominion always has his subjects armed. History is full of such examples" (P, p. 78). His advice on this score follows from chapter 14, where the prince has been warned of the evils of being disarmed, and from chapter 12, where the prince has been told of the disastrous consequences of hiring mercenary and auxiliary troops.

On the surface, perhaps, Machiavelli's advice on arms seems sound, even "un-Machiavellian," but that impression begins to blur if we consider a seemingly

obvious point. Machiavelli's suggestion that the granting of arms inspires loyalty and makes partisans out of subjects, if taken to be sincere, fails to account for the possibility, indeed the probability, that arms may also facilitate plots, incite insurrection, and inspire rebels. The new prince who arms his subjects may just as easily make himself a mark for overthrow by creating the very instrument of his own destruction, namely, a civilian militia. Moreover, earlier in the treatise, when Machiavelli discusses such virtuous new princes as Francesco Sforza in Milan or Cesare Borgia in the Romagna, he makes no mention of their having armed their subjects, doubtless because they did not. His bold claim that "history is full of such examples" is followed by no examples at all, an odd omission for a thinker who is otherwise so willing to present specific historical examples for the prince to emulate.

Most curious of all, however, is Machiavelli's omission of a historical example that would have meant much to Lorenzo. In a letter written to Piero Soderini at the same time he was composing *The Prince*, Machiavelli (1961) makes mention of some causal histories that have led various princes to greatness, and notes that

Lorenzo de Medici disarmed the people to hold Florence; Messer Giovanni Bentivogli in order to hold Bologna armed them; the Vitelli in Castello and the present Duke of Urbino in his territory destroyed the fortresses in order to retain their states; Count Francesco and many others built them in their territories to make themselves sure of them. (1961, p. 98)

Machiavelli cites the Bentivogli, the Vitelli, and Francesco Sforza in his discussion of the things princes contrive in chapter 20, but he never acknowledges the example of Lorenzo de Medici, a new prince who disarmed his subjects in order to hold them. Surely this is odd, for of all the historical examples for the younger

Lorenzo to emulate, his grandfather would have been the best. Not only was *il Magnifico* the most artful *principe* of the Medicean line, he was also the most brilliant secular figure of the age, both loved and feared by the Florentines. But quite clearly, the example of Lorenzo the Magnificent directly contradicts Machiavelli's advice on arming subjects, and Machiavelli appears content to omit this piece of information in his "definitive" advice to the new prince.

The issue here, however, is not simply whether new princes have in the past routinely armed their subjects and, in essence, created civilian militias. At issue is another, more immediately historical matter: is Machiavelli's advice wise counsel for a Medici in Florence? From what we know of the history of the city, the answer to this question can only be no.

In Florence, the idea of liberty was deeply rooted in political tradition. The city's sense of freedom persisted through periods of oligarchical rule, rigged elections, and partisan foreign policies. A proclamation of July 1329, passed by the *pratica* (a citizen assembly), declared that the city would never submit to the autocratic rule of one man, "since liberty is a celestial good which surpasses all the wealth of this world" (Rubinstein, 1968, p. 450). That "celestial good" was what the Florentines believed contributed to their greatness. Coupled with this tradition of republican liberty, the Florentines had a history of strong opposition to the Medici, which grew particularly virulent in the mid-fifteenth century. In an oath sworn in May of 1466, 400 citizens over the age of 14 declared a political program of opposition to Medici rule that demanded, in part, that citizens "be free to debate and judge public and popular government" (Rubinstein, 1968, p. 458). Nor were the Florentines without individual voices raised in defiance of the Medici usurpation of liberty. In his

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Laudatio Florentinae Urbis, Leonardo Bruni declared, "nothing can be achieved by the covetousness of single citizens against the will of so many men," and suggested safeguards against autocratic power (Rubinstein, 1968, p. 446). Alamanno Rinuccini published *De libertate*, a powerful attack on Medici tyranny (Rubinstein, 1968, pp. 461-62), and Girolamo Machiavelli, Niccolo's great-granduncle, was tortured and executed for his part in leading the opposition to Medicean measures instituted under the rule of Cosimo (Machiavelli, 1970, p. 218; Ridolfi, 1963, p. 2).

There is no reason to suspect that this opposition to the Medici or the spirit of Florentine republicanism itself had softened or disappeared by 1512, or that Machiavelli was unaware of it. Without question, the republic was anything but stable and secure; internal struggles between the middle class and the aristocracy were unending, making institutional reform immensely difficult and external affairs precarious. By 1510, Piero Soderini, the gonfalonier, was out of favor; the aristocracy was pressing for his removal from office and for a return to *governo stretto*, though not for an end to the republic. There is, in fact, little evidence to suggest that there was any popular sentiment favoring the return of the Medici, nor is there any indication that the Medici's assumption of power was generated in any major way by forces within the city.⁶ No doubt the family had allies among a small number of wealthy families and some younger aristocrats who stood to gain commercially and politically from the decline of the republic (Brucker, 1969), but in the end, the Medici resumed their power with the aid of Spanish bayonets, not the Florentine citizenry (Gilbert, 1984; Hale, 1977; Schevill, 1936). The fact that, once installed in power, the princes kept foreign troops in the city and guards at the palace indicates that they felt some uncertainty

about their popularity. Perhaps this is why the younger Lorenzo, unlike his grandfather, *il Magnifico*, rarely ventured into public places to mingle and meet with the citizens. When he did, he was accompanied by armed guards (Gilbert, 1984, p. 108).

With respect to the matter of arms and Florence, let us further consider Machiavelli's advice to Lorenzo on fortresses, for it is as curious as his advice on arming the citizenry. In fact, his injunction against a fortress, when read in light of Florentine republicanism, also seems far more in keeping with the interests of the republic than with those of the Medici.

In chapter 20, Machiavelli tells Lorenzo that "a prince who fears his own people more than foreigners ought to build fortresses, but he who has greater fear of foreigners than of his people ought to do without them" (*P*, pp. 80-81). This is clever strategy, considering that Machiavelli has previously condemned the prince who fears his own people, and has stressed the danger of placing too much trust in foreign powers. Hence, the prince who can read at all cannot help but decide, as Machiavelli would have it, not to build a fortress. More metaphorically, the best fortress is to be found in the "love of the people" (*P*, p. 81).

Militarily, Machiavelli's counsel on fortresses runs counter to the prevailing views in Tuscany in the early Renaissance. Fortresses were considered useful in defending a dominion from outside enemies. In opposition to this, Machiavelli argues that a *fortezza* is useless and cites the example of the Castello Sforzesco, which gave "more trouble to the house of Francesco Sforza than any other disorder in the state" (*P*, p. 81).⁷ At best, Machiavelli's military analysis of this matter is exceedingly thin. He seems more interested in the internal, political implications of this strategy, rather than the external military ones. Again, we must examine what such an internal strategy

would mean in the context of Florence under a Medici lord.

If the tradition of liberty distinguished Florentine republicanism in a political sense, then the walls of the city marked her republicanism in a strategic one (Hale, 1968, p. 502). The Florentines were notoriously wary of the subject of fortresses within the city. A *fortezza* symbolized the antithesis of republicanism, signaling the demise of popular governance and the emergence of an inner power elite by providing an autocrat with an impregnable stronghold. Observing the circumstances of neighboring city states, the Florentines saw that princes often constructed fortresses in the name of military security, but in fact used them for purposes of domestic oppression. Not surprisingly, then, the Florentines, with their strong republican traditions, viewed the building of a fortress as both a symbolic and a literal danger.

Machiavelli shares this suspicion. In a letter to Guicciardini, written in 1526, he equates the building of a fortress with the enslavement of the Florentines and warns that "the most harmful thing a republic can undertake is to enact something strong or that easily can be made strong within its body" (Machiavelli, 1961, p. 235). He goes on to observe that, if a fortress existed in Florence, "any powerful man" who conquered the city would, upon entering, find it a convenient stronghold, and the Florentines "would become slaves without any protection" (Machiavelli, 1961, p. 235).⁸ These observations raise yet another puzzle. If Machiavelli is so convinced of the danger a fortress poses to a free Florence and of the advantages it holds for a prince, then why, when devising a strategy for the Medici, does he not recommend the building of a *fortezza*? Indeed, what would a Medici prince who is backed by Spanish troops in a city under siege stand to gain by not fortifying himself? Yet in the face of such facts, and knowing the

problems a fortress would present for republican activity, Machiavelli does not recommend that Lorenzo build one; nor does he recommend any strategies of "containment" other than the "love of the people." How can we account for this puzzling advice on arms and fortresses?

The mystery or oddity of Machiavelli's treatment of arms and fortresses can be explained in only one way. He offers Lorenzo advice on security, with the intention of delivering him into republican hands. Machiavelli has not lost sight of the reality of Florentine politics; he knows full well what the consequences will be if Lorenzo resides in the city, foregoes liberality, arms the people, distrusts the nobles, refuses a fortress, and mingles "from time to time" with the Florentines. In a city where "the desire for vengeance" runs deep and "the memory of ancient liberty" shines bright, an "unarmed prophet" is never fully secure; an unwanted prince who arms his subjects and does not protect himself is even less likely to survive. As his "advice book" proceeds, Machiavelli's warning in chapter 5, "whoever becomes the ruler of a free city and does not destroy it, can be expected to be destroyed by it," (P, p. 18) takes on the character of prophecy. Lorenzo will not destroy Florence; that much is clear. His only alternative (if he takes Machiavelli's counsel) is to reside in the city. Once therein, Machiavelli will have him adopt policies that are, in fact, republican snares designed to entrap him. He will be destroyed. These are the "ditches and pitfalls" that lie beneath the seemingly solid ground of Machiavelli's advice to his Medici lords.

Machiavelli and "The New Sons of Brutus"

If I read it aright, *The Prince* is itself an act of political deception. In addition to the deceitful "advice" in the text itself,

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signs of crafty assault are also to be found outside Machiavelli's text, and all point in only one direction: not only was Machiavelli no friend of the Medici; he was a decided enemy. He believed that the Florentine republic had to rid itself of their control if it was to revive and flourish. Other documentary sources and Machiavelli's own political biography help shore up my case.

Among Machiavelli scholars, it is generally agreed that in 1513 he was concerned with getting into the Medici's good graces, finding employment, and returning to active political life. According to Quentin Skinner (1981, p. 21), "As soon as he came out of prison, Machiavelli began scheming to recommend himself to the city's new authorities." To insure the success of his scheming, Machiavelli wrote numerous letters to his friend, the erstwhile republican Francesco Vettori, now the Medici's ambassador to Pope Julius II and Machiavelli's only access to the corridors of power. At every turn, the political exile appears eager to ingratiate himself to the Medici and, through Vettori, to assure the family of his trustworthiness, his honesty, and his support. In the famous letter of December 10, 1513, Machiavelli (1961, p. 142) tells Vettori of his "little work" *On Princedoms* and of his intention to offer it to the new prince, Giuliano, in the hope that the prince will welcome it and make use of the talents of the author. The dedicatory pages of *The Prince* seem to provide further evidence that Machiavelli was concerned with promoting the greatness of the Medici and with assuring their success through the benefit of his knowledge of the "art of the state."

That Machiavelli was a schemer is beyond doubt. Nor does there seem to be any question that he was eager to have *The Prince* gain the eye and approval of Giuliano and, later, Lorenzo. However, those who are tempted to read *The Prince* as so much Machiavellian opportunism

would do well to consider what Machiavelli has to say in *The Discourses* about "the sagacity and severity of Junius Brutus," the "father of Roman liberty" (D, p. 402). We will return to the severity of Brutus shortly. Let us first consider his sagacity.

In book 3, chapter 2 of *The Discourses*, Machiavelli notes that more than anyone else, Junius Brutus deserves to be esteemed for his "simulation of folly" (D, p. 403). Having neither the arms nor the forces to mount an open war against the kings of Rome, Brutus instead ingratiated himself with those in power. On this matter, Machiavelli takes issue with Livy's interpretation of Brutus's motives and suggests a reading of his own:

Titus Livius gives but one reason that induced Brutus to this simulation, namely, that he might live in greater security and preserve his patrimony, yet if we well consider his conduct we are led to believe that he had another reason, which was that by thus avoiding observation he would have a better chance of destroying the kings and of liberating his country, whenever an opportunity should offer. (D, p. 403)

Clearly, Machiavelli not only holds Brutus in great esteem for his simulation of folly, but also for his commitment to liberating his country, and he offers Brutus as a model for others who are "dissatisfied with their ruler." In Machiavellian terms, if one cannot play the lion and threaten the prince directly by force of arms, then one must be a fox and undermine the enemy from within. "It is advisable . . . at times to feign folly, as Brutus did," Machiavelli writes, "and this is sufficiently done by praising, speaking, seeing and doing things contrary to your way of thinking and merely to please the prince" (D, p. 404).

Interpreters of Machiavelli should be wary of repeating Livy's mistake about Brutus, and so avoid being tricked by appearances. If Machiavelli had designs on the Medici, as I think he did, then it seems reasonable to expect that he would seek to

prove himself a trustworthy advisor, and so get the Medici to follow his advice. If we keep Machiavelli's praise of Brutus's sagacity in mind, then his own flattery of Lorenzo and his appeals to Vettori seem even less likely to be matters of simple opportunism, and rather more deeply political. Like Brutus, Machiavelli trades on the appearance of friendship and admiration for his ruler; he "feigns folly" for a political purpose—to ingratiate himself to Lorenzo, thereby earning his trust so that the prince might be destroyed—much as Junius Brutus destroyed the kings and restored the liberty of Rome.

There are other reasons to question whether Machiavelli was so monomaniacally consumed with getting his old job back that he simply shed his republican sympathies and dedicated himself to serving Florence's Medici lords. First, there is the political history of the Machiavelli family itself. Though family legacy need not determine one's loyalties, it generally serves as a source of personal identity as well as of reflection, particularly in an age where a family's identification with the political order is of great and lasting importance. Machiavelli himself points out in a chapter in *The Discourses*—straightforwardly entitled "Reasons why the same family in a city always preserves the same character"—that what a youth hears praised or censured within his family "becomes afterwards the rule of his life for all time" (*D*, p. 535). As Machiavelli's biographer tells us, the Machiavelli were an old Florentine family, noted for their active participation in Florentine public life and, most significantly, for their devotion to the republic. They gave the city some 12 gonfalonieri and 54 priors (Ridolfi, 1963, p. 2). As I have noted, Girolamo Machiavelli died in prison because he dared to speak out against the oligarchy of Cosimo de' Medici, and another Machiavelli, Francesco, was remembered for a public speech in 1424 that condemned tyranny and praised

liberty: "The enjoyment of freedom makes cities and citizens great: this is well known. But places under tyranny become deserted by their citizens and engage in their extermination" (Pitkin, 1984, p. 204). Whether Machiavelli knew of Francesco's speech is uncertain, but its sentiments were in all likelihood passed on, for they are clearly evocative of some of the central tenets of *The Discourses*.

In short, there seems little question that the Machiavelli family viewed Piero de' Medici's ouster in 1494 (when Niccolò was 25) and the revival of republicanism more favorably than did the aristocratic Tournabuoni, Guicciardini, Strozzi, or Neroni families, who feared for their interests under a radically republican regime. Also, the sentiments of Florence itself were generally more "Machiavellian" than aristocratic. Piero's three attempts to return forcibly were met with armed resistance from the citizenry. Effectively defeated in 1498—the year Niccolò was appointed Second Chancellor—the Medici finally ceased plotting against the republic, retired to their country villas, and left *fortuna* to do what they could not.

In compiling evidence of Machiavelli's animus toward the Medici, we must, secondly, consider some crucial events in his own life, as well as the wary view the Medici took of him. Although the family was a suspicious lot, they were not wholly indiscriminating in their purge of republicans. Thus, when the chancery was reorganized in 1512, the First Chancellor, Marcello Virgilio, was allowed to remain in office (Hale, 1961, p. 134). In fact, Machiavelli was the only chancery official dismissed by the Medici (Gilbert, 1984, p. 173).⁹ Without doubt, the Medici regarded Machiavelli with considerable suspicion. As we know, nearly a decade later they granted him a modest stipend to write his Florentine history, but even though his literary skills were gaining fame, his political trustworthiness was

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still very much in doubt. Indeed, the family never allowed him back into any position of political power.

We must also remember the Boscoli-Capponi affair. A few months after his removal from office, Machiavelli was implicated in a conspiracy against Giuliano when his name was found on a list in the possession of one of the two young conspirators. The Medici imprisoned and tortured him on the rack but found out nothing (Mattingly, 1958, p. 485). Machiavelli may have been ignorant of the plot, but it is a significant political fact that the anti-Mediceans surely perceived him to be a potential ally, one who might be counted on for support had the assassination succeeded.

Nine years later, in 1522, Machiavelli again fell under suspicion following the abortive plot to assassinate Cardinal Giulio de Medici. The plot was organized by two of his closest friends, Zanobi Buondelmonti (to whom he had dedicated *The Discourses* a few years earlier) and Luigi Alamanni, a poet and fellow member of the Oricellari circle. In 1524, one of the accomplices revealed that Buondelmonti had mentioned Machiavelli's name as one of several citizens who should be invited to join the plot (Ridolfi, 1963, p. 203), but by then the dust had settled. Buondelmonti and Alamanni had fled the country, two others had been executed, and Machiavelli was in refuge in San Casciano, writing his Florentine history.

For all we know, Machiavelli may have or may not have been involved in either or both of these conspiracies. Assuming, conservatively, that he was not, we can read this noninvolvement either as a sign that he was loath to conspire against and more willing to work with the Medici in order to stabilize Florence, or as a sign of his wariness about the success of conspiracies in general, even if he wanted to see the Medici removed from power. What Machiavelli writes appears to substantiate

the latter view, at least in part. His famous chapter on conspiracies in *The Discourses* (which Buondelmonti must have read), though filled with advice on how to conspire, is replete with warnings about the slim chances conspirators have for success (*D*, pp. 410-36). And in his history, Machiavelli writes, in reference to attempted assassinations of the Medici, that "because conspiracies rarely succeed, they most often bring about the ruin of those who plan them and they bring greatness to those against whom they are directed" (*D*, p. 263). We might expect, then, that Machiavelli's reluctance (if reluctance it was) to participate in conspiracies owed more to caution than to allegiance. Indeed, his republican friends must have taken his noninvolvement in their conspiracies (if again, noninvolvement it was) in this latter light, for in 1527, when the Medici were once again forced from Florence, Buondelmonti and Alamanni, now returned, supported their friend's appeal for employment in the restored republic, at a time when the "enemies of the republic," Guicciardini and Vettori among them, were being called into court (Gilbert, 1984; Ridolfi, 1963).

Apart from *The Prince*, Machiavelli's writings provide us with some final clues about his attitude toward the Medici. What he writes in reference to the family and their supporters is decidedly negative, although his true opinion is not always immediately apparent—nor could it be. These were treacherous times and one risked imprisonment, or worse, if one spoke too critically of the Medici lords or their policies. Political tracts and correspondence had to be crafted slyly; an enemy of the regime had to know how to appear to be loyal, even though he actually was not.¹⁰

Machiavelli practices this political strategy in the *History of Florence*. The work was not simply an intellectual endeavor; there were political implica-

tions as well, since it had been commissioned by Cardinal Guilio, a member of the Medici family. Machiavelli was not unaware of the delicacy of his task. He wrote to Guicciardini asking for advice on what to say about the family and how to say it. Then he concluded, "I . . . shall try to act in such a way that, since I tell the truth, nobody will be able to complain" (Machiavelli, 1961, p. 206). What we find in the history, particularly in the sections devoted to the Medici, is truth telling of a particularly "Machiavellian" kind. There are numerous flattering references to the Medici, to be sure. Machiavelli calls Cosimo, "the most famous and respected citizen . . . who ever lived," describes Piero de Medici as a man of "*virtù* and goodness" and a great citizen, and extolls Lorenzo the Magnificent's beneficence, his contribution to the city, his dignity, and his property, "worthy of a King" (Machiavelli, 1970, pp. 220, 246, 315-17). In short, Machiavelli's truth telling seems designed to cast the Medici in the best possible light, and to play to the cardinal's family pride.

A closer reading, however, reveals a different sort of truth telling. If we put Machiavelli's assessment of the Medici princes in the context of some of his more general comments on the corruption of Florence, the family does not fare so well. At the start of book 7, where he shifts from a consideration of foreign affairs to "troubles at home," Machiavelli notes that the unity of a city is threatened most by the presence of factions and partisans. Citizens, he says, can make a reputation either publicly or privately. The latter course is reprehensible, for "this sort of behavior gives rise to factions and partisans, and a reputation won in this way is as offensive as the other kind is valuable" (Machiavelli, 1970, pp. 214-15). The tactics employed by private citizens include doing good to various other citizens, helping someone with money, getting him undeserved honors, and beguiling the

lower classes with public benefactions and amusements. In short, the private citizen creates a coterie of "hangers-on," as Machiavelli puts it, in order to advance his own personal goals. Machiavelli's tone indicates his contempt for such activity and for citizens such as these, who contribute to the corruption of cities like Florence.

We need only match Machiavelli's description of the offensive behavior of private citizens with his ostensible praise of the Medici to see that in extolling the princes he is actually condemning them. By his account, the Medici are private citizens; they corrupt the republic by playing to factions. Liberality was Cosimo's forte. We have already seen that Machiavelli thinks liberality deafens a people to the cry of liberty and lulls them into forgetting their freedom. Lorenzo dispensed pensions and aid to the nobility and put on tournaments and pageants for the populace, tactics used by the private citizen in order to foster partisan support. It is Machiavelli's lengthy account of Piero's short-lived regime, however, that best captures his aversion for Medici maneuvers, for he gives there a detailed picture of the ravages of factional and party circumstances that plagued Florence during the Medici years (1970, pp. 228-41). In various ways, then, Machiavelli shows us how circumstances at once necessary for this family's perseverance were profoundly destructive of Florence herself.

Guilio's (now Pope Clement) enthusiastic acceptance of this historical masterwork suggests that Machiavelli achieved his goal—writing a truthful account in such a way that nobody complained. He had also, however, registered a distinctively "Machiavellian" conviction, one that took the form of subtle historical analysis: in order to survive, the Florentine republic must rid itself of the Medici.¹¹

We might turn to one final piece of

written evidence. Machiavelli's conviction that the Medici were a threat to Florentine liberty surfaces in *The Discourses* as well, and returns us to his discussion of Junius Brutus. If Brutus's sagacity enabled him to restore the republic of Rome, his severity helped him to maintain it. The father of the Roman republic condemned his own sons to death because they were the enemies of liberty, even as he was liberty's defender. The lesson Machiavelli (*D*, p. 405) draws from Brutus's example is a characteristic one; whoever makes himself a tyrant and fails to kill Brutus, or restores liberty to a former republic and fails to kill the sons of Brutus, will not maintain himself for long. This seemingly nonpartisan advice is followed by an example, that of Piero Soderini, the deposed gonfalonier of Florence. Soderini realized that the "new sons of Brutus" had to be destroyed, but, as Machiavelli (*D*, p. 405) writes, he did not have the courage to do it. However, Machiavelli's story of Soderini is more than just a counterexample of the severity of Brutus; what makes it interesting is the moral gloss he adds after telling the tale of the fallen gonfalonier:

[O]ne should never allow an evil to run on out of respect for the law, especially when the law itself might easily be destroyed by the evil and [Soderini] should have born in mind that as his acts and motives would have to be judged by the result . . . everybody would have attested that what he had done was for the good of his country and not for the advancement of any ambitious purposes of his own. . . . But Soderini was the dupe of his opinions, not knowing that malignity is neither effaced by time, nor placated by gifts. So that by failing to imitate Brutus he lost at the same time his country, his state and his reputation. (*D*, p. 406)

Machiavelli's harsh judgment of Soderini can be read cynically, of course; the gonfalonier's decline was Machiavelli's as well, and he had reason to resent it. However, the nature of Machiavelli's criticism—his reference to the "evil" that threatened the country and the "malignity" that would not disappear over

time—suggests there is more at stake for him here than the mere salvaging of his reputation. When evil threatens liberty, hopes hang on the sagacity and severity of those who revere the republic. Brutus met this test; Soderini failed it, and so Florence lost her freedom. In Machiavelli's eyes, Soderini's great weakness was that he did not love the republic enough to act boldly and eliminate her enemies before they could conquer Florence and obliterate liberty.¹² Machiavelli's disdain for Soderini is outweighed only by his hatred for those whom Soderini could not destroy—those who threatened liberty—the "new sons of Brutus," the Medici and their allies.

The Prince in Perspective

I began by suggesting that Machiavelli's genius resides in his appreciation of crafty assault in all its guises—the lover's trickery, the general's stratagem, the prince's artifice—but in truth, Machiavelli directs his most penetrating attention to the actor/advisor behind the assault, the one who, observing the scene from a distance, controls the lover, the general, the prince. That is, what Machiavelli admires is a kind of Renaissance artistry—a strategic perspective—that allows for a unique conception of space or terrain, and consequently makes possible the manipulation of persons and events. Ligurio employs just such a perspective in *Mandragola*; Fabrizio exhibits it in his topographical advice to generals in *The Art of War*. But *Mandragola* and *The Art of War* are simply later versions—one dramatic, the other military—of the strategic perspective Machiavelli himself practices as advisor to the prince.

We might measure Machiavelli's success as a political advisor who sees things strategically by the labels he has earned—"political realist" and "master of *realpolitik*"—and by the statesmen and politicians, from Metternich to Kissinger, who

have adapted his strategic perspective to their own times and circumstances. Yet to stop here, as do so many of the political scientists and political actors who cite him, is to overlook a matter of deepest importance to Machiavelli, namely, the political values—republicanism and liberty—that inform his perspective as advisor to Lorenzo de Medici. When, in the introduction to *The Discourses*, Machiavelli (*D*, p. 101) tells his friends to "[look] rather to the intention of him who gives than to the thing offered," he implicitly underscores the distinction between the apparent meaning of any given work, political counsel, or strategic perspective and its deeper purpose. To put this more broadly, political advising involves more than the capacity to analyze events or to see things as they "really are," for the "reality" that informs the analysis is neither a neutral observation nor a scientific truth, but a perspective colored by the values, purposes, and political commitments of the advisor who offers them. When these are at odds with the interests of those in power, the advisor may choose to retire, to capitulate, to oppose openly, or (as Ligurio puts it) "to pursue deceit to its envisioned dearest goal" (Machiavelli, 1957, p. 38) by painting a particular vision of reality for the ruler whom he or she counsels, with a precise purpose in mind. The latter is Machiavelli's strategy as advisor in *The Prince*; his purpose is the restoration of republican liberty.

I want to argue, then, that there is more involved in Machiavelli's advice in his little treatise than a presentation of *realpolitik*, but at the same time suggest that his deeper purpose—to deceive—is not at odds with reading *The Prince* either in terms of a new science or as a work of Renaissance artistry. By way of clarification, let us consider how Machiavelli himself depicts his perspective in *The Prince* and thereby gain one final interpretative clue to his aims and intentions.

In his dedication to Lorenzo, Machia-

velli consciously invokes Renaissance artistry in its most literal sense, and draws an analogy between himself, the advisor to princes, and the landscape painter:

[F]or in the same way that landscape painters station themselves in the valleys in order to draw mountains or high ground and ascend an eminence in order to get a good view of the plains, so it is necessary to be a prince to know thoroughly the nature of a people and one of the populace to know the nature of princes. (*P*, p. 4)

By inviting us to recall the great innovation of Florentine painting—its attention to accurate representation of pictorial space—Machiavelli also discloses a necessary quality of the political advisor. The intellectual disposition, or, to return to Machiavelli's visual metaphor, the "vantage point" of the advisor, must be fully dimensional and complete. It encompasses the actors and influences that populate and permeate the vast political landscape, and thus avoids the restricted perspectives of the prince or the populace, whose visions are governed solely by their respective relationships to one another. Unlike the actors he observes, the advisor stands "outside" the political canvas and integrates particulars into a sweeping contextual vision of reality. He sees actors not as isolated figures and events not as disconnected instances, but as parts of a richly constituted tapestry, a variegated field of competing interests and ambitions. The advisor's special disposition and imagination are, then, the very opposite of the short-sightedness Machiavelli deplores as the mark of politically ineffectual men, those who cannot control events or see beyond their immediate circumstances.¹³

But, as I have argued, there is more involved in Machiavelli's advice than a detached depiction of political reality, just as there was more involved in the Renaissance art of perspective than the achievement of pictorial veracity. In a literal sense, the discovery of perspective also introduced the art of deception to Renais-

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sance painting. Though the depiction of reality was a central concern for the painter, it did not involve a simple mirroring of the physical universe. Rather, the painter selected particular elements of the visible world, then arranged and conveyed them so as to give the illusion of reality (Brucker, 1969). This act of artistic deception involved more than a dazzling display of technical virtuosity; also vital to it was the relationship the painter forged between the illusion he presented and the person who observed it. Thinking of his canvas as a window through which he viewed the world, the artist sought to convey the illusion as reality, and to stimulate the observer's emotions and sense of possibility (Ackerman, 1969; Brucker, 1958). From "inside" the painting, the artist tantalized his observers with a seductive vista and pulled them toward a point or prospect that seemed attainable. At its most powerful, perspectival art induced the viewers into actually feeling a part of the painting, as though they could step into it and secure the prospect that beckoned them. The artist performed a feat of aesthetic manipulation even as he accomplished an act of pictorial veracity. He used his science to produce a material work of deception, an art.

In much the same way as the art of deception is a distinguishing characteristic of Renaissance perspectival painting, so it is a part of Machiavelli's strategic perspective in *The Prince*. Just as we can only appreciate the artist's act of aesthetic manipulation if we consider how the painting plays upon the observer's sensory impressions and "tactile values" (Berenson, 1909, p. 11), so we can confirm Machiavelli's act of political manipulation only if we consider how *The Prince* as text plays upon his reader's—Lorenzo de Medici's—values, desires, and sense of political possibility. Machiavelli will entice Lorenzo with a vision that will overwhelm his every other thought and distort his senses. With this in mind, we might

recall the moment in *Mandragola* where Machiavelli (1957, p. 25) offers a poetic comment on Ligurio's power and on the outrageous gullibility of old Nicia:

Our doctor here, would not suspect a lie
If he were told that jackasses can fly;
He has a heart so set on fatherhood,
That he's forgotten every other good.

Ligurio can deceive Nicia because of his strategic perspective. He knows how to play upon the old man's desires and to organize his field of choices by advancing some alternatives and concealing others. He makes the prospect of Nicia's fathering a child so palpable that this promise of living on comes to control Nicia's world, his sensibilities, and his every perception. Thus the old doctor is trapped; duped not only by Ligurio's wiles but by his own vanity, his grandiose expectations set the stage for his becoming the cuckold.

If we bring our own strategic perspective to *The Prince* and look to the relationship Machiavelli establishes between his text and its intended reader, we might see a political version of Ligurian deception at work. Machiavelli offers Lorenzo the promise of a different sort of fatherhood—the fathering of the state of Italy. Machiavelli's strategy is Ligurian—to promise greatness to the Medici lord and thus render him susceptible to the further flatteries that will, in fact, undo him. Nowhere in *The Prince* is the Ligurian strategy as evident as in the famous chapter 26. There Machiavelli paints the prospect of the prince as saviour of Italy, leader of his people, unifier of the fractious city-states, forever immortalized by his power and glory. The passion and spirit of these Machiavellian declamations have long troubled many of his interpreters, who puzzle over the marked contrast between this final chapter and the cold calculation of the rest of *The Prince*. However, the troublesomeness of chapter 26 begins to recede if we remember that technical

precision and vivid imagination were no strangers to the Renaissance painter nor, for that matter to Ligurio as he set out to trap his prey. Accordingly, we might read Machiavelli's final call to action as the "bait," or, to return once more to perspectival art, as the "vista" he offers Lorenzo. If the chapter does its work, Lorenzo, like Nicia, will "forget every other good" and so become not only Machiavelli's puppet, but the dupe of his own grandiose expectations of earthly power and political immortality.

Thus, Machiavelli sets out to manipulate the dimensions of Lorenzo's world. After presenting a particular scope of possibilities in *The Prince*, he artfully narrows the field of choices so that, in the end, the prince will live, act, and arm himself in the manner that his advisor recommends. Thus Machiavelli performs a feat of crafty assault, even as he accomplishes an act of political veracity. Lorenzo will be lured into following his advisor's dangerous counsel even as he reads this indeed unprecedented work of *realpolitik*. The beauty of the deception, were it to work, lies in Lorenzo's belief that his acts follow from his own *virtù* and seem perfectly in keeping with his aim to maintain power in life and achieve glory after death, while in reality they work to restore the republic.

Postscript

As a text, *The Prince* succeeded in securing Machiavelli's future fame and in sealing his notoriety. As a trap, it secured nothing. From all we know, Lorenzo never even read it. The republic Machiavelli wanted so passionately to see revived in Florence did indeed come later, but by different means than he envisioned and for but three short years, after which the Medici were installed in the city again. How could Machiavelli's stratagem have come to naught? How could his trap have failed to spring as he had hoped?

The deceiver himself would have a wry and ready answer: *Fortuna*, that mysterious goddess who governs half our actions, thwarted his plans and fouled his chances for success. As the chronicler tells it, on the day Machiavelli presented *The Prince* at the palace, Lorenzo was also given a gift of greyhounds, an unfortunate circumstance indeed, for the Medici lord was more intrigued with his hounds than with princely governance (Barincou, 1961, pp. 76-78). Yet there is more to the turn of fortune's wheel than this. Despite the greyhounds, it is hard to imagine why Lorenzo, a suspicious prince, would have taken this former republican, this *manerino* of Soderini, into his confidence in the first place. Machiavelli's every attempt to appear to be other than he was in the end was no match for his unblemished reputation as a Florentine republican, and so *The Prince* remained unread and Machiavelli unsummoned, forced to return to the countryside, where he divided his days between the "ancient courts" and his favorite and altogether appropriate pastime—snaring thrushes with his bare hands (Ridolfi, 1963, p. 140).

His misfortune takes yet one more turn, however, and the story is well known. When the republic was restored in 1527, Machiavelli eagerly reapplied for his old job at the Second Chancery. The new republicans, however, were suspicious—at the least they viewed him as an untrustworthy opportunist, at the most as a pro-Medicean. They had, it seems, been more successfully duped by *The Prince* than Lorenzo himself, and they too refused to allow Machiavelli's return to political life. The irony is hard to miss: Machiavelli had, quite simply, outfoxed himself.

Whatever misfortune was dealt this master of deception, however, his designs in *The Prince* now seem clear. We need only to remember circumstances and recall what the chronicler reports on the occasion when the advice book was eclipsed by the hounds. Upon leaving the

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Medici palace, Machiavelli was said to have muttered that "though he was not a man to plot against princes, his little book would avenge him" (Barincou, 1961, p. 78). Far from avenging him, *The Prince* has for five centuries accused this Florentine patriot. His vindication is long overdue.

Notes

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1. In international relations, see Waltz (1954) and Gulick (1955); in comparative politics, see Bluhm (1965); in organization theory, see Pfeffer (1981); in political psychology, see Nardulli, Flemming, and Eisenstein (1984).

2. See, among others, Wolin (1960), Pocock (1975), Shumer (1979), Skinner (1981), Hulliung (1983), and Pitkin (1984).

3. Quotations from Machiavelli's works are cited in the text using the following abbreviations: *P*: *The Prince* (1950, pp. 3-98); *D*: *The Discourses* (1950, pp. 101-540).

4. In a related fashion, J.R. Hale (1961, p. 175) suggests that Machiavelli thought "regeneration can be best organized by a prince, but when he feels that civic virtue has been restored, he should retire." Yet surely Machiavelli would not think that a prince would simply "retire" after restoring civic virtue or, for that matter, be interested in restoring the only virtue Machiavelli considers "civic"—republicanism. He knows too much of political rulership and Renaissance Florence to expect that.

5. Machiavelli (*P*, p. 78) offers one exception to this armament rule: the prince who acquires a new state in addition to the old one where his troops are stationed should disarm the former. However, this exception does not apply to the Medici, who were in exile as private citizens before their return to Florence and in control of no "old" state.

6. Furthermore, circumstances did not change dramatically. A report on Lorenzo, made in 1515 by the Venetian ambassador, notes: "This Lorenzo has been made captain of the Florentines against their own laws. He has become ruler of Florence: he orders and is obeyed. . . . They used to cast lots; no longer . . . the majority of the Florentines have no taste for the power of the house of Medici" (Hale, 1977, p. 99).

7. Overall, Machiavelli's advice on fortresses is ambiguous and could be a short essay in itself. In *The Art of War*, Fabrizio never advises against for-

resses; in fact, his discussion centers not upon their usefulness but upon their size, their relation to surrounding ditches, the design of portculisses, and the like. Machiavelli (1965, pp. 183-94) seems to be suggesting that fortresses are both important and necessary. In *The Discourses*, however, he appears resolutely opposed to fortresses, deeming them both "unnecessary" and "injurious" to prince and republic alike. "Good armies," he writes, will suffice instead of citadels (*D*, p. 365). The fact remains, however, that Machiavelli argues against a *fortezza* in *The Prince*, and the advice, whatever else it is, is fully in keeping with Florentine republican sensibilities.

8. Machiavelli's thesis was tested dramatically in 1534, when Alessandro de Medici not only disarmed the Florentines, but also constructed a fortress. The *Fortezza da Basso* secured Alessandro from his subjects, but not from his cousin Lorenzino, who assassinated him in bed in 1537. More importantly, however, it seems Machiavelli's prediction was accurate—Florentine republicanism was never restored. Hale (1968, p. 502) notes that the *Fortezza da Basso* was "the visible evidence that freedom was in fetters."

9. Machiavelli's dismissal seems to have been motivated by at least three factors: (1) his special friendship with Gonfalonier Soderini (he was contemptuously known as Soderini's *mannerino*); (2) the hostility of the pro-Medicean aristocracy, who never ceased viewing Machiavelli as an enemy of their class and a supporter of the republican cause; and (3) his reputation as the organizer of the Florentine civilian militia that had fought the Medici and their Spanish allies at Prato (see Gilbert, 1984, pp. 172-74; Hale, 1961, pp. 134-35; and Ridolfi, 1963, pp. 130-32).

10. Although my argument bears some similarity to that of Leo Strauss, especially as expressed in his *Persecution and the Art of Writing* (1952), the differences seem to me much more important. Strauss (1952, p. 33) excepts Machiavelli's name in his list of persecuted writers, but surely Machiavelli was politically persecuted; we have the biographical evidence, and Machiavelli himself (circumspectly) expresses as much on a number of occasions (Machiavelli, *P*, p. 4; 1961, pp. 10, 104, 143-44). I suspect that Strauss did not include the Florentine in his list because of his commitment to reading Machiavelli as a teacher of evil and his writings as "immoral and irreligious" (Strauss, 1958, p. 12), a view that often blinds him to the historical context and the political and biographical circumstances we must bring to bear upon our understanding of Machiavelli's aims and intentions.

11. A number of scholars have drawn this conclusion on the basis of their reading of *The History of Florence* as so much Machiavellian dissembling (see Gilbert, 1977, pp. 82-92; Najemy, 1982, pp. 575-76; and Skinner, 1981, pp. 84-86).

12. Ridolfi (1963, p. 203) cites an epitaph Machia-

velli wrote for Soderini that gives graphic evidence of his opinion of the man who might have saved the republic but had not the *virtù* to do so:

The night Pier Soderini died
His soul went to the mouth of Hell;
And Pluto cried: Silly soul,
Why come to Hell?
Go to Limbo with the Infants.

13. Wolin (1960, pp. 21-22, 203-9) quite rightly characterizes similar attributes as part of the political theorist's vocation, Machiavelli's included. However, the tension between the role of "advisor" and the vocation of "theorist" is equally important, and though beyond the scope of this essay, deserves more concentrated attention.

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HUMAN DIGNITY, HUMAN RIGHTS, AND POLITICAL REGIMES

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It is often argued that internationally recognized human rights are common to all cultural traditions and adaptable to a great variety of social structures and political regimes. Such arguments confuse human rights with human dignity. All societies possess conceptions of human dignity, but the conception of human dignity underlying international human rights standards requires a particular type of "liberal" regime. This conclusion is reached through a comparison of the social structures of ideal type liberal, minimal, traditional, communist, corporatist and developmental regimes and their impact on autonomy, equality, privacy, social conflict, and the definition of societal membership.

The international human rights elaborated in the Universal Declaration of Human Rights and the International Human Rights Covenants often are held to be compatible with a great variety of political regimes. For example, Khushalani (1983, p. 404) argues that "the concept of human rights can be traced to the origin of the human race itself," and that "all the philosophies of our time" are committed to human rights. Likewise, Graefrath (1983, p. 6) argues that international human rights standards "can be adapted to any legal system" (cf. Buultjens, 1980; Gros Espiell, 1979; Marasinghe, 1984; Mojekwu, 1980; Pollis, 1982; Ruffin, 1982; Stackhouse, 1984; Wiarda, 1982). We argue, however, that international human rights standards are

based on a distinctive substantive conception of human dignity. They therefore require a particular type of "liberal" regime, which may be institutionalized in various forms, but only within a relatively narrow range of variation. The authors cited above confuse human rights with human dignity.

"Human dignity" figures prominently in international human rights documents; for example, the International Human Rights Covenants proclaim that human rights "derive from the inherent dignity of the human person" (1966). Furthermore, every form of political regime implicitly reflects a particular social conception of human dignity. Nonetheless, human rights and human dignity are quite distinct notions.

Conceptions of human dignity, in their social and political aspects, express particular understandings of the inner (moral) nature and worth of the human person and his or her proper (political) relations with society. Human rights, by contrast, are the equal and inalienable rights, in the strong sense of entitlements that ground particularly powerful claims against the state, that each person has simply as a human being. Human rights are a particular social practice that aims to realize a distinctive substantive conception of human dignity (Donnelly, 1982a). Conceptions of human dignity vary dramatically across societies, and most of these variations are incompatible with the values of equality and autonomy that underlie human rights. Most regimes—and their underlying social conceptions of human dignity—necessarily deny both the idea and the practice of human rights.

In order to examine the relations between human rights and conceptions of human dignity across a wide range of regimes, our analysis relies heavily on the use of ideal types—"the construction of certain elements of reality into a logically precise conception" (Weber, 1946, p. 59)—especially ideal-type conceptions of the human person and his or her obligations to and claims upon society and the state. We first specify the philosophical and structural connections between the "liberal" conception of human dignity and the principle and practice of human rights. Then we show how four major contemporary regime types, which we call *communitarian*, necessarily repudiate human rights because of their commitment to alternative social conceptions of human dignity.

The particular interpretation of liberalism we adopt provides the philosophical and structural basis for international human rights norms. This is not a paper on liberal theory. In another context we would argue for the authenticity of our interpretation, but here we claim only that

it is a plausible, standard reading of the liberal tradition. Our subject in this article is human rights, not liberalism. Therefore, even if our definition should prove to be stipulative, the substance of our argument, which focuses on the social and political requirements of human rights, would remain largely unaffected.

We should also note that we do not join arguments about the content of lists of human rights. Instead, as is common in the human rights literature, we accept the list in the Universal Declaration of Human Rights without argument. In particular, we avoid rehashing old arguments about economic and social rights. Both on theoretical grounds (see Donnelly, 1982b; Donnelly, 1985, ch. 6; Shue, 1980, pt. 1) and in light of the nearly universal official acceptance of the Universal Declaration, we adopt the full list of rights it provides, with civil, political, economic, and social rights on an equal footing.

While these two simplifying assumptions narrow our focus, our argument remains significant and controversial. We contend that internationally recognized human rights require a liberal regime. Other types of regimes, and the conceptions of human dignity on which they rest, may be defensible on other moral or political grounds, but they will not stand up to scrutiny under the standard of human rights.

Liberalism and Human Rights: A Necessary Connection

Liberalism, Equality, and Personal Autonomy

We follow Ronald Dworkin (1977, ch. 12; 1985, ch. 8) in arguing that the heart of liberalism is expressed in the basic political right to equal concern and respect:

Government must treat those whom it governs with concern, that is, as human beings who are capable of suffering and frustration, and with respect, that is, as human beings who are capable

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of forming and acting on intelligent conceptions of how their lives should be lived. Government must not only treat people with concern and respect, but with equal concern and respect. It must not distribute goods or opportunities unequally on the ground that some citizens are entitled to more because they are worthy of more concern. It must not constrain liberty on the ground that one citizen's conception of the good life . . . is nobler or superior to another's. (Dworkin, 1977, p. 273)

The state must treat each person as a moral and political equal—not assure each an equal share of social resources, but treat all with equal concern and respect. Inequalities in goods or opportunities that arise directly or indirectly from political decisions—and many such inequalities are easily justified within a liberal regime—must be compatible with the right to equal concern and respect.

Personal liberty, especially the liberty to choose and lead one's own life, clearly is entailed by the principle of equal respect: for the state to interfere in matters of personal morality would be to treat the life plans and values of some as superior to others. A certain amount of economic liberty is also required, at least to the extent that decisions concerning consumption, investment, and risk reflect free decisions based on personal values that arise from autonomously chosen conceptions of the good life. Liberty alone, however, cannot serve as the overriding value of social life, as the end to be maximized by political association.

Liberty readily degenerates into license and social atomization unless checked by a fairly expansive, positive conception of the persons in relation to whom it is exercised. If liberty is to foster dignity, it must be exercised within the constraints of the principle of equal concern and respect. In fact, autonomy and equality are less a pair of guiding principles than different manifestations of the central liberal commitment to the equal worth and dignity of each and every person.

Each human being is of equal moral worth individually, whatever his or her

social utility. Individuals—regardless of who they are or where they stand—have an inherent dignity and moral worth that the state must not merely passively respect, but for which it must demonstrate an active concern. Furthermore, everyone is *entitled* to this equal concern and respect. Minimum standards of political treatment are embodied in (human) rights; they are not merely desirable goals of social policy. This implies a particular conception of the relation of the individual to the community and the state.

Man is a social animal; human potential, and even personal individuality, can be developed and expressed only in a social context. Society requires the discharge of certain political functions, and large-scale political organization requires the state. However, the state—especially the modern state—also presents particularly serious threats to human dignity. The state is easily turned to the denial of equal concern and respect, through the enforcement of a particular vision of the good life or the entrenchment of privileged inequality. Therefore, human rights have a special reference to the state, in order to keep it an instrument to realize, rather than undermine, equal concern and respect. In the inevitable conflicts between the individual and the state, the liberal gives *prima facie* priority, in the areas protected by human rights, to the individual.

For the liberal, the individual is not merely separable from the community and social roles, but specially valued precisely as a distinctive, discrete individual—which is why each person must be treated with equal concern and respect. The state and society are conceived, in more or less contractarian terms, as forms of association for the fuller unfolding of human potential, through the exercise and enjoyment of human rights. Human dignity, for the liberal, is largely encompassed in the vision of life as an equal and autonomous member of society, enjoying a full range of human rights.

This liberal view of man is rooted in structural changes that began to emerge in late medieval and early modern Europe, gained particular force in the eighteenth and nineteenth centuries, and today are increasingly the norm throughout the world. The "creation" of the private individual separate from society is closely linked to the rise of a new, more complex division of labor, the resulting changes in class structure (particularly the rise and then dominance of the bourgeoisie), and a new vision of the individual's relationship to God, society, and the state.

These developments are well known, and need not be recounted here. For our purposes, though, it is important to stress that in the social changes of modernization—especially migration, urbanization, and technological development—the all-encompassing moral whole of traditional or feudal society was replaced by a much more segmented social order. In particular, politics was separated from religion, the economy, and law, which were likewise separated from one another. Individuals, too, were separated from society as a whole; no longer could persons be reduced to their roles, to parts of the community. With separate individuals, possessing special worth and dignity precisely as individuals, the basis for human rights was established.

Occurring parallel to these changes in society was the equally well-known development of the modern state. The new bourgeois class was initially a principal backer of the newly rising princes and kings and their states; both shared an interest in freeing themselves from the constraints of the old feudal order. However, as the modern state's power grew, it increasingly threatened the individual citizen. Bourgeois "freemen" began to demand, therefore, that they indeed be free. Such demands eventually took the form of arguments for the universal natural rights and equality of all men. In the new socially mobile society,

in which entrance to and exit from the bourgeois class was relatively unpredictable, a new set of privileges could not readily be reserved for a new elite defined by birth or some similar characteristic. Rather, in order that some (the bourgeoisie) might be able to exercise these new rights, they had to be guaranteed for all.

Thus, human rights came to be articulated primarily as claims of any individual against the state. Human rights lay down the basic form of the relationship between the (new, modern) individual and the (new, modern) state, a relationship based on the *prima facie* priority of the individual over the state in those areas protected by human rights. Human rights are viewed as (morally) prior to and above society and the state, and under the control of individuals, who hold them and may exercise them against the state in extreme cases. This reflects not only the equality of all individuals, but also their autonomy—their right to have and pursue interests and goals different from those of the state or its rulers. In the areas and endeavors protected by human rights, the individual is "king," or rather, an equal and autonomous person entitled to equal concern and respect.

In practice, of course, these values and structural changes remain incompletely realized even today, and for most of the modern era they have been restricted to a small segment of the population. Nonetheless, the ideal was established and its implementation begun. Even if the demand for human rights began as a tactic of the bourgeoisie to protect its own class interests, the logic of universal, inalienable personal rights has long since broken free of these origins.

Furthermore, while these processes of sociopolitical individuation and state building were first played out in Europe, they are increasingly the rule throughout the world. As a result, the structural basis for a society of equal and autonomous in-

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dividuals is being universalized. Social structure today increasingly parallels the near universal diffusion of the idea of human rights and the philosophical claim that human rights are universal. Individual human rights, therefore, increasingly appear not merely as moral ideals, but as both objectively and subjectively necessary to protect and realize human dignity (cf. Howard, 1986).

Liberalism and International Human Rights

The standard list of human rights in the Universal Declaration of Human Rights can be easily derived from the liberal conception of the individual and the state. Other lists, of course, have been and may be derived from these principles, but we would argue that the near perfect fit between liberalism and the Universal Declaration reflects a deep and essential theoretical connection.

To be treated with concern and respect, whether equal or unequal, requires prior recognition of the individual as a moral and legal person, which in turn requires certain basic personal rights. Rights to recognition before the law and to nationality (Universal Declaration, Articles 6, 15) are prerequisites to political treatment as a person. In a somewhat different vein, the right to life, as well as rights to protection against slavery, torture, and other inhuman or degrading treatment (Articles 3, 4, 5) are essential to recognition and respect as a person.

Rights such as freedoms of speech, conscience, religion, and association (Articles 18, 19) protect a sphere of personal autonomy. The right to privacy (Article 12) even more explicitly aims to guarantee the capacity to realize personal visions of a life worthy of a human being. Personal autonomy also requires economic and social rights, such as the right to education (Article 26), which provides the intellectual resources for informed autonomous choices and the skills needed to act

on them, and the right to participate in the cultural life of the community (Article 27), which recognizes the social and cultural dimensions of personal development. In its political dimension, equal respect also implies democratic control of the state, and thus rights to vote and to freedoms of (political) speech, press, assembly, and association (Articles 19, 20, 21).

The principle of equal concern and respect also requires the government to intervene to reduce social and economic inequalities that deny equal personal worth. The state must actively intervene to protect those who, as a result of natural or voluntary membership in an unpopular group, are subject to social, political, or economic discrimination that limits their access to a fair share of social resources or opportunities. Rights such as equal protection of the laws and protection against discrimination on such bases as race, color, sex, language, religion, opinion, origin, property, birth, or status (Articles 2, 7) are essential to assure that all people are treated as fully and equally human.

In the economic sphere, the traditional liberal attachment to the market is not accidental: quite aside from its economic efficiency, the market places minimal restraints on economic liberty, and thus maximizes personal autonomy. However, market distribution of resources can have grossly unequal outcomes. Inequality *per se* is not objectionable to the liberal, but the principle of equal concern and respect does imply a floor of basic economic welfare; degrading inequalities (Shue, 1980, pp. 119-23) cannot be permitted. The state also has an appropriate interest in redressing market-generated inequalities because a "free market" system of distributing resources is actively backed by the state, which protects and enforces property rights.

Differential market rewards are not neutral; they reward morally equal individuals unequally. Market distributions

may be substantially affected by such morally irrelevant factors as race, sex, class, or religion, while many of the "talents" richly rewarded by the market are of dubious moral significance. Even "achieved" inequalities, should they threaten the (moral) equality or autonomy of other citizens, present at least a *prima facie* case for state intervention. The principle of equal concern and respect requires the state to act positively to cancel unjustifiable market inequalities, at least to the point that all are assured a minimum share of resources through the implementation of social and economic rights. In human rights terms, this implies, for example, rights to food, health care, and social insurance (Articles 22, 25).

Efforts to alleviate degrading or disrespectful misery and deprivation do not exhaust the scope of the economic demands of the principle of equal concern and respect. The right to work (Article 23), which is essentially a right to economic participation, is of especially great importance. It has considerable intrinsic value: work is essential to a life of dignity, insofar as man is conceived as *homo faber*. It also has great instrumental value, both for the satisfaction of basic material needs and for providing a secure and dignified economic foundation from which to pursue personal values and objectives. A (limited) right to property (Article 17) can be justified in similar terms.

Finally, the special threat to personal autonomy and equality presented by the modern state requires a set of legal rights such as the presumption of innocence and rights to due process, fair and public hearings before an independent tribunal, and protection from arbitrary arrest, detention, or exile (Articles 8-11). More broadly, the special threat to dignity posed by the state is reflected in the fact that all human rights are held particularly against the state. Moreover, they hold against all types of states, democratic as

much as any other: if one's government treats one as less than fully human, it matters little, if at all, how that government came to power. The individual of course has social duties (Article 29), but the discharge of social obligations is not a precondition for having or exercising human rights.

We have thus moved from the liberal principle of equal concern and respect to the full list of human rights in the Universal Declaration. These rights, in turn, demand—and if implemented would play a crucial role in creating—a liberal society, and the ideal person envisioned by liberalism (cf. Donnelly, 1985, ch. 3). It would be equally simple to work back from the Universal Declaration to the principle of equal concern and respect. In fact, the association between liberalism and human rights runs so deep that the realization of human rights is the principal liberal standard for evaluating the achievements, and even the legitimacy, of any regime (cf. Donnelly, 1985, pp. 69-73).

Liberalism vs. the Minimal State

In practice, obviously, even the best of actual liberal regimes fall short of the ideal we have been discussing, and the human rights records of many self-professed liberal societies merit severe criticism. Furthermore, many avowed liberals view liberty and equality as largely antagonistic principles to be traded off against one another, rather than as complementary dimensions of the single principle of equal concern and respect. One way to make this tradeoff is to choose liberty and disregard equality, establishing a "minimal" or "nightwatchman" regime.

Advocates of the minimal state (e.g., Nozick, 1974) would largely limit the state to protecting public order and private property. To assure the good behavior of the nightwatchman, "nega-

tive" civil and political rights are also required, especially civil liberties, narrowly conceived as rights to public noninterference in the private lives (very broadly understood) of individuals. Yet minimalism also explicitly protects property rights while rejecting economic and social human rights. Beyond minimalism's obvious incompatibility with international human rights standards (which minimalists readily allow), its deep commitment to protecting private property while denying all other economic and social rights borders on logical contradiction; we can see no way that precisely and only this one economic right can justifiably be allowed on the minimalist's list of human rights.

The standard rights-based (i.e., not merely utilitarian) arguments for the right to private property in such contexts rest on the importance of guaranteed private economic activity and resources for the enjoyment of personal autonomy. Clearly, however, such an argument does not justify a right to unlimited individual accumulation: at a certain point, additional economic resources contribute nothing at all to personal autonomy, and long before that point the marginal return becomes vanishingly small. Even more importantly, exactly the same argument can be made for other social and economic rights. In fact, a substantially stronger case can be made for rights to work, a minimum standard of living, and health care.

In any case, the minimal state is almost certain to be self-destructing if it recognizes equal, universal civil and political rights. The denial of political participation usually rests on a desire to protect social and economic privilege, while those previously excluded from political participation tend to use their newly acquired power to obtain a fair, or at least a tolerable, share of social resources (cf. Goldstein, 1983). The emergence of the Western welfare state and popular pressure throughout the Third World for

social services clearly suggests that implementing equal, universal political rights will transform a minimal regime.

The only way to avoid this would be to entrench a right to private property against the exercise of all other human rights. This is obviously unjustifiable; no plausible theory of human nature or dignity yields this one right as superior to all other human rights. However, lesser entrenchment, allowing redistribution beyond a certain level of accumulation, would be ineffective. If the point beyond which redistribution would take place were set democratically, a minimal regime would almost certainly be democratically abolished, or at least dismantled over time. Any other way of setting the limit, however, would deny the equality of political rights.

In other words, the minimal state, in its very essence, is a violator of human rights, even within the limits of its own terms of reference. Liberalism's dual pursuit of autonomy and equality is replaced in minimalism by a single-minded pursuit of autonomy understood largely as the social guarantee of the broadest possible sphere of private action, virtually irrespective of its social consequences. For the minimalist, human dignity is expressed principally in the unequal, achieved consequences of private, largely conflictual, action.

The minimal state thus is not the pure form of liberalism it is often represented to be by both minimalists and various leftist critics of liberalism. Rather, it is a perverse and internally inconsistent narrowing of liberalism that is also inconsistent with international human rights standards.

Equality, Autonomy and Communitarian Societies

Having shown that human rights and liberal regimes are closely matched, it re-

mains to be shown that other types of regimes are incompatible with the demands of human rights, a task that we have begun with the discussion above of the minimal state. In this section we examine four major types of communitarian society, which together encompass the vast majority of contemporary nonliberal regimes.

We define *communitarian societies* as those that give ideological and practical priority to the community (sometimes embodied in the state) over the individual. Such societies regard their members as worthy of concern and respect, but only as members of society performing prescribed roles. Their concepts of human dignity, therefore, are not rooted in the notion of human rights. Communitarian societies are antithetical to the implementation and maintenance of human rights, because they deny the autonomy of the individual, the irreducible moral equality of all individuals, and the possibility of conflict between the community's interests and the legitimate interests of any individual.

Traditional Societies

Traditional societies are communal, status-based societies, governed according to principles and practices held to be fixed by tradition. They are usually ethnically homogeneous and agricultural, and frequently stateless. In traditional society, one's worth, rights, and responsibilities arise from and remain tied to differential membership in a particular society, with unequal, status-based privileges and duties resting on age, sex, caste, or other ascriptive hierarchies. The idea that one is entitled to equal concern and respect and a wide range of inalienable personal rights simply because one is a human being is utterly foreign to traditional societies. Only certain kinds of people are defined as moral persons, that is, human beings.

Although most people in traditional societies have at least some rights and privileges, these are contingent on the proper fulfillment of social roles, rather than basic personal rights held against society. Even within the recognized social boundaries, some people may be defined as outsiders, as nonbelievers are defined in strict Islamic societies, or ethnic strangers in traditional Africa. The relationship between the individual (if he or she may be so called) and society is by definition nonconflictual; everyone's interests are incorporated into the higher value system represented by the political-religious-legal decision makers. Man and society are assumed to be inseparable. The very idea of inalienable individual rights held equally by all against the community is, if comprehensible, likely to be viewed with horror (cf. Legesse, 1980, p. 124).

In traditional societies, there is no notion of the autonomous individual. One's worth, even one's existence, is defined by one's place, one's role in the community; apart from the community, one does not exist, or at least such an existence is largely without moral value. One's dignity—which usually is conceived primarily as an attribute of one's kinship, age, sex, or occupational group—is obtained or validated by discharging the (traditionally defined) duties of one's station, rather than by autonomously creating or unfolding a unique individual existence. In traditional society, there are neither human beings, in the relevant moral sense, nor equal, inalienable, and universal rights.

Many traditional societies were slave or caste societies; few were subject to democratic control in even a very loose sense of that term. Individual deviations from communal norms usually were harshly repressed, and women and outsiders usually were treated as inferior beings. Nonetheless, in theory at least, and often in practice as well, a certain sort of dig-

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nity, tied to the proper fulfillment of social roles, could be achieved by most people. Furthermore, social responsibilities usually were coupled with reciprocal social and economic protections. In traditional societies most people had a defined, secure, and (within its own terms) dignified place in society.

Therefore, one can at least understand, and perhaps even contextually justify, traditional society's denial of human rights. One might even argue that the traditional conception of human dignity is superior to that of liberalism. For example, it might be argued that most people prefer regulated, secure social roles, with their concomitant sense of belonging, to autonomy and its attendant insecurities. Insider "individuals" may well have fared better in many ways as part of traditional society than as, say, a textile worker in mid-nineteenth century England or South Korea today.

Such arguments, however, are not human rights arguments. To defend traditional society is to reject a society based on equal, inalienable, universal personal rights in favor of a status-based society. To prefer traditional society to liberalism is to reject a society of equal and autonomous individuals with inalienable personal rights in favor of a society of unequal, regulated occupants of social roles, incorporated into the community. Traditional society and human rights cannot be combined without violence to both (cf. Donnelly, 1982a and Howard, 1986, ch. 2).

Communism

By *communism* we mean an ideal type regime modeled on the structure and official ideology of contemporary Soviet bloc countries. The key feature of such societies is a communist party-state committed to total, revolutionary transformation of social and personal life. The connection between such regimes and the writings of Marx or the "authentic" Marx-

ist tradition is an issue that cannot, and certainly need not, be addressed here. Our concern instead is with the frequently encountered argument that communist regimes are entirely consistent with international human rights norms.

While there are striking similarities between traditional and communist societies, especially in the submergence of the individual to the community (state) and in the use of social (class) roles to define individual worth, there are no less striking differences. Traditional societies have at most a rudimentary state apparatus, whereas in communist societies the state is the central social institution, despite ideologically obligatory references to its withering away. Rather than the often more or less face-to-face relations of small traditional society, communist societies range in size from millions to over a billion people. Instead of a relatively simple division of labor, they have a complex industrial division of labor, and rather than an ethnically homogeneous community, often bound together by real or mythic kinship ties, most communist countries are multi-ethnic.

This alters the entire texture of social relations. The communist state simply cannot be the functional equivalent of the traditional community; it necessarily appears as a distant, separate institution, incapable of providing the social and psychological support of close-knit traditional communities. Therefore, being subsumed into the "community" is quite a different process in communist societies.

While in traditional society the individual is never fully differentiated from the group, in communist societies individuals have been thoroughly differentiated. The modern economy, with its complex division of labor and extensive role-segmentation, necessarily produces economically, and thus socially, distinct individuals, and state bureaucracies are structured to deal (only) with (anonymous or interchangeable) individuals.

The political task for communist regimes, therefore, is to reabsorb the individual into the (state) society. The primacy of the state/society therefore must be politically created—as is underscored by the very project of revolutionary social transformation. One must be *made* a part of the community in communist societies.

However, since the basic socioeconomic organization of life under communism continues to (re)produce differentiated individuals, the process of (re)incorporation must be constantly repeated. Undifferentiated economic reincorporation is impossible, however, and ideological reincorporation, no matter how hegemonic, is insufficient. Direct political coercion, therefore, is a feature of communist collectivism that generally is absent from traditional society (because of the effectiveness of other means of social control).

As the task of the state/party/proletariat is to transform all aspects of social existence, private life is not merely subject to public regulation, but must be made public, and regulated by the state, if the revolution is to succeed. Those who follow a bourgeois or otherwise reactionary road are entitled to neither respect nor concern; at best they are ignored, and more often they are actively repressed. As one East German scholar states, "there is no freedom for enemies of the people" (Klenner, 1984, p. 15), who are defined as social outsiders. Such a belief readily leads to the identification and repression of pariah social classes and, in extreme cases, class-based "genocide" directed against kulaks or similar class enemies (Kuper, 1981, pp. 99-100).

The ethnic homogeneity of traditional society is replaced by class homogenization. "Class position," however, means simply conformity to behavioral norms specified by the state. Equality, rather than a fundamental and inviolable moral fact, is reduced to mere social sameness. In communist societies, one is equal not

by birth or by nature, but only to the extent that one is essentially indistinguishable from one's fellow communist citizens, an embodiment of the new communist man. Communist societies thus produce a distinctive sort of homogenized, de-individualized person.

Communist societies obviously must violate a wide range of civil and political rights during the revolutionary transition, and necessarily, not merely as a matter of unfortunate excesses in practice. Even after communism is achieved, the denial of civil and political rights remains necessary to preserve the achievements of the revolution. The permanent denial of civil and political rights is required by the commitment to build society according to a particular substantive vision, for the exercise of personal autonomy and civil and political rights is almost certain to undermine that vision.

Furthermore, communist regimes, for all their achievements in providing economic and social goods and services, are fundamentally incompatible with economic and social *rights*. In communist societies, the possession and enjoyment of all rights are contingent on the discharge of social duties. For example, Article 59 of the Soviet Constitution (1977) states that "the exercise of rights and liberties is inseparable from the performance by citizens of their duties" (cf. Burlatsky, 1982; Egorov, 1979, p. 39). Thus, for example, access to higher education and desirable jobs is closely linked to political connections or behavior. Few rights of any sort are secure in such a regime, and no human rights, in the strong sense of equal and inalienable entitlements of all individuals, can be recognized.

It is important to stress the difference between having a human right and merely enjoying the substance of a right; between, for example, having food and having a right to food, or speaking freely and enjoying a right to free speech. In communist (and other communitarian) socie-

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ties, one may be guaranteed the *substance* of certain human rights (Shue, 1980, pp. 75-76)—that is, goods, services, and opportunities may be enjoyed. They are not, however, enjoyed *as rights*; those who conform receive certain benefits, but the state may freely bestow or withdraw these as it sees fit (cf. Donnelly, 1985, pp. 11-12, 52-53, 77-80). This is as true of economic and social rights as civil and political rights. One is not entitled to these benefits simply as a human being, one does not have the special control provided by possession of a right, and one's claims to enjoy these benefits do not have the force of human rights.

In communist regimes, in fact, even as a citizen one is entitled to nothing from the state: "Human rights . . . do not exist outside the state or against the state. The state is their creator" (Lopatka, 1979, p. 7; cf. Weichelt, 1979, p. 3). Rights are acquired only by the discharge of class obligations, as defined by the state (Lieberman, 1979, p. 14). Social outsiders, such as landowners or the bourgeoisie, may lose not only their former property rights, but also all other rights.

Communist society thus rests on a social utilitarianism fundamentally incompatible with human rights. The good of society, as determined by the state/party, always takes precedence over all else. Because individual "rights" must always yield to social purposes, as enunciated by the state, such "rights" are worthless; no matter what the state does, it cannot be held guilty of violating them. Whatever the benefits and opportunities citizens may (contingently) receive—and they are undeniably substantial in some communist regimes—communism represents a thorough denial of human rights.

Corporatism

Corporatism, a principal form of contemporary right-wing regimes, can be defined as

a system of interest representation in which the constituent units are organized into a limited number of single, compulsory, non-competitive, hierarchically ordered and functionally differentiated categories, recognized or licensed (if not created) by the state and granted a deliberate representational monopoly within their respective categories in exchange for observing certain controls on their selection of leaders and articulation of demands and support. (Schmitter, 1974, pp. 93-94)

Corporatist regimes present themselves as neutral instruments to regulate and mediate the antithetical interests of labor and capital, with other groups—such as women and youth—often officially organized and incorporated into the political structure as well, further undercutting basic structural conflicts. In practice, however, class rule is unambiguously at the heart of corporatism. The essential purpose of its ideology and political structure is to prevent further class conflict and entrench the extant economic hierarchy.

The state proclaims the equal dignity of all segments of society. Meanwhile, unequal private power and property accumulate. Workers and peasants are not necessarily excluded from a share of social benefits—for example, state controlled trade unions may be allowed to pursue certain improvements in working conditions or living standards, so long as class conflicts are denied—but they benefit only inadvertently or as a side-payment to co-opt potential opponents of the ruling corporate coalition. Equal concern and respect is at best ignored.

One variant of corporatism, which can be called *authoritarianism* for want of a better term, preserves an important sphere of private autonomy and activity. Religion and education, for example, may be left as a private matter. This privacy, however, is only a realm of public indifference. It is quite different from positive respect for or protection of a right to privacy and related human rights. Privacy (of thought, religion, belief) is not so much protected in authoritarian regimes as it is ignored—and it is ignored only as

long as it does not interfere with the basic corporatist bargain. Personal autonomy is, at best, not a right but a contingent benefit.

The *fascist* variant of corporatism, however, is actively hostile to the private. In reaction to what it views as the unabated individualism of liberal society, fascism proclaims a romantic ideology of (mythic) consensus, homogeneity, and personal comfort in conforming to social roles. Human dignity is to be achieved through integration into an all-encompassing moral order, represented by the fascist state. Much as in communism, any challenge to this order, including deviation in personal values and beliefs, is treated as a threat to the entire social fabric.

This ideology of the primacy of the state readily leads to terror and scapegoating. Nonviolent denials of civil and political right are likely to be inadequate to prevent independent "political" activity, now redefined to include much of "private" personal life. Direct terror is likely to be necessary; so also is the creation and persecution of outsider or scapegoat groups, in extreme cases culminating in genocide. Such persecution not only allows the state to displace real social tensions arising from the corporatist character of society, but, in the very denial of the rights of the scapegoat group, reaffirms the unity of the fascist individual, society, and state.

From a human rights perspective, however, fascism is merely the extreme form of corporatism; fascism may actively violate more rights, but authoritarian corporatism is unlikely to protect many more. One cannot even assuredly say that life is preferable for the average individual in authoritarian corporatism; for example, if public indifference results in anomie, the intense feeling of belonging espoused by fascism may seem preferable, at least for insiders. In any case, authoritarian corporatism's public indifference to

the bulk of society is certainly a denial of equal concern, while its denial of independent political action is incompatible with equal respect. Whatever the form, corporatism denies inherent personal dignity and equal concern and respect in the very bargain that defines the regime.

Development Dictatorship

One further type of communitarian regime, which we call *development dictatorship*, should be briefly noted. In development dictatorships, the principal resource of the ruling elite is control of the means of coercion, justified in the name of the most rapid possible economic development. Development, which has achieved an unprecedented ideological hegemony in the Third World, is easily presented as the moral equivalent of war, requiring the subordination of the individual to the state. Therefore, in the hands of repressive elites it nicely justifies a wide range of human rights violations, especially since the connection between particular violations and underlying development goals is likely to be at best very loosely defined.

Development dictatorship is distinguished from corporatism or communism in large measure by its class structure. In development dictatorships, economic class position is less the source of power than the result of control of the state. In nationalized economies, the organizational (Markovitz, 1977, ch. 6) or bureaucratic (Shivji, 1976, pt. 3) "bourgeoisie" is composed of occupants of high-level office in the military, the government, the bureaucracy, or the ruling party. A parasitic private bourgeoisie, essentially living off its economic relations with the state, may also exist. Controlled by members of these various elites, who have few resources other than coercion to maintain their power, development dictatorships frequently degenerate in cycles of coups and countercoups, or, once stabil-

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ized, evolve into some sort of corporatist regime. The regime rests lightly on top of society, rather than being a political representation of deep underlying socio-economic forces (cf. Hyden, 1983, ch. 2).

Even where the regime's commitment to development is genuine, rather than merely a cover for elite depredations—so that there is an attempt to provide the substance of some economic and social rights—enshrining development as an overriding social objective assures that individuals and their rights will regularly be ignored. The value put on privacy tends to be low, as private goals might interfere with national development goals. The identification of "outsiders," economic saboteurs, or similar scapegoat groups is a common diversionary tactic when development plans fail. Full personal dignity is conceived largely as an abstract future good, to be realized only after success in the struggle for development. In general, individual human rights, especially rights against the state, the essential agent of development, must wait until development has been achieved. Once more, we are faced with a choice between human rights and alternative social goals based on a radically different conception of human dignity.

Communitarianism and the Impossibility of Human Rights

Whether communitarianism is forward or backward looking, it is structurally, ideologically, and philosophically incompatible with human rights. The view of human dignity found in all communitarian societies is that the individual realizes himself as part of the group by unquestioningly filling his social role or being loyal to the state. This conception of human dignity is incompatible with human rights.

At the core of this incompatibility is the denial of social value to personal autonomy and privacy. In communitar-

ian societies the state (or traditional authorities), as the representative of society, must control family life, religion, education, and all other potentially independent aspects of life. Any institution that might influence or challenge the reigning regime and its ideology must be eradicated, or at least regulated; often one's very beliefs, and certainly all aspects of one's behavior, are treated as legitimate matters for social regulation.

When personal autonomy is thus denied—even repressed as a threat to society—moral equality must also be denied; some people—those who "fit in"—are treated as more worthy of concern or respect than others. The full range of international human rights must thereby be violated.

The rule of law and procedural due process are obviously incompatible with such regimes; pursuit of the community's substantive goals overrides "mere procedures." Due process is also rejected because it suggests that political organs representative of the full community might treat citizens unfairly, a possibility denied by the communitarian premise of the regime. Equal protection of the laws, and nondiscrimination more broadly, also are incompatible with communitarian regimes. In fact, positive discrimination against social deviants is essential to the political pursuit of unity; differences between individuals or groups (other than those that are officially sanctioned) are not to be protected—let alone valued as expressions of autonomy—but rather repressed, or at best ignored. In communitarian regimes, one is entitled to the protection of the laws and a guaranteed share of social resources and opportunities only to the extent that one fits within certain substantive, ideologically defined categories.

Political participation is similarly restricted, both in its substance and its participants. Debate over fundamental social and political aims cannot be al-

Table 1. Social Conceptions of Human Dignity: A Typology

Regime Type	Autonomy vs. Role Fulfillment	Conflict vs. Consensus	Repression of Outsiders	Valuation of Privacy	Equality vs. Hierarchy
Liberal	Autonomy	Conflict	No	High	Equality
Minimal	Autonomy	Conflict	No (?)	Very high	Hierarchy
Traditional	Roles	Consensus	Yes	Very low	Hierarchy
Communist	Roles	Consensus	Yes	Very low	Equality
Corporatist	Roles	Consensus	Yes	Low	Hierarchy
Developmental	Roles	Consensus	Yes (?)	Low	Equality

lowed, because they are already set by tradition or the reigning ideology. Likewise, politics is dominated by a small elite, chosen by ascription, restricted party membership, or other nondemocratic means—or, where the forms of democratic politics are utilized (e.g., communism), real control lies elsewhere (e.g., a vanguard party).

Many communitarian societies, however, do perform relatively well in providing the substance of economic rights. Many espouse, and some do achieve, relative equality of material circumstances and a basic floor of material security. But such economic "rights" are mere benefits, contingent on approved membership in the political community and on the performance of social duties. Citizens are not entitled to these goods and services; at most they may petition for them, not claim them as rights. Material security certainly is valuable, whether it is a right or a privilege, but such "security" is precariously insecure in the absence of human rights held against the state, since it can be taken away as easily as it is granted.

In sum, communitarian regimes fall short of the standard of human rights in all major areas. Much as liberalism is necessarily committed to protecting, implementing, and fostering the enjoyment of the full range of internationally recognized human rights, communitarian regimes necessarily violate the full range of human rights.

Human Dignity, Human Rights, and Political Regimes

We can pull together this discussion in a typology of social conceptions of human dignity. Table 1 lists the positions of our six types of regimes on five sociopolitical variables shown in the discussion above to be important to the social definition of human dignity. It is immediately apparent from the first four columns that these regimes fall into two broad classes, *individualistic* (liberal and minimal) regimes and *communitarian* (traditional, communist, corporatist, and developmental) regimes.

Not surprisingly, the first four variables are rather closely related. Society's attitude towards autonomy is especially important. A commitment to personal autonomy requires accepting a certain degree of social conflict, largely precludes enforcing the substantive models of belief and behavior that are the basis for the repression of outsiders, and leaves open a considerable realm of valued private activity. Likewise, a stress on role fulfillment implies a consensual society: roles are defined so as to produce consensus when properly performed; "outsiders" (those without approved or valued roles) are repressed; and privacy, which exists outside of predefined roles, is not socially valued.

There are, however, no less important differences within each of these two classes of regimes. Liberalism's commit-

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Table 2. Social Conceptions of Human Dignity and Human Rights Performance

Regime Type	Equality or Hierarchy	Valuation of Belonging	Civil and Political Rights	Economic and Social Rights
<i>Individualistic Regimes</i>				
Liberal	Equality	Moderate	Yes	Yes
Minimal	Hierarchy	Very low	Yes	No
<i>Communitarian Regimes</i>				
Traditional	Hierarchy	Very high	No	Substance only
Communist	Equality	High	No	Substance only
Corporatist	Hierarchy	Varies	No	No (?)
Developmental	Equality	Moderate (?)	No	Substance (?)

ment to autonomy is matched by a commitment to equality; human dignity, for the liberal, requires the union of autonomy and equality. This commitment to equality further strengthens the tendency not to repress outsiders, or even to define outsider groups other than noncitizens, who are ignored rather than repressed.

In contrast, those at the bottom of the minimal state's social hierarchy are denied economic and social rights, as a result of the absence of a commitment to equality and the presence of an extremely high valuation of privacy (especially private economic activity) under minimalism. Thus, they may be seen as indirectly oppressed economic outsiders, and if the lower classes attempt to challenge this denial of economic and social rights, direct repression is likely. Furthermore, whereas liberalism merely accepts a certain amount of social conflict as an unavoidable consequence of personal autonomy, and even tempers conflict by the pursuit of social and economic equality, minimalists tend to view social conflict in no worse than neutral terms, and even as desirable competition between unequal, atomistic individuals.

There are also important differences among communitarian regimes. For example, there are considerable differences in the substantive bases used for the definition of social membership and roles. The most important differences, though,

concern the valuation of equality and belonging (the obverse of privacy), as we can see in Table 2, which correlates the major determinants of social conceptions of human dignity with the human rights performance of each type of regime.

All communitarian regimes reject civil and political rights, which can be recognized only when individual autonomy is valued over role fulfillment. However, traditional and communist regimes, one hierarchical, the other egalitarian, do provide the substance of (at least some) economic and social rights (for insiders); that is, they provide goods, services, and opportunities, but without the power or control that comes with enjoying these benefits as rights. The value placed on equality then largely determines the range and distribution of these benefits. Communist regimes are committed to providing them equally, and in great and ever increasing quantity. Hierarchical traditional regimes, however, guarantee only a minimum floor for all (or at least all but chattel slaves, untouchables, and similar near-outsider groups).

Corporatist and developmental regimes—again, one hierarchical, the other egalitarian—do not generally offer even this much. The typical (authoritarian) corporatist regime protects only the interests of the ruling coalition (although fascist corporatism is likely to provide at least some economic and social

benefits to all insiders—thus the questionable “no” in Table 2). Developmental regimes are ideologically committed to providing the substance of economic and social rights for all (insiders), at least in the long run, but since the social composition of such regimes usually belies and precludes the realization of this commitment, it remains of at best questionable practical significance.

This suggests that at least as important a variable as equality or hierarchy is the valuation of belonging. Traditional and communist regimes highly value belonging, and thus provide the substance of many social and economic rights to all insiders, while corporatist and developmental regimes, which do not guarantee even the substance of economic and social rights, place lower value on a sense of belonging. This conclusion is also implied by the comparison of liberal and minimal regimes. The absence of economic and social rights in minimal regimes is explained not simply by the absence of a social commitment to equality, but also by the very low valuation of belonging.

Only when autonomy, equality, and at least a moderately high value on belonging are combined—as in liberalism—do we find a commitment to economic and social rights, and not just their substance. Only with a commitment to personal autonomy will a regime actively protect civil and political rights. In other words, only in a liberal regime can there be a fundamental political commitment to the full range of internationally recognized human rights.

Other social systems may claim to have competing views of human rights. They do not. Rather, they rest on competing views of human dignity, all of which deny both the centrality of the individual in political society and the (human) rights of men and women to make, and have enforced, equal and inalienable civil, political, economic, and social claims on the state. Only liberalism, understood as

a regime based on the political right to equal concern and respect, is a political system based on human rights.

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U.S. DEFENSE SPENDING AND THE SOVIET ESTIMATE

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There is a glaring and potentially important discontinuity surrounding discussions of U.S. defense expenditure policy making. On the one hand, a growing body of empirically-based research questions whether the U.S. reacts in any significant fashion to the military expenditures of the Soviet Union. On the other hand, defense policy makers routinely justify defense increases as a response to similar increases by the Soviet Union. The discontinuity is resolved in the context of a multistep model of the defense expenditure policy-making process that incorporates a new estimate of Soviet defense spending and mass public opinion. Once formulated, the model provides answers to the following two questions: (1) Does the U.S., insofar as defense spending is concerned, react to the military expenditures of the Soviet Union? (2) If so, what is the magnitude of the reaction? The answers indicate that not only does the U.S. react to estimated Soviet defense spending, but that the reaction is directly responsible for a very substantial portion of the post-1978 increases in U.S. military expenditures.

There is a glaring and potentially important discontinuity surrounding discussions of U.S. defense expenditure policy making. On the one hand, a growing body of empirically-based research questions whether the U.S. reacts in any significant fashion to the military expenditures of the Soviet Union. The thrust of recent findings is captured by Cusack and Ward's observation (1981, p. 448) that "[there is] . . . little compelling evidence that an 'arms race' embodies the primary dynamic underlying U.S. defense expenditures." Others who have been unable to demon-

strate significant reactivity of the U.S. to the USSR include Fischer and Kamlet (1984), Majeski (1983), Domke, Eichenberg, and Kelleher (1983), Freeman (1983), Majeski and Jones (1981), Fischer and Crecine (1981), and Nincic and Cusack (1979). The primary determinants of U.S. defense expenditures are usually traced to incrementalism and domestic influences (Moll and Luebbert, 1980).

On the other hand, defense policy makers routinely justify the increases in U.S. defense spending as a response to increases in the spending of the USSR. In an address to the United Nations, Presi-

dent Reagan provides a clear indication of the need to estimate the defense expenditure patterns of one's adversaries: "the amount and type of military spending by a country are important . . . as a measure of its intentions, and the threat that country may pose to its neighbors" ("Text of Reagan's UN Speech on Nuclear Arms Race," *New York Times*, June 18, 1982). According to Secretary of Defense Caspar Weinberger, insofar as the United States is concerned,

The most obvious and significant of these threats is the global challenge posed by the only nation that rivals us in military power—the Soviet Union. (Department of Defense—Annual Report for Fiscal Year 1982 [1981])

Explicitly stating the defense budgetary implications of the Soviet threat, Mr. Weinberger asserts:

You're making a terrible mistake if you try to adjust your defense budget to food stamps, harbor dredgings and highways. It's the threat that makes the budget. *You've got to build your budget on the Russian budget.* (quoted by White, February 6, 1983, emphasis added)

U.S. defense expenditure decisions are often justified as a necessary response to the spending decisions of the Soviet Union.

The debate over the primary stimulus for U.S. defense expenditure decisions is an important issue given the recent and dramatic changes that have occurred in U.S. defense spending. In 1968, at the peak of the Vietnam War, the U.S. defense expenditure total of \$77.3 billion was 45% of all federal spending and 9.5% of the gross national product (GNP). Over the next 10 years defense expenditures grew slowly to \$103 billion—23% of federal spending and 5% of the GNP. In the ensuing 6 years, the U.S. defense expenditure total has more than doubled to \$238 billion—28.1% of federal spending and 7% of the GNP. Substantial portions of the recent defense expenditure increases have been attributed by policy makers to the need to react to increases by the Soviet

Union. If there is no empirical evidence that the U.S. reacts to the USSR, serious questions arise concerning both the justifications and the current level of U.S. spending. It is important, therefore, that careful scrutiny be given to the following questions: (1) Does the U.S., insofar as defense spending is concerned, react to the military expenditures of the Soviet Union? (2) If so, what is the magnitude of the reaction?

There are at least two reasons for the previously discussed discontinuity between the research community and policy makers. First, even though a great deal of the research has focused on incrementalism and domestic factors, it has overlooked several political factors that are likely to impinge on the defense policy-making process. Specifically, many models ignore the sequential character of the policy-making process; the importance of the relative balance of U.S.—USSR spending (especially if there is a spending gap); and the pervasive impact that public opinion has on all stages in the defense expenditure policy-making process. In short, existing models have not been completely specified, and hence all *ceteris paribus* inferences must be questioned.

A second reason for the discontinuity lies in the fact that there are substantial differences in the way in which Soviet defense spending is measured. Little is known about Soviet defense spending since it is formulated in secrecy, and since the USSR only reports a single line entry for defense expenditures each year. The previously cited research makes use of estimates of Soviet defense spending that are derived directly from reported Soviet data. The best of these estimates, that produced by the Stockholm International Peace Research Institute (SIPRI), essentially takes what the Soviet Union reports in its official documents and converts it to dollars.¹ Since the Soviet Union reported only minimal change in military expendi-

tures during the 1972-1980 period, the SIPRI estimates remained virtually unchanged from 1972 to 1980, and have continued to the present to show that the U.S. is outspending the USSR. The relative lack of change in the SIPRI estimates of Soviet defense spending, coupled with the appearance of U.S. superiority, could be responsible for the "no reaction" finding.

U.S. policy makers have not accepted the reported Soviet defense expenditure figure at face value. As Kaufman (1985, p. 191) notes, "this figure is considered uninformative because its scope is not defined and changes in the announced figure do not correspond with changes in the observed level of military activities." To obtain an estimate of Soviet defense spending, the CIA has developed a building-block methodology to determine what it would cost in U.S. dollars to duplicate Soviet military activities.² In 1976, the CIA dramatically revised its estimate of USSR defense spending upward, based upon two assumptions: (1) the Soviet Union was willing to allocate 12-13% of its GNP to national defense, and (2) they [the CIA] had underestimated the costs to the Soviet Union of producing military hardware. By 1979 the CIA (1979, p. 3) concluded that increases in Soviet defense spending were so dramatic that the U.S. now lagged 45% behind the USSR. Consequently, the evidence used by the policy makers shows not only dramatic increases in the Soviet estimate, but also that the USSR is outspending the U.S. by a substantial amount.

To determine whether the U.S. does react to Soviet defense expenditures and, if so, to what extent, requires a three-pronged research program. It is first necessary to specify a model of the defense expenditure policy-making process that recognizes both the multistage character of the budgetary process and the fact that a Soviet estimate and public opinion are potentially relevant causative

factors. In this vein, we offer a model based upon a synthesis of the organizational process and bureaucratic politics perspectives. Once a model is specified, a measure of Soviet defense spending that could have been used by policy makers is formulated. The model, in conjunction with the measure of Soviet defense spending, is then evaluated empirically. Upon completing these three stages, the presence and magnitude of a U.S.-Soviet reactive linkage can be investigated.

Modeling the Defense Expenditure Decision

Any investigation of a U.S.-USSR reaction must take place in the context of a correctly specified model of the defense expenditure policy-making process. In the sections that follow, two distinct perspectives on the policy-making process—*organizational process* and *bureaucratic politics*—are presented and then synthesized. The *organizational process* perspective provides a foundation for the structure of the policy-making process, while the *bureaucratic politics* perspective introduces substance. Together the synthesis offers a plausible and coherent characterization of the U.S. defense expenditure policy-making process.

Organizational Process and the Structure of the Process

Each segment of the U.S. government concerned with the defense budget can be viewed as an organization. Hence, to explain U.S. defense expenditures, it is necessary to determine how organizations make decisions. The underlying structure of the process employed is based upon Hoole (1976), Ostrom (1978), and Marra (1985), and derived from behavioral organization theory.

From this perspective, five "design" principles are used to construct a model of

the defense expenditure policy-making process:

1. Organizations factor decisions into subproblems. Each organization deals with only a segment of the overall decision.
2. All other things being equal, organizations do not change. A change in behavior requires a stimulus.
3. An organization's initial cue is taken from the previous step in the policy-making process.
4. A limited set of environmental variables is monitored by the defense sector; consequently, much of the environment is ignored.
5. Each organization adopts decision rules that are simple, additive, and temporally stable.

Based upon these principles, the investigation will be carried out in terms of equations of the following form:

$$D_{it} = D_{i,t-1} + u^*; \quad (1)$$

$$(D_{it} - D_{i,t-1}) = u^*; \quad (1a)$$

$$u^* = a_{i1}(D_{i-1,t} - D_{i-1,t-1}) + v^*; \quad (2)$$

$$v^* = \sum a_{ij}E_{jt} + e^*; \quad (3)$$

$$e^*_{it} = \text{random behavior}; \quad (4)$$

where

D_{it} = organization i 's decision at time t ;

E_{jt} = environmental variables;

a_{i1} = interorganization connection;

a_{ij} = reaction coefficients for essential variables ($j \geq 2$).

Combining equations (1) through (4) into a single entity suggests the following general decision rule for actors in the U.S. defense expenditure policy-making process:

$$\Delta D_{it} = a_{i1}\Delta D_{i-1,t} + \sum a_{ij}E_{jt} + e^*_{it}, \quad (5)$$

where

ΔD_{it} = the change in the organization's decision ($D_{it} - D_{i,t-1}$).

Equations (1) through (5) constitute a model of the steps of the U.S. defense expenditure policy-making process based upon principles of behavioral organization theory. The subscript i in each equation denotes that multiple organizations are involved in this process. These organizations factor the decision into subproblems, and hence each only deals with a segment of the overall decision problem.

Equation (1) suggests that a decision at time t should be very similar to the decision made at time $t - 1$. This equation indicates that a decision maker searches in the vicinity of the previous decision for subsequent responses on his or her part. There is a subtle but important theoretical distinction underlying equation (1a): the focus is on the *change* in behavior from one time period to the next. Previous behavior is viewed as a *base* from which decision makers deviate only slightly. Given the complexity of the behavior under examination and the cognitive limitations of decision makers, it is highly unlikely that the budget decisions of any of the organizations are rebuilt from zero each year. Instead, the implication of equation (1a) is that each organization accepts its own prior decision and focuses on changes in behavior from one time period to the next.

Equation (2) highlights the fact that organizations not only factor the problem into parts, but use an organizational decision that is temporally prior to their own decision as their base. By doing so, each organization is able to cut down on the time and organizational resources required to make its decision. This suggests that yearly changes are based in part on the behavior of the previous step in the policy-making process. The parameter, a_{i1} , provides an indication of the magnitude of this interorganizational connection.

Equation (3) states that deviations from

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the previous step can be attributed to a set of environmental variables. From the fourth design principle it follows that this is a finite set. The set of environmental variables varies with respect to the goals of the organization, and hence may differ from step to step. The composition of these subsets will be examined in the next section. Any remaining behavioral variation will be viewed as random—equation (4). Finally, equation (5) reflects the notion that the decision rules adopted by each organization in the defense expenditure policy-making process are simple, additive, and temporally stable. The observed changes can be viewed as a response to the previous organizational step and to factors in the external environment.

Bureaucratic Politics and the Substance of the Process

Formal models of the defense expenditure policy-making process have often ignored the bureaucratic politics perspective. According to Allison (1971, p. 173), the bureaucratic politics explanation is achieved by "displaying the game—the action-channel, the positions, the players, their preferences, and the pulling and hauling—that yielded, as a resultant, the action in question." As elaborated by Allison and Halperin (1972), the bureaucratic politics explanation, which focuses on intranational politics to explain national behavior, identifies three central questions:

1. Who plays?
2. What determines each player's stand?
3. How are players' stands aggregated to yield governmental decisions and actions?

Each question is important to an explanation of defense policy making.

To answer question (1), the present inquiry focuses on three organizational actors: the president, Congress, and the Department of Defense (DoD). These organizations are important because they make the three primary decisions that are necessary to produce a defense expenditure total: the president's budget request, the congressional appropriation, and the DoD's expenditure decision. In addition, these organizations represent the primary participants in the intranational political process. Accounting for the outcome of this political process provides the explanation for the defense expenditure decision as the resultant of three separate, though interconnected, decisions.

The answer to question (2) provides the necessary guidance to elaborate and specify the environmental variables in equation (3) for each organization, and thereby to specify completely equation (5) for each organization. The bureaucratic actors "choose in terms of no consistent set of strategic objectives, but rather according to various conceptions of national security, organizational, domestic, and personal interests" (Allison and Halperin, 1972, p. 43). Consequently, each player's stand is determined by different interests, and each organization is likely "to see quite *different faces of the issue*" (Allison and Halperin, 1972, p. 48). The face of the issue for each organization is determined by the answers organizational policy makers give to questions related to the following three dimensions of U.S. defense expenditure policy making:

- a. How much can we afford?
- b. How much is enough?
- c. How much does the public want?

Answers to these three questions connect the policy maker's decisions to the external environment.

Insofar as question (a) is concerned, the defense expenditure decision rests squarely in the context of what has been called

the Great Equation (Fischer and Kamlet, 1984):

$$\begin{array}{l} \text{DEFENSE} \\ \text{EXPENDITURES} \end{array} + \begin{array}{l} \text{NONDEFENSE} \\ \text{EXPENDITURES} \end{array} \\ = \text{REVENUES} + \text{DEFICIT}$$

Each defense expenditure decision has implications for at least three other basic decisions: the level of nondefense spending, anticipated revenues, and the deficit/surplus. As Fischer and Kamlet (1984, p. 357) note, "each of the four terms . . . is politically or economically significant in its own right." Insofar as the defense/nondefense tradeoff (i.e., guns vs. butter) is concerned, the opportunity costs in each domain are intimately connected to estimated revenues and the deficit. Increased revenues and/or a surplus increase the opportunities for additional defense spending, while decreased revenues and/or a deficit decrease such opportunities.

With respect to question (b), the primary focus turns to a determination of how much spending is necessary to maintain a sufficient level of defense. Throughout the postwar period, it seems clear that the U.S. has focused on a very consistent set of foreign policy goals: a preference for action, anticommunism, and containment of the Soviet Union (Kegley and Wittkopf, 1982, p. 36; Ostrom and Job, 1986). To be able to act to repel communist aggression and/or contain the Soviet Union requires a substantial commitment to national defense. Hence, these goals provide a rationale both for monitoring and reacting to Soviet defense expenditures. Therefore, the answer to "How much is enough?" depends to a very great extent upon how much the Soviet Union is spending.

Since the president and all members of Congress are generally elected, it is possible that mass political attitudes can have an impact on defense decision making. To the extent that the public has an opinion about defense spending, the answer to

question (c) will indicate how responsive each actor is to mass political attitudes. Russett (1972, p. 301) introduces two propositions concerning the impact of public opinion on defense spending. First, military expenditure decisions are affected by public opinion. Second, public opinion typically restricts military expenditure decisions. Even though this research suggests that negative public opinion is the primary opinion input, positive opinion may, in some circumstances, lead to increases in defense spending.

The fact that there are multiple actors involved who have different conceptions of how much the U.S. can afford, how much is enough, and how much the public wants suggests that the output of the defense expenditure policy-making process will be a *political resultant*. It is a *resultant* to the extent that no single organizational actor is in complete control of the final outcome. It is *political* because the individual organizations see various sides of the issues and respond differently to the three dimensions discussed above.

Turning to question (3), the organizational design principles lead to decision rules, in the form of equation (5), and thereby provide a clear means for determining how the individual organizational decisions are combined to achieve the final decision. It is possible, because of the interorganizational connection, to aggregate the three decisions to explain the year-to-year changes in U.S. military expenditures.

A Mediated Reactive Linkage Explanation

A synthesis of the two approaches provides structure and substance to our model of defense expenditure policy making. The explanation is achieved by locating both the interorganizational and environmental connections that influence the decisions of each organization, and by highlighting the implications that flow

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from the sequential nature of the policy-making process.

Interorganizational Connection. The interorganizational connection for each organization's decision is hypothesized to be an important determinant of defense policy making. It is, in effect, the base from which changes from the previous behavior begin. We hypothesize that the president's budget is tied directly to an estimate of Soviet defense spending generated within the U.S. government. This connection seems plausible in light of the "lessons" of Munich and of the anti-communist/containment rhetoric that has been pervasive throughout the postwar era. Few political figures have suffered from taking an anti-Soviet stance, while many have suffered from being associated with the absence of such a stance. Furthermore, not only did agencies within the government begin to monitor and estimate Soviet defense expenditure behavior at the behest of the executive branch (Prados, 1982), but failure to react to these changes has proven politically unwise. One of the lessons of the 1960, 1980, and 1984 elections is that when the U.S. is behind or perceived to be behind the Soviet Union, it is politically risky for a candidate to fail to call for dramatic increases in the defense effort. The president has a strong incentive to base his request on the most recent change in Soviet defense spending.

As a consequence of interest in reelection, and because of the dynamics of the political process, Congress is in a difficult position. In spite of the potential value of defense spending to each member's district, there are many other priorities competing for scarce fiscal resources. To ease the burdens of decision making, we assume Congress reacts to the president's budget request. If the president is asking for a large increase, Congress will be hard-pressed to deny him at least a portion of the request, for the reasons dis-

cussed earlier. If the president asks for no change or a decrease, Congress is likely to go along with him, because he can be blamed directly for failing to increase the amount of money targeted for defense. Thus, Congress bases its decision on the president's budget request.

The Department of Defense has massive obligated and unobligated reserves that could conceivably be spent in a given year (Ostrom, 1978). It cannot, however, spend whatever it wishes. The Department of Defense is dependent upon Congress for subsequent appropriations, and hence is likely to be sensitive to what the Congress has appropriated (Ostrom, 1978, p. 948). Therefore, it is likely that the year-to-year changes in defense spending will be a function of the changing congressional appropriation. Congressional changes serve as the interorganizational base for the DoD.

The Environmental Connection. The three faces of the defense expenditure decision discussed previously—How much can we afford? How much is enough? and How much does the public want?—suggest that three distinct sets of environmental factors have the potential to affect the defense expenditure decision: international factors (X_{jt}), economic factors (Y_{kt}), and domestic/political factors (Z_{mt}). This classification provides the following amplification of equation (3):

$$\begin{aligned} \sum a_{ij} E_{it} = & \sum b_{ij} X_{jt} + \sum c_{ik} Y_{kt} \\ & + \sum d_{im} Z_{mt} \end{aligned} \quad (3a)$$

To keep the policy-making rules simple and to complete the specification of equation (3a), we have chosen to investigate two stimuli in each of the three environmental subsets.

In the international arena, it is hypothesized that the difference between the U.S. and USSR defense expenditure totals (i.e., the spending gap/surplus) and the extent to which the U.S. is involved in an on-

going use of the military will have an impact on the defense expenditure decision. In each case, there will be a clear signal from the international arena that more resources must be given to national defense.

In the economic arena, it is hypothesized that organizational decision makers monitor both the change in expected revenues and the size of the surplus or deficit. These two factors send different signals to organizational policy makers concerning the opportunities for growth in the defense budget. If revenues are increasing, it may be possible to increase defense spending without cutting non-defense spending or incurring a deficit. However, if the desired increases lead to a deficit, it will mean that nondefense spending must be cut, or that more resources will have to be allotted to servicing the debt.

In the domestic political arena, it is hypothesized that organizational decision makers pay attention to mass public attitudes toward defense. By and large, the public does not have strong opinions on defense. What causes policy makers to take notice, however, is the appearance of clear opinion in favor of increasing defense spending or in favor of reducing defense spending. If either one of these attitudes is prevalent, it is likely that the organizational actors will take notice and react to the public's sentiment.

A Sequential Policy-Making Process. The goals and interests that are important to each actor determine which environmental stimuli will be monitored by each actor. Since each actor's environmental reaction coefficients (b_{ij} , c_{ik} , d_{im} in equation 3a) reflect its interests and goals, the reaction coefficients for the six environmental variables may differ from actor to actor. Two of these differences are likely to be systematic, and hence can be anticipated in the form of "system hypotheses." First, the organizational process and bureaucratic politics perspectives suggest

there will be an increasing reliance on the previous actor in the process (i.e., $a_{i1} > a_{i-1,1}$). As the policy-making process unfolds, the actors in previous steps will have incorporated their reaction to environmental stimuli; the temporally prior decision thus provides a current summary of the state of the environment as seen through the eyes of the previous organization. Second, as a result of this reliance, not only will subsequent actors in the process be less likely to react to the full range of environmental stimuli (i.e., the subset of environmental factors for actor i will not be larger than the subset of environmental factors for actor $i - 1$), but the impact of the environmental factors at subsequent stages will be attenuated (i.e., $a_{ij} < a_{i-1,j}$).

Substituting equation (3a) into the previous set of equations yields a complete decision rule for each organization:

$$\Delta D_{it} = a_{i1} \Delta D_{i-1,t} + \sum b_{ij} X_{jt} + \sum c_{ik} Y_{kt} + \sum d_{im} Z_{mt} + e_{it}^* \quad (5a)$$

Given an equation of the form of equation (5a) for each organization, it is possible to provide an explanation for the changes in U.S. defense expenditures. On the basis of the development to this point, a number of implications arise concerning the possible presence and location of a linkage between the defense expenditure policy making of the United States and that of the Soviet Union. First, since the prior defense expenditure decision is taken as a base, it is necessary to look at changes in behavior to locate a reaction. Second, there are economic and political factors—in addition to USSR behavior—that may impinge on the decisions at each stage. Third, any reaction to Soviet spending will be filtered, via the interorganizational connection, through the remaining steps in the policy-making process.

The Soviet Estimate

In spite of the number of estimates

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generated both in and out of government, every indication suggests that the members of the U.S. defense policy-making establishment utilize the estimates provided by the CIA.³ As Burton (1983, p. 85) observes,

The CIA's estimates of the dollar cost of Soviet defense activities are used in all branches of the United States government to measure the overall size and direction of Soviet defense programs in resource terms. Some go so far as to equate resource inputs with military capability, and to interpret them as direct measures of the Soviet military threat.

Others readily concede the importance of the CIA's estimate even if they disagree with its actual magnitude (cf. Holzman, 1980, 1982; Lee, 1977; Rosefielde, 1982).

Since 1964, the U.S. Arms Control and Disarmament Agency (ACDA) has regularly published a volume containing military expenditure data for most countries. Volumes prior to 1974 reported estimates of Soviet defense expenditures "adapted from national data and various estimates made by Western analysts." In 1975, ACDA reported that unnamed U.S. government agencies were the source of the published Soviet estimate. In 1976, and in each subsequent volume, the CIA was identified as the source for the ACDA estimate of Soviet military expenditures. Although we cannot state unequivocally that ACDA estimates prior to 1975 were derived from CIA estimates, the ACDA/CIA series that is used represents the best public estimate of the official estimates of USSR defense expenditures made by U.S. governmental agencies.

To ascertain whether the U.S. in fact reacts to the USSR, Soviet budget information that could have been used by U.S. policy makers at the time the decisions were made must be located. To this end, it is necessary to establish a number of criteria that a plausible Soviet estimate must satisfy.⁴

During the Senate Watergate Hearings, Howard Baker, senator from Tennessee, gained national exposure with his per-

sistent question, "What did the president know and when did he know it?" This question is similarly pertinent to the study of U.S. defense spending. In order to test for a reaction to Soviet military expenditures one needs to use the Soviet data that satisfies certain temporal restrictions. Information having utility for decision makers must be available at the time the decisions are made. This implies consideration of three issues related to what information defense decision makers had and when they had it.

First, in terms of a relevant decision-making time horizon, defense policy makers obviously could not have used data revised after the fact. The ACDA/CIA presents revised data sets in each annual presentation. For our purposes, the important question is not whether the ACDA/CIA current or revised estimates are more accurate, but which set is most representative of the information available when the decision was made. We would contend that the contemporaneous estimate is the one more likely to have been used by defense policy makers. Therefore, all revised data is removed from consideration. In operational terms, the following procedure is employed: the original estimate of Soviet expenditures for a given year is the one that will be included in our data set. By chaining the contemporaneous ACDA/CIA estimates in this fashion, it is possible to approximate more closely the information available to U.S. defense planners at the time their decisions were being made. These constitute the best estimates of Soviet defense expenditures for the ACDA/CIA data.

Second, it is necessary to determine the relevant historical period within which U.S. decision makers have shown sensitivity to and concern with Soviet military expenditures. We contend that the time frame within which Soviet data has been considered is relatively brief, spanning the period from 1964 to the present. As Prados (1982, p. 245) observes,

Estimates of Soviet military spending were a product of McNamara's tenure at the Pentagon when, in order to have Soviet spending data to incorporate in systems analysis studies, the secretary of defense put substantial pressure on CIA to produce such estimates.

There is no indication that the magnitude of Soviet defense spending was of great concern to anyone in the U.S. government prior to 1964.

The third temporal consideration is related to the lag time between Soviet spending decisions and the generation of the estimates of Soviet spending and their use by U.S. defense expenditure policy makers. We contend that there is, *at minimum*, a three-year lag resulting from a *two-year lag* in the Soviet estimate and the *one-year look ahead* by the president. The information lag is a result of the timing of the U.S. defense expenditure budgeting process. For example, in preparing his fiscal year 1984 defense budget, President Reagan finalized his request in the latter part of 1982, in order to present his proposal to Congress in early 1983. If information is to affect the president's

fiscal year 1984 defense expenditure decision, it must be available no later than the latter part of 1982. The generation of the Soviet estimate is, by all accounts, an extremely complicated task. It stands to reason, then, that it would take a considerable amount of time following the end of the Soviet budget year (which is on an annual cycle) to come up with and justify an estimate of Soviet spending. Accepting these limitations, the most recent and available Soviet estimates would pertain to calendar year 1981. If there is to be a reaction to Soviet spending in the fiscal year 1984 budget, it must be to 1981 Soviet expenditure behavior. Data on Soviet defense spending for 1982, 1983, or 1984 was simply not available to policy makers at the time the fiscal year 1984 decision was made.

On the basis of the requirements that a Soviet estimate must satisfy, an ACDA/CIA Soviet defense expenditure series—hereafter referred to as X_{1t-3} —has been generated for USSR fiscal years 1964–1982. This series, presented in Table 1,

Table 1. The ACDA/CIA Soviet Estimate

U.S. Fiscal Year	Year Available	USSR Fiscal Year	Best ACDA/CIA ^a Estimate	Estimated ACDA/CIA Gap
1967	1966	1964	\$ 40,000	\$ 9,470
1968	1967	1965	40,000	5,880
1969	1968	1966	47,000	7,093
1970	1969	1967	52,000	15,357
1971	1970	1968	55,000	22,265
1972	1971	1969	60,000	17,785
1973	1972	1970	65,000	12,070
1974	1973	1971	74,000	472
1975	1974	1972	81,000	-5,924
1976	1975	1973	86,000	-12,777
1977	1976	1974	103,000	-25,450
1978	1977	1975	119,000	-34,100
1979	1978	1976	127,000	-39,109
1980	1979	1977	139,788	-44,231
1981	1980	1978	153,600	-50,558
1982	1981	1979	181,400	-66,387
1983	1982	1980	207,400	-74,560
1984	1983	1981	236,700	-80,604

^aACDA/CIA: U.S. Arms Control and Disarmament Agency/Central Intelligence Agency. Estimates are in millions of current U.S. dollars.

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constitutes the best evidence, and as such, is a potential tool for testing the hypothesis that the United States reacts to the military expenditure behavior of the Soviet Union.

Recognition of the three-year information lag also has implications for U.S.-USSR spending comparisons. At any particular time, the best estimate of the spending gap—hereafter referred to as X_{2t-3} —is as follows:

$$X_{2t-3} = D_{3t-3} - X_{1t-3},$$

where D_{3t-3} is the U.S. defense expenditure total lagged three years. Note that in computing the gap for each fiscal year, we are using data lagged three years. As such, the estimated gap at $t-3$ is the most recent information that the president and Congress have about the current state of the U.S.-Soviet balance. As can be seen in Table 1, the ACDA/CIA data suggests that since Soviet fiscal year 1972, the U.S. has trailed the USSR in spending and the gap between the superpowers has been increasing. Note, however, that given our previous argument, the U.S. could not have recognized and/or reacted to the 1972 gap until fiscal year 1975.

If the presence and magnitude of the perceived gap is important—and we contend that it is—the gap should play a prominent role in any explanation of defense expenditure policy making. This is not to say that the ACDA/CIA data is correct. Rather, if U.S. policy makers are making decisions on the basis of this data—whatever its accuracy—it is necessary to test a model with data that captures (and measures) these perceptions.

Empirical Evaluation

The following equations represent the operational version of equation (5a) for each of the three organizational actors, and correspond to the three decisions that we are modeling: the annual changes in the defense budget recommendations,

appropriations, and expenditures made, respectively, by the president, Congress, and the Department of Defense:

$$\begin{aligned} \Delta D_{1t} = & a_{11} \Delta X_{1t-3} + b_{12} X_{2t-3} \\ & + b_{13} \Delta X_{3t-1} + c_{11} \Delta Y_{1t-1} \\ & + c_{12} Y_{2t-1} \\ & + d_{11} \times (Z_{1t-1} \times \Delta X_{1t-3}) + e_{1t} \end{aligned} \quad (6)$$

$$\begin{aligned} \Delta D_{2t} = & a_{21} \Delta D_{1t} + b_{22} X_{2t-3} \\ & + d_{22} \times (Z_{2t-1} \times \Delta D_{1t}) + e_{2t} \end{aligned} \quad (7)$$

$$\begin{aligned} \Delta D_{3t} = & a_{31} \Delta D_{2t} + b_{32} X_{2t-3} \\ & + d_{31} \times (Z_{1t} \times \Delta D_{2t}) + e_{3t}. \end{aligned} \quad (8)$$

See the Appendix for the definitions, operationalizations, and sources for all variables. Note that each equation contains an interorganizational connection—for the president: change in Soviet estimate (ΔX_{1t-3}); for Congress: the change in the president's budget (ΔD_{1t}); and for the DoD: change in congressional appropriation (ΔD_{2t}). The environmental variables were selected from the previously discussed international, economic, and domestic/political environmental factors.⁵ Where appropriate, the environmental variables have been lagged to reflect the information available to each actor in the defense expenditure policy-making process at the time each decision was made. Negative and positive public opinion (Z_{1t} and Z_{2t} , respectively) have been introduced into the model in an interactive fashion with the interorganizational connection, reflecting our contention that certain configurations of the domestic/political environment lead each organization to adjust the magnitude of its interorganizational connection.

To address the central questions identified at the beginning of this article (Does the U.S., insofar as defense spending is concerned, react to the military expenditures of the Soviet Union? If so, what is the magnitude of the reaction?), the parameters in equations (6) through (8) must be estimated and evaluated. Con-

Table 2. Coefficient Estimates for the Mediated Reactive Linkage Model

Variable Description	Coefficient	Standard Error	t ratio
<i>President's Budget Request</i>			
Change in Soviet estimate (ΔX_{1t-3})	.456	.255	1.79 ^a
Spending gap (X_{2t-3})	-.322	.056	-5.72 ^b
Change in war costs (ΔX_{3t-1})	.830	.241	3.44 ^b
Change in revenues (ΔY_{1t-1})	.267	.046	5.79 ^b
Estimated deficit (Y_{2t-1})	.414	.058	7.20 ^b
Negative public opinion (Z_{1t-1})	.536	.171	3.14 ^b
$R^2 = .926$; $SE = 3995$; $DW = 2.16$ Condition Number = 9.23			
<i>Congressional Appropriation</i>			
Change in presidential request (ΔD_{1t})	.558	.075	7.47 ^b
Estimated spending gap (X_{2t-3})	-.117	.032	-3.67 ^b
Positive public opinion (Z_{2t-1})	.188	.076	2.48 ^a
$R^2 = .957$; $SE = 3016$; $DW = 1.73$ Condition Number = 3.79			
<i>Department of Defense Expenditures</i>			
Change in congressional appropriation (ΔD_{2t})	.717	.089	8.10 ^b
Estimated spending gap (X_{2t-3})	-.101	.032	-3.21 ^b
Negative public opinion (Z_{1t})	-.182	.078	-2.33 ^a
$R^2 = .935$; $SE = 2550$; $DW = 1.84$ Condition Number = 4.86			

^aCoefficient significantly different from 0.00 at the .05 level.

^bCoefficient significantly different from 0.00 at the .01 level.

sistent and efficient estimates were obtained using a Zellner or two-stage Aitken generalized least squares (GLS) procedure (see Kmenta, 1971). The resulting estimates and summary statistics are presented in Table 2. Each of the equations accounts for a very substantial portion of the variation in the changes for the three annual defense expenditure decisions: 92.6% for the president's decision, 95.7% for the congressional decision, and 93.5% for the DoD decision. The standard errors for each equation are also relatively small: \$4.0 billion for the president, \$3.0 billion for the Congress, and \$2.6 billion for the Department of Defense. Moreover, examination of the Durbin-Watson statistics suggests that the null hypothesis of no serial correlation can be accepted for all three equations. In terms

of multicollinearity, the condition numbers for the three equations (Belsley, Kuh, and Welsch, 1980, pp. 100-104) indicate that the degree of collinearity is not a serious problem. At a minimum, the estimated model has a substantial degree of empirical support in explaining the changes in each stage of the U.S. defense expenditure process. To assess the plausibility of the explanation, it is necessary to examine each equation separately.

President's Request

The president's decision rule, given by equation (6), suggests the following observations. First, the president does appear to base his change in defense budget requests on changes in the ACDA/CIA estimates of Soviet military spending. On

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average, presidential defense budget requests increase by a factor nearly one-half the estimated Soviet increase. Note that the coefficient has the lowest *t*-ratio, suggesting that the president is not totally reliant on his base; instead, he monitors and reacts to many other factors as well. This decision rule appears quite stable, except during periods when there is substantial public opinion against defense spending. Under these circumstances, presidents appear to try to match estimated Soviet increases dollar-for-dollar. It is significant that the president's reliance on the base increases dramatically in the face of negative public opinion. During these periods, the change in Soviet spending provides an important cue and source of justification for the president's budget.

Second, the president is quite sensitive to the estimated spending gap between the U.S. and the USSR, attempting to reduce this gap by approximately one-third each year. When considered in concert with the base, presidents pay close attention to the ACDA/CIA perceptions of the military spending behavior of the Soviet Union. The Soviet estimate has a marked impact on presidential defense budget requests.

Third, the estimated coefficient for the change in war costs suggests that presidential defense budget requests will, *ceteris paribus*, decrease during periods of deescalation. That this coefficient is not significantly different from 1.0 suggests that war costs are simply added or subtracted off the top. This runs counter to previous findings that war has a ratchet effect on defense spending (Russett, 1972).

Fourth, presidential defense budget requests are directly proportional to the projected change in total federal revenues. This coefficient suggests that presidents have, on average, proposed a "share of the pie" for defense that is nearly 27% of the revenue increases. Note that this coefficient is very close to the actual defense share of the U.S. federal budget, and

hence may reflect the attempts of recent administrations to divide revenue increases in rough proportion to their current share.

Finally, presidential defense budget requests appear to be inversely related to the estimate of the total federal deficit ($Y_{2t} - 1$ is negative when there is a deficit), indicating that these deficits act as constraints on the size of the proposed increase. While this finding is plausible, it must be interpreted with some caution. The size of the estimated federal deficits for the period covered by this analysis is relatively small by current standards, with a maximum value just under \$91.6 billion. Whether this empirical relationship will remain stable in the post-sample period (where estimated deficits are at least twice this magnitude) cannot be determined at this time.

Congressional Appropriations

The estimated version of the congressional decision rule, given by equation (7), provides strong support for the hypothesis that changes in Congress's defense appropriations are tied to changes in presidential requests. The magnitude of the estimated coefficient would seem to indicate, however, that Congress has not been totally acquiescent to executive proposals, cutting them by an average of 44%. It should be kept in mind, though, that we are dealing with changes rather than levels, so that a percentage reduction in a proposed increase for defense is still an increase.

Congress is also responsive to public opinion vis-a-vis defense spending. Whereas the president adjusts his defense budget requests in response to negative public opinion, members of Congress appear to respond to positive public opinion. The logic for this shift is interesting. By imposing a cutting rule on presidential defense budget requests, Congress can, in effect, view itself as responsive to

a public mood that feels too much is being spent on defense. On the other hand, if the barometer of public opinion favors increases in defense expenditures, then Congress may be inclined to grant the president more of his sought-after requests. As can be seen in Table 2, Congress has been even more receptive to executive requests during periods when public opinion has favored increasing the defense budget: the average cut is reduced to slightly more than 25%.

Congress does not disregard evidence suggesting that the U.S. is lagging militarily behind the Soviet Union. Changes in congressional defense appropriations are adjusted in response to the estimated spending gap between the U.S. and the USSR. There is empirical support for the hypothesis that Congress adjusts defense appropriations in response to the estimated spending gap, though this reaction is less than that demonstrated by the president. This result suggests that members of Congress do, in fact, monitor information pertaining to the international environment. Therefore, in addition to reacting to the president's request (based in part on the gap), Congress makes an independent assessment and adjusts its appropriation decision accordingly.

Finally, it should be noted that the operational version of the congressional decision rule excludes factors from the economic environment. We do not wish to imply that members of Congress are inattentive to such factors, but rather that they rely, to a large degree, on the president's incorporation of these concerns into his budget request. For example, the forecasts of federal revenues and deficits produced by the Congressional Budget Office (CBO) have been quite similar to those provided by the president; hence their information base has been nearly identical. Should these estimates begin to diverge sharply in the future, one might expect less congressional reliance on the president's assessment of the economy.

Department of Defense Spending

Of all the actors in the defense expenditure policy-making process, the Department of Defense is the most severely constrained in terms of its latitude to engage in spending behavior that varies from one year to the next. The principal cue for DoD expenditure changes is the change in the congressional defense appropriation. We have hypothesized that DoD expenditure changes will be very similar to these congressional changes. This parallel behavior is to be expected, given that DoD is dependent upon Congress for future appropriations. From the estimated version of the DoD decision rule, given by equation (8), it can be seen that these expenditure changes are strongly linked to changes in congressional defense appropriations—spending nearly 72% of the congressional changes.

The DoD is also responsive to the climate of public opinion. During those periods when there is a preponderance of public opinion against defense spending, the DoD reduces its defense expenditures to avoid controversy and a further backlash. Our analysis suggests that during such periods, the DoD will spend only 53% of the change in congressional appropriations.

There is also empirical support for the proposition that U.S. defense expenditure decisions are reactive to the estimated spending gap between the superpowers. It appears as though the DoD attempts to reduce this gap by an additional 10% each year. That is, the perceived gap has an influence independent of the previous actors in the policy-making process. This suggests the DoD feels free to spend more if there is an estimated spending gap between the U.S. and the USSR.

Finally, as with the congressional policy-making rule, there is no indication that economic factors influence the DoD decision. Consequently, the only economic impacts will be reflected in the interorganizational connection.

System Hypotheses

Table 2 provides support for the two "system" hypotheses developed earlier. The pattern of increasing reliance on the previous actor in the process is empirically supported. Moreover, it can be seen that the range of relevant environmental factors is greatest for the president and decreases for the Congress and the DoD. Even though the number of environmental factors decreases, the explained variance for each equation remains substantial.

From a substantive perspective, the acceptance of the two system hypotheses leads us to conclude that the decision rules for both the Congress and the Department of Defense are less complex than that for the president. These simpler formulations should not be taken as evidence that organizational or bureaucratic forces do not exist for these actors, but rather that an increased reliance on the interorganizational connection implies that a narrower set of interests combines to produce the annual changes in congressional defense appropriations and DoD expenditures. This greater reliance can also be witnessed in terms of the number of environmental variables resulting in adjustments to the base employed by each actor. The president responds to a much wider range of environmental factors than does either the Congress or the DoD. As a consequence of his role as budget initiator, the president plays a pivotal role in the defense expenditure policy-making process.

The Impact of the Soviet Estimate

Having determined that the basic specification has a substantial degree of empirical support, attention can be given to the two central questions identified at the beginning of this article. To determine the extent to which changes in the Soviet estimate and spending gap are translated into increases or decreases in U.S. defense

spending, the joint implications of all three equations must be examined.

Equations (6) through (8) represent the *structural form* of the model under study and are based upon our theory of organizational/bureaucratic behavior. By successively substituting the right-hand side of equation (6) for ΔD_{1t} in equation (7), and the right-hand side of equation (7) for ΔD_{2t} in equation (8), we obtain the *reduced form* equation for the U.S. defense expenditure decision, ΔD_{3t} . Performing the suggested substitutions yields the following reduced-form equation for U.S. defense spending:

$$\begin{aligned}\Delta D_{3t} = & p_{31} \Delta X_{1t-3} + p_{32} X_{2t-3} \\ & + p_{33} \Delta X_{3t-1} + p_{34} \Delta Y_{1t-1} \\ & + p_{35} Y_{2t-1} + v_{3t},\end{aligned}\quad (9)$$

where

$$\begin{aligned}p_{31} = & [(a_{31} + d_{31} \times Z_{1t}) \\ & (a_{21} + d_{22} \times Z_{2t-1}) (a_1 + d_{11} \times Z_{1t-1})]; \\ p_{32} = & \{(a_{31} + d_{31} \times Z_{1t}) \\ & [(a_{21} + d_{22} \times Z_{2t-1}) b_{12} + b_{22}] \\ & + b_{32}\}; \\ p_{33} = & [(a_{31} + d_{31} \times Z_{1t}) \\ & (a_{21} + d_{22} \times Z_{2t-1}) b_{13}]; \\ p_{34} = & [(a_{31} + d_{31} \times Z_{1t}) \\ & (a_{21} + d_{22} \times Z_{2t-1}) c_{11}]; \\ p_{35} = & [(a_{31} + d_{31} \times Z_{1t}) \\ & (a_{21} + d_{22} \times Z_{2t-1}) c_{12}]; \\ v_{3t} = & \{(a_{31} + d_{31} \times Z_{1t}) \\ & [(a_{21} + d_{22} \times Z_{2t-1}) e_{1t} + e_{2t}] \\ & + e_{3t}\}.\end{aligned}$$

The a_{ij} , b_{ij} , c_{ik} and d_{im} are the structural coefficients from equations (6) through (8). The reduced-form equation provides a summary of the way in which annual changes in U.S. defense expenditures are affected by each of the predetermined variables in the system; it incorporates all

**Table 3. Estimated Reduced Form Coefficients, Expenditure Equation
[Equation (9)]**

Year	Change in Soviet Estimate (<i>t</i> - 3)	Estimated Spending Gap (<i>t</i> - 3)	Change in War Costs (<i>t</i> - 1)	Change in Revenue (<i>t</i> - 1)	Estimated Deficit (<i>t</i> - 1)
1968	.18	-.31	.33	.11	.17
1969	.14	-.26	.25	.08	.12
1970	.30	-.26	.25	.08	.12
1971	.30	-.26	.25	.08	.12
1972	.30	-.26	.25	.08	.12
1973	.30	-.26	.25	.08	.12
1974	.30	-.26	.25	.08	.12
1975	.30	-.26	.25	.08	.12
1976	.30	-.26	.25	.08	.12
1977	.40	-.31	.33	.11	.12
1978	.18	-.31	.33	.11	.12
1979	.18	-.31	.33	.11	.12
1980	.18	-.31	.33	.11	.12
1981	.24	-.36	.44	.14	.22
1982	.18	-.29	.33	.11	.17
1983	.30	-.26	.25	.08	.12
1984	.30	-.26	.25	.08	.12

Note: Each series of coefficients is computed using the formula for p_{3j} associated with equation (9), the defense spending reduced form equation. The values of each p_{3j} change in accordance with the changing values of Z_{1t-1} , Z_{2t-1} , and Z_{1t} . For example,

$$p_{31} = [(a_{31} + d_{31} \times Z_{1t})(a_{21} + d_{22} \times Z_{2t-1})(a_{11} + d_{11} \times Z_{1t-1})]$$

depicts the manner in which the reduced form coefficient for changes in the Soviet estimate varies in response to the changes in the three public opinion variables.

of the reaction coefficients from each of the three actors into a single equation. Since the two public opinion variables enter the model in an interactive fashion, the characterization of the reduced form is somewhat complicated. To simplify the presentation, the two public opinion variables have been included in the definition of each reduced-form coefficient.

A Mediated Reactive Linkage

Equation (9) illustrates the impact of each of the predetermined variables on changes in U.S. defense expenditures, ΔD_{3t} . The p_{3j} in equation (9) are called *impact multipliers* (see Kmenta, 1971), since they measure the immediate response of DoD policy makers to changes in all of the environmental fac-

tors. The expenditure reduced-form equation illustrates that the impact of the environmental factors is mediated both by the presence of three organizational actors *and* by public opinion. For example, the impact of perceived changes in Soviet military expenditures, denoted by p_{31} , is a multiplicative function of the interorganizational connections between the president, Congress, and the DoD, with adjustments made in response to public opinion. The mediation of the estimated gap between U.S. and USSR spending, p_{32} , is more complex, since it represents the combined effects of each actor's individual perception of this gap, as well as the interorganizational connections and public opinion.

Table 3 presents estimates of the derived reduced-form coefficients for each

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year in our sample. The reduced-form coefficients evidence some rather marked changes over time in response to changes in mass public opinion. As a basis for comparison, the coefficients for 1968 provide an indication of the values of each reduced-form coefficient when public opinion does not affect the policy-making process (i.e., $Z_{1t} = 0$, $Z_{1t-1} = 0$, $Z_{2t} - 1 = 0$). The model indicates that in 1968, the change in U.S. defense expenditures consisted of 18.3% of the change in the Soviet estimate, -31.4% of the spending gap, 33.3% of the change in war costs, 10.7% of the change in revenues, and 16.6% of the estimated deficit.

The changes in the predicted values of the reduced-form coefficients over time, as portrayed in Table 3, indicate that public opinion has altered the degree to which the five environmental variables have affected changes in U.S. defense spending. The results in Table 3 also shed light on the previously discussed research findings of Russett (1972). Insofar as his first finding is concerned, we concur: public opinion does affect defense expenditure decisions. Unlike Russett (1972), however, the present results indicate that public opinion can either increase or decrease the yearly change in defense spending. Furthermore, it is noteworthy that negative public opinion can lead to an expenditure increase. In the face of negative public opinion, the president increases his reliance on his base—the change in the Soviet estimate. Thus, if there is an increase in the Soviet estimate and if public opinion is negative, the president's budget request will increase. This, in turn, will be transmitted through the remainder of the policy-making process.

The spending reduced-form equation makes explicit the two ways in which the reactive linkage is *mediated*. On the one hand, the three steps, in conjunction with the interorganizational connection, serve to introduce the interests of all three organizations into the policy-making

process. On the other hand, mass public opinion about appropriate levels of defense spending, both positive and negative, alters the process substantially.

The Year-to-Year Impact

To assess the expenditure implications of the impact multipliers, Table 4 uses the expenditure equation reduced-form coefficients to provide information concerning the relative impacts of each of these variables throughout the period of fiscal years 1968–1984. Table 4 presents the following quantity for each environmental variable in the system:

$$p_{3j} \times E_{jt}, \quad (10)$$

where E_{ij} represents the environmental variables. These quantities represent the "contribution" of each of the five environmental variables to the predicted change in U.S. defense spending. For example, in 1984, the forecasted expenditure change of \$16,680 million is the result of \$8,690 million from the change in the Soviet estimate, \$20,965 from the spending gap, -\$1,616 million from the change in revenues, and -\$11,359 million from the estimated deficit. The response to the USSR pushes the forecasted values up and the two economic factors drive it down.

From Table 4 it is also possible to assess the degree to which changes in the values of the predetermined variables have affected changes in U.S. defense spending. The impact of changes in the Soviet estimate has been uniformly positive throughout the 1968–1984 period. These changes, which have had especially large impacts in 1977, 1982, and 1983, can be traced to the joint impact of negative public opinion and large increases in the Soviet estimate. The impact of the spending gap has been varied. Prior to 1975, the U.S. was "ahead" (i.e., the spending gap was a spending surplus), and hence it reduced the change in defense spending. From 1976 on, however, the presence of a

Table 4. Single Period Impact, Historical Forecasts [Equation (10)]
(in millions of current U.S. dollars)

Year	Change in U.S. Defense Expenditures		Contribution to Forecasted Value				
	Actual	Predicted	Change in Soviet Estimate (t - 3)	Estimated Spending Gap (t - 3)	Change in War Costs (t - 1)	Change in Revenue (t - 1)	Estimated Deficit (t - 1)
1968	9,908	2,708	0	-1,847	4,191	670	-307
1969	520	-614	954	-1,845	397	884	-1,004
1970	-715	-702	1,483	-3,994	372	2,061	-623
1971	-2,598	-4,265	890	-5,791	-1,018	872	782
1972	604	-3,776	1,483	-4,626	-1,465	667	165
1973	-1,853	-1,815	1,483	-3,139	-1,043	2,328	-1,444
1974	4,327	-31	2,669	-123	-372	1,144	-3,349
1975	7,350	3,616	2,076	1,541	-1,440	3,012	-1,573
1976	2,991	5,707	1,483	3,323	0	2,321	-1,420
1977	7,666	6,716	6,756	7,993	0	1,923	-9,956
1978	7,485	11,340	2,921	10,710	0	5,605	-7,896
1979	11,971	8,276	1,460	12,283	0	4,756	-10,223
1980	17,827	13,585	2,334	13,824	0	5,417	-8,058
1981	23,256	21,447	3,368	18,069	0	6,840	-6,470
1982	26,754	28,485	5,060	19,413	0	8,945	-4,933
1983	27,634	26,908	7,711	19,393	0	5,074	-5,269
1984	16,929	16,679	8,690	20,965	0	-1,616	-11,359

spending gap has had an ever expanding impact on changes in U.S. defense expenditures. It can be seen that the impact of changing war costs has, with the exception of 1968, been rather inconsequential. Estimated revenues, however, have led to large increases from 1978 to 1983. The drop in expected revenues following the Reagan tax cut led to a substantial decline in the predicted change in U.S. defense spending. Finally, the estimated deficit has had a dampening effect on defense spending throughout the 1977-1984 period.

A comparison of the actual and predicted changes in U.S. defense expenditures in Table 4 shows that the model is capable of explaining a wide variety of behavior. Note especially, the ability of the model to "track" the increases prior to fiscal year 1984, and also to account for the rather dramatic decline in the size of the fiscal year 1984 increase. As can be seen, throughout the 1978-1984 era, the two Soviet spending variables and revenue increases have been the primary

sources for the dramatic increases in U.S. spending. Furthermore, the presence of a deficit is pulling in the opposite direction. Even though our model is different from that presented by Fischer and Kamlet (1984), by displaying the impact of each of the variables over time we can also demonstrate the tension that often exists between fiscal and strategic considerations in the context of defense budgeting. Finally, it is of considerable interest to observe that the large increases in U.S. defense spending are attributable to changes in the five environmental variables, and not to changes in the structure/substance of the policy-making process. Wide variations in the changing defense expenditure time series can occur in the context of a temporally stable policy-making process.

The Changing Base

An underlying premise of the present explanation is that the previous spending

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Table 5. Cumulative Impact, Historical Forecasts [Equation (11)]
(in millions of current U.S. dollars)

Year	Cumulative Change in U.S. Defense Expenditures	Contribution to Cumulative Forecast Value				
		Change in Soviet Estimate ($t - 3$)	Estimated Spending Gap ($t - 3$)	Change in War Costs ($t - 1$)	Change in Revenue ($t - 1$)	Estimated Deficit ($t - 1$)
1968	9,908	0	-1,847	4,191	670	-307
1969	10,428	954	-3,692	4,589	1,554	-1,311
1970	9,713	2,437	-7,686	4,961	3,615	-1,935
1971	7,115	3,326	-13,477	3,943	4,487	-1,153
1972	7,719	4,809	-18,103	2,478	5,153	-988
1973	5,866	6,292	-21,242	1,436	7,482	-2,432
1974	10,193	8,961	-21,365	1,063	8,626	-5,781
1975	17,543	11,037	-19,824	-377	11,638	-7,354
1976	20,534	12,520	-16,501	-377	13,959	-8,774
1977	28,200	19,276	-8,508	-377	15,882	-18,730
1978	35,685	22,197	2,202	-377	21,486	-26,626
1979	47,656	23,657	14,486	-377	26,242	-36,849
1980	65,483	25,991	28,377	-377	31,659	-44,908
1981	88,739	29,360	46,446	-377	38,499	-51,378
1982	115,493	34,420	65,859	-377	47,445	-56,311
1983	143,127	42,131	85,252	-377	52,519	-61,580
1984	160,056	50,821	106,216	-377	50,902	-72,940

decision is the base; each year's change becomes part of the base for the subsequent year. Table 5 shows that if changes in the U.S. defense expenditures are accumulated over time (i.e., adding all of the changes), the base has increased by \$160,056 million since 1967.⁶ The impact of each of the environmental variables can be accumulated in a similar fashion to obtain a rough indication of the implied source of changes in the base. For this reason, it makes sense to look at the cumulative impact of each variable on the rising level of U.S. defense spending. In this vein, Table 5 provides the following accumulation for each of the five factors:

$$\sum_{t=1968}^{1984} p_{3i} \times E_{it} \quad (11)$$

These values represent the accumulated impact of each variable over time; hence, the 1984 values for each of the predetermined variables provide an indication of

how much of the \$160,056 million change in the base from 1967 to 1984 can be attributed to each of the environmental variables.

As can be seen in Table 5, the cumulative impact is as follows: \$50,821 million for changes in the Soviet estimate, \$106,216 million for the spending gap, -\$377 million for changes in war costs, \$50,902 million for changing revenue, and -\$72,940 million for the deficit. The two measures of Soviet behavior sum to over \$157,000 million, and the two economic measures sum to over -\$22,000 million.⁷

It is noteworthy that the cumulative change in defense spending totaled only \$35,000 million through fiscal year 1978. Since that time, the total changes have quadrupled. The primary source of the increase is the spending gap. From the initial time that the ACDA/CIA reported that the U.S. spending lagged behind that of the Soviet Union, U.S. defense expenditures—and the base—have increased

dramatically. In the context of this model, it is clear that the reaction to the perceived changes in Soviet defense expenditure behavior, as well as the perceived gap, have been the primary sources of the dramatic rise in U.S. defense spending in the post-Vietnam era.

Conclusions and Implications

On the basis of the preceding analyses, five conclusions merit special attention. First, the estimated version of the model provides evidence that public opinion concerning the appropriate magnitude of the U.S. defense effort plays an important role in U.S. defense expenditure policy making. As shown in Table 3, some of the reduced form coefficients are altered considerably in the presence of certain configurations of public opinion. Furthermore, the impact of the changing coefficients can be substantial, especially in the case of negative public opinion and large increments in the Soviet estimate. Second, after indicating that a usable Soviet estimate must meet certain standards (annual production, restriction to the post-1964 period, and availability when policy makers are deliberating), the model demonstrates that changes in U.S. defense spending are directly related to perceived changes in Soviet spending, as well as to the perceived spending gap between the U.S. and USSR. Not only is there a reaction at each step in the policy-making process (see Table 2), but as Tables 4 and 5 illustrate, the two measures of Soviet behavior have provided the primary impetus for increases in the U.S. defense effort. Third, the discontinuity between the justifications of the policy makers and findings of the research community disappears when the specification of the model of the policy-making process is enhanced and a more appropriate estimate of Soviet defense spending is used. The findings from our research are consistent with the assertions of the defense policy makers:

the U.S. does react to the ACDA/CIA Soviet estimate. Fourth, as a consequence of the sequential character of the policy-making process, the impact of the environment is mediated by both the interests/goals of the three organizational actors and public opinion. The changes in U.S. defense spending must be viewed as a *political resultant*. Fifth, changes induced by public opinion and/or large changes in the five environmental inputs can lead to dramatic changes in the expenditure decision within the confines of a stable policy-making process.

It is important to note that the previously discussed reactive linkage is between estimated Soviet spending and actual U.S. spending. This has two important implications for the ongoing debate about the appropriate level of defense spending. First, as noted previously, the ACDA/CIA series is constantly revised to take into account mistakes that have been made in previous estimates. As a consequence, the revised data is much more accurate than the current data (Kaufman, 1985). Even though there is a high degree of uncertainty associated with the most current estimate, our earlier arguments concerning the previous year's decision as the base imply that it is unlikely the revised data will be incorporated into defense expenditure policy making. Therefore, policy makers proceed with uncertain estimates of Soviet defense expenditures.

Second, the uncertainty concerning the ACDA/CIA's Soviet estimate may be systematically biased toward overestimating the level of Soviet spending. Recently, the CIA has reported that it overestimated increases in Soviet defense spending during the entire 1977-1983 period (Kaufman, 1985). Kaufman (1985, p. 181) notes that "the process which led to the 1983 revisions began about a year earlier. . . . To date, the agency has not disclosed . . . why it took until 1983 to disclose the fact that the trend had changed."

One reason it took so long to disclose the reduction in the Soviet estimate could be that the ACDA/CIA Soviet estimate is more than a best estimate of Soviet behavior; it could be affected by bureaucratic self-interest. To assess the veracity of the ACDA/CIA estimates of Soviet defense spending, it seems prudent to begin to consider the manner in which these estimates are formulated. Due to timing problems, we chose not to incorporate the generation of these estimates into the basic model; they are treated as exogenous inputs.⁸ The organizational/bureaucratic perspective we adopt may provide future researchers with a set of initial assumptions about the behavior of the actors responsible for the production of these estimates. For example, a view of the CIA as a "player in a position" suggests they will see a "face of the issue" that is a product of their own organizational interests. Although we have no knowledge that organizational interests have an impact on the Soviet estimate, the secrecy surrounding the ACDA/CIA and the complexity of the building-block method raise the possibility that the Soviet estimate could be biased in a "strategic" fashion. There are ample precedents in other areas of bureaucrats behaving so as to maximize their self-interests.

According to our model, reactions to the changes in the Soviet estimate and attempts to close the spending gap (which may not have existed), have accounted—as can be seen in Table 5—for over \$100 billion of the increase in defense spending since 1978! Therefore, not only is U.S. defense spending higher than it would have been had the lower growth estimates been used, but the changes that have already occurred have been incorporated into the base. It is unlikely that the U.S. defense expenditure total will be reduced in light of the recent CIA disclosures of overestimation. The structure of the defense expenditure policy-making process and the Soviet estimate have enor-

mous consequences for current and future considerations of the defense/nondefense tradeoff.

Appendix. Data Definitions

The *dependent variables* are the annual changes in presidential defense budget requests (ΔD_{1t}), congressional defense appropriations (ΔD_{2t}), and expenditures by the Department of Defense (ΔD_{3t}). Data for the first two variables were taken from the *Congressional Quarterly Almanac* (1984, 98th Congress, 1st session, 1983 vol. 39). The *Budget of the United States Government* (1964–1984) is the source for U.S. military expenditures. Each of these variables is reported in current dollars on a U.S. fiscal year basis.

Changes in the Soviet estimate (ΔX_{1t-3}), as indicated above, were taken from the U.S. Arms Control and Disarmament Agency's annual publication, *World Military Expenditures and Arms Transfers* (1963–1982; 1985). These yearly changes are the ACDA/CIA estimates of Soviet military expenditures at time $t-3$ minus the ACDA/CIA Soviet estimates at time $t-4$.

The *estimated spending gap* (X_{2t-3}) is the difference between U.S. military expenditures at time $t-3$ and the ACDA/CIA Soviet estimates at time $t-3$.

Changes in U.S. war costs (ΔX_{3t-1}) are the estimated additional costs above what would have been spent to maintain a baseline force in peacetime in time t minus similar costs in time $t-1$. These war costs were taken from *The Economics of Defense Spending: A Look at the Realities*, July 1972 (DoD, 1973, p. 149). This document was produced by the U.S. Department of Defense (comptroller).

Estimates of revenue changes (ΔY_{1t-1}) and *total deficits* (Y_{2t-1}) were taken from the U.S. Department of Treasury document, *Treasury Bulletin*. Each of these variables is a "current estimate" rather than the actual total for a given fiscal

year. Revenue changes reflect the forecasted revenues for fiscal year $t - 1$ minus the forecasted revenues for fiscal year $t - 2$. The deficit variable is the anticipated difference between forecasted revenues at time $t - 1$ and total planned expenditures at time $t - 1$.

Public opinion data on defense spending was taken from *Gallup Poll Reports* (March 1985, Report No. 234). The negative opinion variable (Z_{1t}) was coded 1 whenever there was a plurality of public opinion favoring decreased defense spending. This occurred in 1969, 1971, 1973-74, 1976, and 1982-83. Otherwise, this variable was coded 0. The positive opinion variable (Z_{2t}) was coded 1 whenever there was a plurality of public opinion favoring an increase in defense spending. This occurred twice, in 1980 and in 1981. Otherwise, this variable was coded 0.

Notes

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1. Prior to the 1974 volume of the *SIPRI Yearbook*, official Soviet budget figures were simply converted to dollars using a purchasing power parity rate. Beginning with the 1974 volume, however, SIPRI made adjustments to the single line item Soviet budget figure by estimating "budget residuals" in other categories that might be expected to hide military outlays, e.g., research and development totals in the science allocation in the All-Union budget. This approach is very similar to that advocated by Lee (1977). Since 1979, SIPRI has published what it terms a "compromise" figure that is somewhere between official Soviet pronouncements and CIA estimates. SIPRI's estimation procedure, like that of the CIA, appears cloaked in secrecy. Upward revisions in recent volumes reflect changes in the purchasing power parity rate. For further details, see SIPRI (1974, p. 172 ff.; 1979, pp. 28 ff.; 1985, pp. 250 ff.).

2. There is a difference between defense costs and defense expenditures. The spending estimate does not necessarily reflect the costs to the Soviet Union

of its defense effort (Kaufman, 1985). With respect to the accuracy of these estimates, much of the controversy focuses on the "building block," or "direct-costing," method that the CIA employs. This method "involves defining the set of activities which make up the US defense effort, estimating the magnitude of comparable Soviet activities in physical terms, and applying US prices to the Soviet activities" (Burton, 1983, p. 86). Based upon this procedure, the CIA has recently concluded that the Soviet Union outspends the U.S. by a wide margin. Some have taken issue with the technical validity of dollar-dollar comparisons, the various weights assigned by the CIA to different components of the Soviet defense effort, the "index-number problem," etc. (Burton, 1983; Cockle, 1978). Others have taken issue with the "bottom line." Holzman (1980, 1982), in the only extended study of the index number issue in military expenditure comparisons, argues that the CIA routinely produces dollar estimates that inflate the true value of Soviet defense activities. The opposite view is offered by Lee (1977) and Rosefielde (1983). Our intent is not to resolve or even address the issues surrounding the accuracy of the CIA estimates. Instead, we acknowledge the controversy over how much the Soviet Union spends for defense, and turn our attention to the effect(s) of this estimate on the U.S. defense expenditure policy-making process.

3. Given the secrecy surrounding this estimate, determining which group(s) within the CIA is/are responsible for its production is difficult. Richelson (1985, p. 28) identifies the Office of Strategic Research, which is housed within the Directorate of Intelligence, as the production source for *A Dollar Cost Comparison of Soviet and U.S. Defense Activities, 1968-1978*.

4. The Soviet estimate is presented in a variety of different metrics, depending upon whether the analyst chooses current or constant roubles/dollars. We recognize that the choice of metric will have an impact on the magnitude of the estimate; we have chosen to characterize the estimate in terms of current dollars, since this is the metric in which the defense budget decisions are presented.

5. As a check on the specification, we estimated an equation containing all six variables for each actor, using ordinary least squares (OLS) regression. In addition, collinearity diagnostics were performed on each equation (cf. Belsley, Kuh, and Welsch, 1980) and highly collinear factors were identified. These equations proved—from both a statistical and substantive point of view—to be the best.

6. The base year for predicting levels is 1967, since our first observation on ΔD_3 is the change from 1967 to 1968.

7. The sum of these five factors does not equal \$160,058. The difference is due to the accumulated error.

8. The timing problems arise because if one

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follows the logic of our argument in operationalizing the Soviet estimate, it is necessary to explain X_{1t-3} with variables whose subscripts would be t , $t-1$, and $t-3$.

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THE POLITICAL ECONOMY OF THE TARIFF CYCLE

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How can protectionism and "free" trade succeed one another? Our answer focuses on the changing balance of private actors' political demands. These actors acquire interests in tariff policies because their assets are spatially concentrated, and trade in these assets is subject to various limitations. Actors in regions experiencing no new investment in an established industry ("old" regions) have interests that sometimes differ from those in regions where there is new investment. We show that old regions have no reason to be involved in tariff politics at business cycle peaks; during troughs, whether a state becomes more or less protectionist depends, ceteris paribus, on the relative political strength of old import-competing and old exporting interests. If old import-competing industries outweigh the old exporters, then protection will tend to increase at the trough and decrease at the peak of a business cycle; the opposite result occurs when old exporters are more influential.

Although there is an elaborate normative theory of international trade and tariffs, political scientists and economists have been less successful in developing an adequate positive theory of tariffs. One feature of tariff policy is that tariff levels tend to move in a cycle coupled with the business cycle. Tariff barriers are lowered during periods of relative prosperity, only to be raised again during world recessions. Although the existence of a "tariff cycle" coupled to the business cycle is commonly assumed by journalists, policy makers, and some scholars (Strange, 1979), the

underlying logic of such a cycle and its relation to the level of economic activity have never been fully explored.

An understanding of the politics of tariff making is of course important in its own right: the regulation of international trade is a ubiquitous policy tool in capitalist states. (Indeed, commercial policy antedates fiscal and monetary policy literally by centuries.) It is also important as a means to understanding some broader questions: Under what circumstances will interest group demands be met by countervailing pressures from other interests? How can a political

system move from liberal to nonliberal policies and back again? How is it possible for the entire international capitalist system to oscillate between periods of relatively free trade and relatively tightly managed trade?

It is natural to begin the task of constructing a theory of the politics of protection by an analysis of the effect of restrictions on foreign competition upon the distribution of domestic income. Political scientists have studied ways by which interest groups with identifiable stakes in particular protectionist measures press their demands (Bauer, Pool, and Dexter, 1963; Schattschneider, 1935). Economists have attempted to identify the structural characteristics of an industry—size distribution of firms, labor intensity, geographical dispersion—that tend to increase the industry's political leverage (see, for example, Lavergne, 1983, and the references therein). Published analyses typically examine protectionist pressures in terms of industry interests. The focus on the industry implicitly assumes that once a productive input is employed in a particular industry it is committed to that industry and cannot easily be redeployed to other industries. Therefore, the earnings of these committed inputs are tied directly to the level of demand experienced by the industry to which they are committed.

The effect of commercial policy on the distribution of income is different when factors of production are mobile than when they are not. If factors were not committed to use in particular industries, but could be easily redeployed in response to shifts in the pattern of product demand, then factor owners would not be concerned with any particular industry, but with the economy-wide demand for their particular input. Commercial policies that expanded the demand for labor relative to capital would be favored by labor; the converse would also be true. Therefore, the lines of political cleavage

would fall between labor and capital on tariff politics (see, for example, Stolper and Samuelson, 1941).

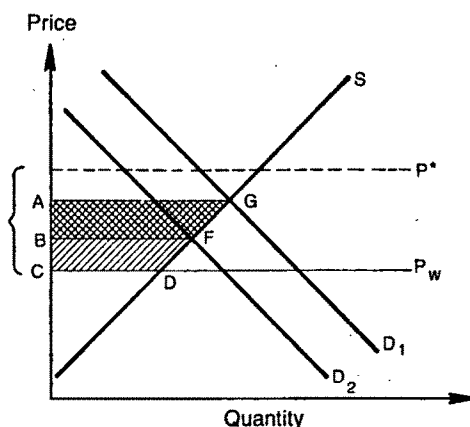
If factors were mobile, we should not expect to see a high degree of correlation between the patterns of lobbying activity of labor and capital. Conversely, if resources once committed to an industry have returns dependent on that industry's level of activity, then we should see owners of both labor and capital *within a given industry* taking the same position on protectionist measures. Magee's (1978) study of lobbying activity found that owners of both labor and capital within a given industry tend to take the same position on protectionist measures. This finding supports the assumption, inherent in existing studies, about industries constituting interest groups.

While previous studies focusing on industry interests in protection have enhanced our general understanding of the politics of protection, it is apparent that something important is missing from a positive theory of tariffs based solely on the analysis of industry interests. The notion of rent seeking implies that owners of industry-specific factors should be as vigorous in their defense of existing protection as they are in seeking additional protection. Furthermore, as Figure 1 illustrates, both the losses suffered by reductions in protection and the gains secured by increases in protection are greater when such changes occur in high-demand conditions than when they occur in low-demand conditions. Therefore, if industry-specific forces in import-competing industries *alone* were to produce the temporal pattern of changes in the level of protection, we should observe barriers rising when industry demand is high and possibly eroding when industry demand is diminished. This is in direct conflict with the evidence (Gallarotti, 1985; McKeown, 1984; Takacs, 1981) that trade barriers tend to be raised when industry demand is low. Of course, it is

often the case that when one industry is experiencing a state of low or high demand, many other industries are also experiencing a similar condition. Therefore, a state of demand cannot readily be identified as a factor that can account for variations in levels of protection across industries at any specific point in the business cycle. For this reason, it is best to consider theories based on industry-specific factors alone as theories which explain interindustry differences in the average level of protection afforded over the course of a business cycle, rather than an explanations of changes in the level of protection at any given time. A fully adequate theory of tariff policy should account not only for which interests receive protection, but when.

The theory we present accepts the general notion that tariffs are shaped by interest group political activities, but we conceive of interest groups in a way that departs significantly from previous treatments. We assume that some resources are not only industry specific, but also *place specific*. The introduction of place-specific resources in conjunction with consideration of the pattern of ownership of these resources allows us to identify an intertemporal asymmetry of interests among members of the same industry. In particular, if the typical individual owns a geographically diversified portfolio of assets, place specificity would add nothing to the analysis, since the gains to any one industry from rent seeking (no matter where located) will be offset by corresponding losses to others.¹ However, most individuals are not so diversified. While ownership of some assets is diversified geographically, ownership of others is not. Because the assets individuals own tend to be concentrated where they live, place specificity plays a crucial role in the theory, and allows us to account for the relation of tariff to business cycles.² The consideration of place-specific resources also yields

Figure 1. An Industry's Gains from Tariffs Under Conditions of Low and High Demands



P is the prevailing world price; the tariff, t , raises the price of imports to P^* . Under conditions of high demand, D_1 , the rent accruing to protected industries is thus the polygon $AGDC$; under conditions of low demand, D_2 , the rent is only $BFDC$.

a set of hypotheses on tariff coalition formation that have not heretofore been discussed.

In what follows, we first show how the economic stakes of actors change across regions and industries, and over time. We then consider factors that mediate between the economic stakes and the expression of effective political demand. Finally, we present some testable hypotheses that emerge from the theory, and consider empirical findings gleaned from other studies.

In order to make progress on this task we adopt the following strategy. First, we attempt to explain changes in the pattern of societal demands for tariffs. We do not claim to have a complete theory of tariffs per se, because we have not modeled the "supply side"—i.e., the government. Our reading of public choice literature on the supply of regulation (e.g., Peltzman, 1976) leads us to conclude that supply-side dynamics are generally complementary to those of the demand side. The

political science literature on the domestic politics of international economic relations (e.g., Katzenstein, 1978) advances several considerations that are present in individual cases, but that need not be contained in a general theory. Our theory thus deliberately pursues the logic of pluralist theories, to the deliberate exclusion of alternative theoretical perspectives that assign more pride of place to state actors' motivations. Second, we assume tariff increases in one country will be met by retaliatory increases in other countries. This rules out the use of tariffs as a device to increase aggregate demand. Our theory accounts for tariff policy changes on other grounds.

Third, our analysis of nontariff protection is limited. Our theory demonstrates why the general pattern of societal demands for nontariff protection will follow that of demands for tariff protection, and also suggests a circumstance when an industry might seek nontariff protection rather than tariffs, but that is the extent of our analysis of this topic. Nontariff protection often takes the form of administrative action triggered by individual petitioners, and therefore lacks the coalitional dynamics of "pure" tariff politics. Moreover, nontariff protection can be defined very broadly, so as to encompass virtually any government policy that might yield economic benefits for a population of asset holders. In any specific historical context, the array of possible governmental actions may be quite large, and we could not hope to develop a general theory that would encompass all the political choices an interest group might face regarding the type of governmental action it should demand.

Finally, we adopt the simplest and most common assumption about the decision-making processes of actors: utility maximization. We do this because we wish to highlight as sharply as possible the importance of certain structural features of a capitalist economy in producing certain

political results. While the departure of actors' actual decision processes from utility maximization may well account for some important aspects of tariff politics (McKeown, 1984), the argument we present is designed to demonstrate that structural features have a potent effect on tariff politics, independent of whatever argument we wish to make about decision processes. Our argument does not depend upon actors being able to make highly sophisticated and accurate forecasts or highly complex calculations of expected net benefits. We are thus optimistic that the argument is robust with respect to many reasonable departures from orthodox utility maximization.

Actors' Economic Stakes in Protection

Overview of the Essential Ingredients

We present a theory of international tariff levels in which spatial characteristics of industries and regional representation in the political process are central. In particular, we conceive of a nation as divided into distinct political/economic regions. Economically, each region has a particular industrial base. In order to highlight our argument, we will assume that each region possesses a single dominant industry, either import-competing or exporting. Politically, each region has (elected) representation in the national government, and each regional representative is motivated to secure regional benefits at the lowest possible cost. Voters vote their regional pocketbooks: they elect candidates who deliver (or seem to deliver) regional economic benefits.³

Industries tend to shift the regional concentration of their capital within a nation over time. This relocation of industrial activity is just the predictable response to shifts in interregional comparative advantage. Business location decisions are

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responsive to changes in demand patterns due, for example, to population movements, differing regional tax environments, various regional institutions such as unionization laws, transport cost changes, and so forth. Thus we distinguish between *old regions* and *new regions* within an industry. Note that it is not necessary to our theory that each industry be located in both an old and a new region within a single country. Some industries may have only old regions, while others may have only new ones.

We define *old regions* as those in which the dominant industry's productive capacity is contracting at some depreciation rate, even when national industry productive capacity is stable or growing. Correspondingly, *new regions* for an industry are characterized by capital accumulation. Therefore, although *total* industry net investment may be zero, *regional* industry net investment may be either negative or positive. This is a crucial asymmetry among regions that are dominated by a given industry.

Individuals are deemed to be interested in commercial policy to the extent it can be used to increase individual net wealth. Each individual owns a portfolio of assets consisting of quantities of capital and labor. The capital may have both a nonhuman and human component. The human capital, we assume, consists of industry-specific skills. A possible exception is provided by new entrants to the labor force, who are likely to possess relatively small human capital endowments and lower migration costs. Young workers are thus freer to respond to adverse labor markets by exiting a region. The nonhuman capital is of two varieties: ownership claims on various industries and place-specific assets such as land, housing, and so forth. We postulate that stock ownership has no geographic correlates, because stock is typically traded in national markets in which buyers typically diversify their portfolios.

However, ownership of physical assets (homes, small businesses) is geographically concentrated. Owner-managers obviously have both types of assets, but our prototype capitalist is assumed to have a stock portfolio selected without regard to geography.⁴ This is another important asymmetry in the model. Labor, as opposed to human capital, is assumed to be specific to no particular industry, but is regionally specific to the extent there are migration costs. Human capital is similarly place-specific, to the extent that skills are tied to the region's nonhuman capital, because the nonhuman capital in a given region is of a particular "vintage."

The argument proceeds as follows. First, we discuss an individual's asset portfolio and argue that it is disproportionately loaded with regionally-specific assets. Second, we focus on the effects of tariffs, and conclude that old import-competing regions benefit most from protection during cyclical troughs of the business cycle and do not benefit at all during peaks. This asymmetry is the key to our theory of the tariff cycle. Finally, we discuss the interests of other types of regions and explain how the changing economic stakes of different regions generate a tariff cycle coupled to the business cycle.

Individual Wealth and Interest in Protection

In principle, any individual's asset portfolio could be as diversified as desired. Assets could be diversified across industries, skills, and regions. In fact, impediments such as incomplete insurance markets (due to moral hazard and adverse selection problems), legal constraints on trade in human capital, certain tax laws (such as those favoring home ownership), and monitoring costs bias many asset portfolios toward a disproportionately high holding of assets specific to one's own region. For example, over 60% of

Americans own their homes, or at least the associated debt. Similarly, local businesses are regionally specific in regard to owned physical capital, reputation, and the presence of nearby customers. Furthermore, for many individuals, their potential labor services and existing human capital endowments are their most important assets. These assets are place-specific to the extent that job search and migration costs outside the region are significant or job skills are tied to uniquely place-specific capital (as, for example, with eastern underground coal miners versus western strip coal miners).⁵ However, restrictions on slavery and on indenture prevent effective ownership of others' human effort, and so inhibit asset diversification.

All of these forces conspire to give individuals a special interest in the value of their particular region's assets. Thus, there is a *coincidence of spatial economic interests* among a region's residents.

Of course, individuals also own claims to the various industries' assets, both at home and abroad, and so are interested in particular industries regardless of their location. Claims to industry assets, however, may be diversified through the stock market across industries and across regions; we argue that this diversification dilutes the portfolio owner's interest in the health of any particular industry to the point where political action is not rational for them. Consequently, an individual's interest in any particular industry depends especially upon how increased industry activity translates into increased demand for regionally specific assets. For example, the Pittsburgh steel worker is less interested in the American steel industry than in its health in Pittsburgh.

Old Regions and New Regions

Any change in the economic environment typically alters the pattern of efficient production between nations. Over time the composition of a nation's exports

and imports responds to such changes. The United States, for example, has evolved from an agriculturally based economy to a manufacturing and service-oriented economy. Just as importantly, changes in the economic environment induce shifts in the pattern of regional comparative advantage within a nation. In the United States, coal and ore production have moved westward. Steel production has relocated to both the South and the West. The textile industry has contracted in New England but expanded in North and South Carolina.

This relocation of production activity creates for any given industry "old" and "new" regions. In old regions industry is contracting, while in new regions it is expanding. Old regions tend to have a high proportion of the industry's capital stock, older vintages, and a larger work force; most importantly, they exhibit negative net investment. Firms are targeting new regions for new investments, but the inhabitants of these regions cannot easily determine their good fortune until some investment decisions are implemented. Since international and intranational comparative advantage shifts are occurring simultaneously, we could find examples of all four possibilities: old import-competing industry regions; new import-competing industry regions; old exporting industry regions; and new exporting industry regions.

An excellent example of this phenomenon is provided by the case of steel. As can be seen in Table 1, steel production in Pennsylvania peaked in 1973. In 1963 Pennsylvania produced 23% of the U.S. raw steel; by 1983 it was producing only 15%. By contrast, even though steel production in the U.S. peaked nationally in 1974 at 145.7 million tons and has been declining ever since, steel production has expanded in both the central and southern states. In the 1981-82 downturn, the decline in production in Pennsylvania was more drastic than in the newer regions; during the recovery of 1983, production

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Table 1. Raw Steel Production in One Old and Two New Regions, 1963-1983
(in millions of tons)

Year	Pennsylvania	Central States ^a	Southeast ^b	Total U.S.
1963	25.192	3.336	3.677	109.260
1964	30.515	4.076	3.826	
1965	31.998	4.425	4.010	
1966	32.122	4.528	4.196	
1967	29.881	4.644	4.268	
1968	31.018	4.810	4.453	
1969	32.791	5.300	4.674	
1970	30.031	5.198	4.629	
1971	27.655	4.908	4.601	
1972	30.416	4.843	5.562	
1973	33.925	5.596	5.743	
1974	33.535	5.751	5.619	145.720
1975	25.761	5.399	4.795	
1976	26.696	5.079	5.403	
1977	25.737	6.753	5.484	
1978	28.070	7.845	6.444	
1979	28.213	8.260	6.636	
1980	23.502	8.453	6.890	
1981	24.066	9.068	6.497	
1982	10.892	5.417	4.419	
1983	13.000	10.678	5.277	84.615

Source: Data is from the American Iron and Steel Institute, Washington, D.C.

^a"Central states" denotes Minnesota, Missouri, Oklahoma, Texas, Nebraska, and Iowa.

^b"Southeast" denotes Virginia, West Virginia, Georgia, Florida, North Carolina, South Carolina, and Louisiana.

recovered promptly in the newer regions, but was only anemic in Pennsylvania.

In the old regions regional income is falling, and so the return to place-specific assets declines as well, since the demand for these assets depends upon regional income. Less place-specific factors of production—new labor force entrants, new investment capital—emigrate from the old region. New regions will grow. The question arises, When will the interests of residents in both old and new regions coincide on tariff policy? The answer depends upon the level of economic activity.

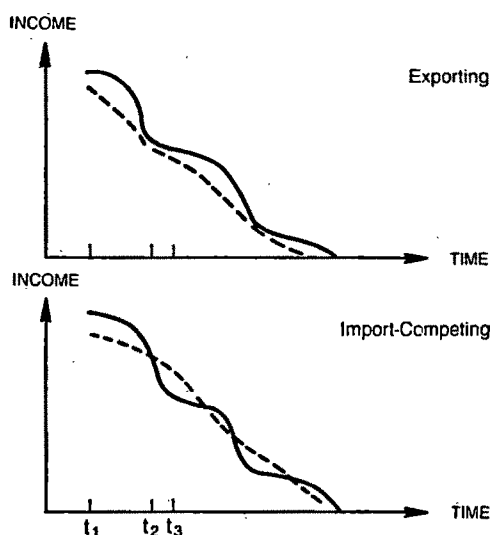
The Business Cycle

We characterize the *business cycle* as exogenously induced fluctuations in the general price level. To the extent that

labor's nominal wage is sticky in a downward direction, unemployment will result during cyclical downturns. Capacity utilization of plant and equipment will typically decline during the downturns. Since the business troughs are economy wide, both old and new regions may experience unemployment, although the old region will be more severely affected, since the plants in old regions typically have higher operating costs. Regional income thus moves with the general business cycle in both old and new regions. The cyclical income variations, however, trend around secular decline in the old regions and secular rise in the new regions, as illustrated in Figure 2.

The pattern of demands for protection that emerges depends upon how regional interests change over the business cycle, and how the changing interests translate

Figure 2. Real Regional Income in Old Regions, With and Without Tariffs (the broken line denotes "tariff")



into changing political behavior. Consideration of comparative advantage characteristics and the pattern of capital investment yields a 2×2 typology of regions: in comparative advantage terms, regions are import-competing or exporting; in capital investment terms, regions are either old or new.

Asymmetrical Effects of Protection over the Business Cycle

It is important to distinguish between the effects on a region of a change in the average level of protection afforded an import-competing industry over the course of a business cycle, and the changes in protection at different points in the cycle. A change in the average level of protection affects the equilibrium level of capacity in the industry. New regions, in which the industry's capacity is being located, have a vital interest in any change in the average level of protection afforded an industry. Old regions, which are losing capacity in any event, do not

share this interest. They cannot expect a tariff increase to prevent the shift of capacity to other locations where profitability is higher.⁶ *A fortiori*, while it is in the long-run interest of new regions to resist any declines in the average level of protection afforded their import-competing industry, the old region has no long-run interest in tariff protection, since, in the long run, the region's income will become independent of the national productive capacity of the given import-competing industry.

A variable of much greater political significance is the short-run economic consequences of a change in the level of tariff protection—even if actors expected an offsetting change in tariffs at a later time. The short-run effect of a change in the level of protection is to change the demand for output from existing capacity. Old regions, which have capital vintages with the highest operating costs, experience the greatest reductions in utilization rates when demand declines. Therefore, old regions have the greatest interest in increased protection at the trough of a business cycle, while at the peak of a cycle, when utilization rates are already high, an old region secures no additional regional income via an increase in tariffs. The asymmetry of the effects of protection on old import-competing regions is illustrated in Figure 2.

New regions will gain, but for an equivalent tariff these gains will not be any greater at the peaks than during the cyclical downturns. Furthermore, before a tariff is enacted and investment begins, new regions may not be aware that they will benefit from protection. After all, before the tariff there may be little or no capital—and, correspondingly, employment—in place in the region. Since the prospective gains are less visible, the new regions may be relatively less protectionist during the peaks than they would be if they had complete information about the effects of a change in tariff policy on in-

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vestment in their regions. Whether they are is not central to our theory, but this possible uncertainty about the location of future activity does add an Olson-like twist to the argument. Tariffs are sometimes explained as occurring because the specific gains are obvious, but the general costs are uncertain and invisible. Here it is the gain to new regions from tariff protection that is uncertain, and so relatively invisible.

The distance between the "no tariff" and the "cum tariff" schedules in Figure 2 reflects the asymmetrical effects of protection in old regions over the business cycle. At the cyclical peaks, capacity utilization is high and the regional benefits of increased investment and production accrue only to new regions. There are nonetheless consumption costs associated with the higher tariff-protected prices, and so real income is lowered on this account. In Figure 2, at time t_1 , for example, the tariff is accordingly shown on balance to lower real income at the peaks. During the cyclical troughs the consumption costs still lower real income. However, now there are offsetting gains in the old region, since the tariff leads to increased capacity utilization and regional employment, which in turn translates into increased regional income and demand for region-specific assets. Thus, at time t_3 , for example, the tariff is shown to raise real income.

The discounted present value of the change in real income due to protection is represented by the difference between the cum tariff and no tariff schedules at each point in time, summed and discounted back to the present. For example, the addition to regional income from a tariff levied at time t_3 is given by the area between the two schedules beyond t_3 . Of course a positive discount factor (interest rate) insures that the real wealth gain is not infinite, because income gains very far into the future receive very little weight.

Two forces now work to enhance the

attraction of tariffs in cyclical downturns. First, during cyclical peaks, tariffs actually impose net income losses. The net gains during the downturn are pushed into the future and so, given a positive discount factor, the present value of future protection is diminished. Thus, for example, at t_1 a tariff levied immediately is of less present value to the old region than a tariff levied immediately at t_2 (McKeown, 1984).

Second, the distance between the no tariff and cum tariff schedules depends upon how much protection is extended to other regions in the overall tariff package. Each tariff levied on any commodity leads to higher consumption costs. Only if the commodity is produced locally and if there is local unemployment will there be compensating favorable consequences for regional industrial activity, and so gains to owners of place-specific assets. (In terms of Figure 2, each tariff for an industry that is not locally important lowers the cum tariff schedule. Indeed, high levels of broad protection can lower regional income below the no tariff income level.) This introduces a bias toward protection during business cycle troughs, since there is more to gain from protection for any particular old import-competing region. Consequently, the gains can remain positive for at least some protectionist coalitions, even though the consumption costs rise with each additional protected industry.

Industries thus find themselves in an environment wherein a tariff on their output holds widest regional appeal during general business downturns. During business cycle troughs there is a community of interest between old and new regions in securing tariffs to protect regional income. Conversely, during cyclical peaks old regions have no great interest in protection, since the gains from changing policy will accrue only to the new regions. Of course, tariffs carry costs for all regions, and the complicated prob-

lem for legislators is to secure protection for their own region's industry while giving up the least amount of protection possible for others, subject to the constraint of winning any vote.

Tariffs and the Business Cycle in Exporting Regions

Import restrictions on one set of goods reduce the relative prices of other goods. During business cycle peaks such restrictions, in conjunction with downward wage rigidity, will lead to at least temporary unemployment in some unprotected sectors and a decline in these regions' asset values. While import restrictions can theoretically increase aggregate domestic employment during the trough of a business cycle, we discount such a possibility, since "beggar-thy-neighbor" tariffs invite retaliation. Such retaliation suppresses foreign demand for exportables and insures that gains to import-competing regions will be offset by losses to export-oriented regions. Exporting (and other nonprotected) industries therefore lose rents from tariff-induced shifts in demand at every point in the business cycle.

Just as the imposition of tariffs has asymmetrical effects across old and new import-competing regions, the removal of tariffs has asymmetrical effects across old and new exporting regions. Old exporting regions, by definition, will not receive increased income flows during business cycle peaks from the removal of domestic tariffs on imports. At the business cycle peak it is new export regions that expand if tariffs are removed. Exporters will gain even more if domestic tariff reduction prompts a symmetric foreign response. Conversely, at the trough of a business cycle, an old export region may gain more than a new export region from tariff reductions.

Given the symmetry between old

import-competing and old exporting regions, our theory implies that if the commonly alleged type of tariff cycle is observed, then the political weight of the old import-competing regions must exceed that of the old exporting regions. When import-competing industries are dominated by old regions, while exporting industries are dominated by new regions, the dynamics of tariff politics will produce a tariff cycle counter to the business cycle.⁷

Translating Economic Stakes into Political Demands

In this section we explore the political dynamics that arise from actors' stakes in achieving or eliminating protection. Although an industry may have a large economic stake in tariffs, its political behavior is mediated by its perception of the likelihood of political success. This in turn is a function not only of its own politically relevant resources, but also those of other actors, and the perceived likelihood that a winning coalition can be formed. Although an industry may obtain nontariff protection through unilateral appeals to the appropriate administrative agency, some nontariff and most tariff protection can only be obtained by some form of collective political action involving multiple sectors of the economy. In fact, governments committed to free trade might tolerate or even promote nontariff barriers awarded via administrative action, in order to divert the energies of actors with the largest stakes in protection so that they do not attempt to assemble a protectionist coalition. The central concern of this section is to identify any factors that impart asymmetries in the political strength of the four types of regions, and to sketch the possible coalition dynamics.

Political Resources and Collective Action Possibilities

One critical determinant of the level of political effort to be expected from a firm is whether it can reasonably expect its efforts to have a substantial effect on political outcomes. Such an expectation is reasonable if the firm is very large, for then even a modest proportion of its resources might be quite large, both in absolute terms and in terms of the total resources devoted to influencing political outcomes. Such an expectation would also be appropriate if the firm were acting in concert with other like-minded firms, so that their resources could be pooled to achieve a common goal. The latter condition can occur when firms have established a formal trade association, or have established a history of working together on political issues. In such circumstances, the well-known "free rider" problem could be at least partially surmounted.

Older regions are apt to be more politically potent than new regions, at least during the initial phases of an economic downturn. If, as Mancur Olson (1982) argues, the likelihood of being organized is an increasing function of the length of time firms in the same region have been established, then there is a *prima facie* basis for believing firms in old regions will be more politically organized than firms in new regions. If the size of firms is an increasing function of the length of time that they have been operating, then there are likely more very large firms in the old regions than in the new ones. This also biases political influence towards the old regions. Finally, as noted above, the future winners from tariff changes are much more difficult to identify than the present losers. As a consequence, organized interest groups representing the interests of new regions are much less likely to be politically significant than those interest groups representing the interests of old regions.

An additional asymmetry is temporal.

In noting that the membership and general health of farm-oriented interest groups has tended to move counter-cyclically, Salisbury (1969) contends that members' contributions to such organizations constitute "luxuries" that they abandon in hard times. Coneybeare (letter to author, April 1983) notes that Chamberlin's (1974) work on collective goods provides a formal illustration of this argument. As depressions reduce incomes, under plausible income elasticities of demand, members will contribute less to organizations providing collective action benefits.⁸ At first glance, this would apparently serve to undercut the political potency of all groups in cyclical downturns. However, if starting up a group is more costly and difficult than maintaining it, then the cyclical shifts in incentives for group contributions would tend to be more debilitating to those interests that had not yet formed groups than to those groups that already had (perhaps in a previous cyclical boom). The latter could conceivably survive the downturn, though with reduced contributions; for the former, the downturn might create an effective barrier to political entry. This is another factor granting an edge to old regions. Moreover, for organizations in old regions, the increased benefits accruing from collective action in times of depression will counteract, to some extent, the effect of declining incomes on members' contribution levels. As argued above, there is no obvious counterbalance available to the new regions.

The general effect of these asymmetries is to strengthen the hand of old regions as opposed to new regions. With more large firms, and more time in which to form trade associations, the old regions have an important political advantage.

Coalition Formation

Coalition formation is relevant to the analysis of the tariff cycle for two reasons. First, a coalition is generally

necessary to achieve passage of tariff legislation. Industries seeking protection during periods of general prosperity may be forced to rely on unilateral action through bargaining with state agencies that can grant nontariff protection, because there are not enough other industries available to act as coalition partners. During a time when business conditions are generally depressed, there are many more potential coalition partners for such a protectionist bid than there are during more prosperous times. Thus, we posit an increase in efforts to enact protectionist legislation at such times simply because of the presence of more protectionist interest groups.

A second point regarding coalitions is that, because of multiplicity of winning coalitions may be possible, each with its own characteristic set of legislative side-payments, there could be a multiplicity of possible political outcomes for any given economic situation. In particular, it is important to understand how sectors that may not initially have a stake in tariffs may acquire tariff protection as a product of the coalition formation process.

A plausible initial presumption is that, because each import-competitive region desires a tariff only on its own output, it will be willing to support tariffs on other commodities only insofar as is necessary for forming and maintaining a winning coalition. Though such regions benefit from tariffs on their own commodities, they are of course hurt by tariffs on other goods. Conceivably, owners of regional assets will be worse off under "tariffs for all" than "tariffs for none"; thus, they will have a vested interest in holding the size of protectionist coalitions to the minimum necessary to win. Nonetheless, policies of "tariffs for all" are not uncommon: prominent historical examples are furnished by the Hawley-Smoot tariff of 1930, the German tariffs of 1879 and 1897, and the French tariffs of the 1830s and 1840s.

Important to understanding "tariffs for

all" is Shepsle and Weingast's (1981) demonstration that uncertainty about being included in a winning coalition induces legislators to collude on "universalistic" allocation criteria for "pork barrel" legislation. When the probability of being excluded is sufficiently great, and the incidence of costs and benefits in one's own district meets certain minimal requirements, legislators will agree to avoid attempting to form minimal winning coalitions, and instead admit all who desire to join.

Another way universalistic coalitions may arise is in situations where legislators are not necessarily uncertain which coalition will win, but where there is no method whereby members of a minimal winning coalition can exclude other players from joining that coalition. Assume there is some possible coalition of import-competitive firms and regional actors that proposes a package of tariffs benefiting each region in that coalition. Assume further that the benefits to each member of the coalition are positive, and that the benefits to nonmembers of the coalition are negative. Under these circumstances, the temptation of nonmembers to join the potential winning coalition will be quite strong. A nonmember who joins the coalition changes his payoff—if the legislation passes—from some negative value to some less negative or positive value. Since the cost of being a nonmember increases as more and more commodities acquire protection, the pressures to join the bandwagon become stronger as more and more others do so. Meanwhile, even though riding on the bandwagon becomes less and less attractive as it becomes more crowded, no single rider finds it rational to dismount, since doing so will not change the voting outcome, and will only result in a worse negative outcome than would be obtained by staying on board (see, for example, Schelling, 1973).

An additional possibility is created

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when some actors in the political system may have been excluded from full participation by the collusion of present participants. Limitations on the suffrage in the nineteenth century are a clear example of this; a similar phenomenon, though with less immediate applicability to tariff politics, is provided by the exclusion of Communist parties from governing coalitions. The current winners of protection may calculate that it is to their long-term advantage not to exclude any participants from a protectionist coalition, because to do so might aggrieve such participants so much that they would be tempted to alter the outcome by widening the circle of participation. Actors will therefore accept outcomes that are suboptimal in the short run, because of their fear of the long-run costs inherent in contending with the entry of new groups into the political process. If there are no groups outside of the process, this collusive dynamic obviously is absent. Such considerations seem to have played a role in French tariff setting in the period 1830-1860, and in the formation of the tariff coalition of iron and rye in imperial Germany.

When the above conditions do not obtain, and when victory is sufficiently probable that existing coalition members are no longer willing to dilute the benefits of protection, the coalition leaders will act to exclude some claimants for protection. The likely candidates for exclusion would be those commodities for which protection generates widespread and visible consumption costs and few regional benefits, and that bring relatively little political clout to a protectionist coalition (too disorganized, too concentrated in a few regions, not particularly important in any region).⁹ Of course, import-competing actors excluded from a protectionist coalition can then improve their position in two ways: either they can join with the exporters to attack others' protection, or they can join the protectionist coalition at a later time:

The opposite situation in tariff coalitions is one in which there are very few potential members of a protectionist coalition. For example, in an economy characterized by overall growth, but with a small number of troubled industries, those industries in trouble might well face difficulties in assembling a large enough coalition to have any chance at all of obtaining any legislative action. Under these circumstances, we argue, such industries would be more likely to attempt to obtain some form of nontariff relief through administrative action, such as a petition for an International Trade Commission ruling to restrict imports in that industry.

An additional consideration in assessing likely coalition dynamics is the presence or absence of additional constraints or costs on the tariff issue arising from linkage to other foreign policy issues. Lowi (1964) argues that U.S. tariff politics after World War II changed in nature, because the greater U.S. role in world affairs led to much greater concern over interdependencies between U.S. tariff policy choices and outcomes on foreign policy issues not involving tariffs. For example, high levels of protection against European producers would have reduced their ability to finance rebuilding of their economies, and would have done nothing to correct the postwar dollar shortage. Such outcomes would have been inconsistent with the general foreign policy objective of establishing strong, capitalist economies in western Europe in order to resist both domestic communism and Soviet invasion.

It should now be clear that a wide variety of coalitional arrangements are consistent with the regional analysis which forms the core of our argument. Since so many forces come to bear on the actual enactment of tariffs, almost anything can happen in any particular case. However, our theory does imply a certain regularity in the pattern of protection, holding other forces constant. Empirical tests of the

theory are therefore possible, at least in principle.

Implications of the Theory

Our basic hypothesis is that old import-competitive regions share an interest with new import-competitive regions in tariff protection for an industry only during periods of depressed demand. During such periods, tariff protection will induce a higher rate of employment and higher regional income for an old region than that region would otherwise experience. However, since old regions do not share in the growth of an industry during periods of prosperity, they have no interest in obtaining protection in prosperous times. Furthermore, since legislative politics are often based upon a mutual accommodation of regional interests, it follows that the probability of putting together a winning coalition of protectionist interests is greater near the trough of a general business cycle, when a would-be protectionist can find the maximum number of potential coalition partners. Conversely, during cyclical peaks, the old regions would not only have no incentive to seek new tariffs, but would even lack incentive to oppose tariff reductions, provided that a resulting increase in imports did not create idle capacity in the old region.

The strength of a given old region's interest in protection for a particular industry, and therefore the probability of a particular industry securing protectionist support, depends upon the importance of the industry output to the region's income. Therefore, industries in the early stages of a regional shift that still have a large fraction of their productive capacity in the old regions are more likely to secure protection during a protectionist wave than industries whose productive capacity is preponderantly in new regions. We have also argued that regional interests

are more closely tied to the labor income than to the capital income generated by a given industry's output. It follows that industries generating large payrolls in such regions are more likely to secure regional support for protectionist demands than industries generating small payrolls in such regions. Moreover, since factor intensities are related to factor shares, other things being equal, more labor-intensive industries in old regions should be more successful in obtaining protection than less labor-intensive industries.

When old plants embody an older technology than that of new plants, variable costs of production are likely to be higher—or at least no lower—in the old than in the new regions. Consequently, a decline in demand for an industry's output will be met by a contraction in output in the old regions relative to the new regions. Therefore, if we look at several industries occupying both old and new regions, we will find that the greater the production cost differentials between regions, the greater the relative contraction and the greater the industry interest in tariff protection during a business downturn.

Another implication of a regional perspective on tariff policy is that when a country is sufficiently small in land area and labor is sufficiently mobile, the variability in tariff policies we have outlined will not occur. If the small country possesses a single industry, then that industry presumably will have already acquired what it regards as an optimum tariff; if the small country possesses multiple industries, the holders of place-specific assets will no longer have their fortunes tied to a particular industry.¹⁰ For retailers and the like who possess region-specific assets but who are not directly employed in the affected industries, there similarly is no reason to bolster declining industries at the expense of rising ones. Essentially, the tariff cycle will disappear, because the spatial asymmetries that drive

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changes in protectionist sentiment over the business cycle will disappear.

Our theory argues that the effects of tariffs are spatially differentiated. The political implications of this for the organization of interests are twofold. First, we will find interest groups involved in tariff politics that are organized along geographic lines. Organizations such as state and local chambers of commerce, or regional government or business organizations in the United States, may have a rational basis for involvement in tariff politics, because of the regional impact of tariff policies. Second, interest groups organized along sectoral lines will tend to have cleavages along geographic lines. We may witness different regions possessing separate trade associations in the same sector—as occurred in the textile industry in the U.S. (Galambos, 1966)—or semiautonomous regional groupings within national sectoral trade associations—such as occurred in the German iron and steel industry in the nineteenth century (Boehme, 1966). Furthermore, the theory implies that the geographic regions represented in protectionist coalitions will change over the business cycle. Interest groups from old import-competing regions will allocate substantial effort to securing a tariff only during cyclical downswings, because only at that time will protection yield substantial benefits.

Finally, the theory does offer an alternative explanation to attempts to account for changes in the openness of the international trading system in terms of a theory of "hegemonic stability" (Gilpin, 1975; Krasner, 1976). Very simply, if business cycles in the nations of the world capitalist economy are synchronized, and if old import-competing regions generally outweigh politically the old exporting regions, then an increase in demands for protection during global downturns will occur throughout the states of the system. This makes it more likely that any given state will defect from a liberal order. It

also makes it more likely that other states will respond to that defection by defecting also, since the costs of taking the "sucker's payoff" are now augmented by domestic pressures for a departure from an open system.

Relation to Previous Empirical Findings

Although we have not yet subjected our theory to empirical testing, we can gain some idea of its accuracy by examining whether its claims are consistent with previously published findings on the politics of tariffs. In this section we discuss several such studies, noting when the results obtained seem to be relevant to our theory and, if relevant, whether they are consistent or inconsistent with our argument.

Central to our argument has been the claim that when industries are enjoying healthy growth in demand, the pressures for protection from import-competitive interests will be reduced, since old regions—which, as we have argued, are especially likely to be politically potent—derive relatively little benefit from protection at such times. The finding of Cheh (1974) that individual U.S. tariffs are negatively related to growth rates for the sectors covered by the tariff is consistent with this argument. Riedel's (1977) finding that West German industries with high growth rates experienced relatively larger reductions in effective protection (but not in nominal protection) in the Kennedy Round is not inconsistent with our argument, but the case would be stronger if the relation obtained for nominal rates as well. Coneybear's (1983) observation that the average 1971 tariff levels for a sample of 35 nations were negatively related to the rate of growth of gross domestic product (GDP) is also consistent with our argument. His finding that 1902 tariffs were positively

related to the growth rate of GDP is vitiated by the fact that the values he used for growth rates were averages for the 42-year period from 1871 to 1913. A more reasonable procedure would have been to use the years just prior to the tariff observations. Takacs (1981) has analyzed requests by U.S. industries for nontariff protection, and finds that such requests are negatively associated with industry growth rates. Lavergne (1983) found that growth rates in U.S. employment had a moderately strong and expected negative relationship to protection only in the case of nominal U.S. tariff levels in 1964 and 1972, and for an index of nontariff barriers developed in the early 1970s. He observed a statistically significant but unexpected negative relation between growth rate in employment and the size of nominal tariff cuts made in the Tokyo Round, and obtained very weak and non-significant results for 1979 nominal tariff levels, effective tariffs in 1965 and 1972, and the size of Kennedy Round cuts. Marvel and Ray (1983) found that sectoral growth rates were positively related to reductions in sectoral U.S. tariffs in the Kennedy Round. They explain this in terms of a Peltzman-Stigler-Becker argument to the effect that rapid growth is a "windfall of good fortune," and that government officials will find it profitable to alter tariffs to share the windfall benefits with the consuming public. Apparently, their implicit argument is as follows: If a shift to high growth is associated with an increase in profitability, and if politicians obtain declining marginal political utility from actions that raise sectoral profits and increasing marginal political disutility from actions that increase consumers' losses, then the maximizing course for the government officials is to reduce the tariff for that sector. Our model does not investigate this supply side argument, but it does offer a complementary "demand side" explanation for the relation between growth rates and tariff protection.

A number of analysts (Constantopolous, 1974, and references therein; Helleiner, 1977; Pincus, 1975, 1977; Ray, 1981; and Reidel, 1977) have analyzed the cross-sectional distribution of protection in North America and western Europe. If we adopt the assumption that the amount of protection an industry receives is a positive function of its level of effective political demand, and that effective political demand is in turn a function of the size of regional effects, then the evidence is generally not inconsistent with our argument. Those receiving the most protection tend to be labor-intensive, low-wage industries. Labor intensiveness is consistent with substantial regional impact due to a large number of employees; low wage levels suggest sectors in western Europe and North America that are at a comparative disadvantage vis-a-vis competitors in the less-developed countries. However, low wages also imply modest effects on such regional variables as retail sales.

Pincus's (1975, 1977) analysis of the distribution of tariffs in the U.S. in 1824 finds that high tariffs are positively related to low proprietorial income shares. He accounts for this in two ways. First, he argues that when the proprietorial income share is low, the benefits to proprietors from a given tariff increase are larger than when proprietorial income shares are high.¹¹ Second, he also seems to treat low proprietorial income shares as a proxy for small numbers of proprietors (i.e., large firms), and then relies on an Olsonian argument about the greater ease of overcoming free riding in small groups. Our theory suggests a third reason why low proprietorial income shares might be associated with effective demands for protection: low proprietorial shares imply that, for a given size investment, the regional benefits are substantially greater than those that would be enjoyed by an industry with a high proprietorial income share, because of the larger payments to local labor.

Another important implication of our

theory is that interest groups active on the tariff issue will be organized on spatial as well as sectoral lines. Most political science analyses of tariff politics have implicitly adopted the sector as the meaningful unit of analysis; attention to the spatial characteristics of interest group activity has been unsystematic and incidental to the primary focus on sectoral alignments. The problem of distinguishing spatial from sectoral activity is further complicated because sectoral associations can themselves be organized along spatial lines; if the analyst is not sensitive to the spatial characteristics of interest group representation, and does not investigate the possibility that certain geographic sections dominate a given interest group, it is not possible to determine from reading the analysis whether there has been a significant spatial component in interest group activity. Nonetheless, we can pick up a few clues by a close reading of the literature.

Pincus (1975) notes that demands for protection in 1824 were spatially concentrated—that is, petitions for a given tariff tended to originate in identifiable regions. He argues that the cost of interstate transport and communication necessitated such a pattern. In addition, a legislature constituted by representation of geographically compact districts would, in any event, tend to channel political activity along regional lines. Of course, such a pattern of spatial activity is also consistent with our weak general hypothesis that activity on tariff politics has a significant spatial component. Schattschneider (1935, p. 125) devoted considerable attention to the spatial characteristics of interest group representation. His conclusion—that when a sector is strongly sectionally concentrated, “wide secondary activity” on tariffs occurs—seems to support our contention that the regional effects of tariffs are politically significant.¹²

Summary

Our account of the tariff cycle is based upon the conventional premise that tariff policy is shaped by the rent-seeking behavior of interested parties.

At a national level, individuals whose assets are tied to import-competing industries have a direct interest in tariff protection for specific industries, regardless of the general level of economic activity. However, the gains to import competitors from tariff protection and the losses to exporters from retaliation and increases in input costs are greater when times are good than when demand is low. Given this symmetry of interests between import-competing and export-oriented industries at the national level, the countercyclic nature of tariff and other trade barriers appears to reflect interest group forces that are not explained by simple reference to nationwide industry interests.

We argue that an explanation for countercyclical tariffs lies in the fact that not all regions whose interests are tied to an import-competing industry share in the benefits of protection in the same way over a business cycle. In particular, old regions, which have lost their locational advantage, cannot expect to share in the rents generated by protection when the general level of activity is high. However, when the industry is generally depressed, it is the older regions, where a large fraction of the idle import-competing capacity is located, that will benefit disproportionately from tariff protection. Therefore, an old region is more likely to join a protectionist coalition when its industrial base faces poor economic conditions than otherwise. Further, when an old region supports a protectionist coalition it is likely to be willing to endure higher costs (through the broadening of the set of industries for which protection is sought) than is a new region, since when it supports protectionism its own expected

gains from the reduction of idle capacity are greater than those of a new region.

The political dynamics of tariff seeking tend to reinforce the central role old regions play in the process. When an industry is a major part of the economic base of a region, citizens of that region who have a stake in the general economic conditions in that region are the industry's natural allies. The industry need not spend much of its own resources to secure political support for its demands from regional political representatives. By contrast, in areas where an industry might expand if given tariff protection, that industry need not be a major employer currently. Further, no region can be certain that a given industry will expand within its borders. Consequently, the industry has fewer natural allies outside its old regions, and must expend its own resources in those regions if it is to secure political support of their representatives.

To summarize, differences in regional interests create the following general dynamics: When general economic conditions are poor, the old import-competing regions will join the protectionist coalition, and pressures for tariffs will increase. Although export-oriented old regions will be joining a free-trade coalition at the same time, in a political system where old import-competing regions outweigh old exporting regions, the activation of the old regions on both sides will favor the protectionists. Conversely, during cyclical peaks, when both types of old regions desert their respective coalitions, the protectionist coalition will suffer a relatively greater loss in political strength. It is thus at the peaks that the free traders have their best winning chances.

Our argument also has a more general implication for the study of interest group activity. If our model is correct, the level of interest group activity on tariffs and many other forms of rent seeking ought to fluctuate with business conditions, with

intense activity most likely to occur at a cyclical trough. Such a relation between business conditions and interest group activity may account for the often-noted disparity in the findings of Schattschneider (1935) and Bauer, Pool, and Dexter (1963) on the levels of activity of interest groups. Whereas Schattschneider, studying decision making at the onset of the Great Depression, paints a picture of powerful, aggressive interest groups, the Bauer, Pool, and Dexter study, performed in the period from 1954 to 1962, portrays interest groups as having limited resources, and as paying limited attention to the tariff issue. The greater prosperity of the latter period may well account for the difference in reported behavior.

Notes

An earlier version of this paper was presented at the 1984 annual meetings of the American Political Science Association and the Public Choice Society. We have received helpful comments from John Conybeare, Joanne Gowa, Miles Kahler, Robert Keohane, Howard Marvel, Jonathan Pincus, Susan Strange, and Robert Walters.

1. This argument neglects the possibility that under some circumstances—in particular those addressed by arguments about “optimal” tariffs—protection might actually raise national income.

2. This is not the only asymmetry that could do so, but it is one asymmetry whose presence seems endemic to private enterprise market systems. Other asymmetries—for example, conflicting interests among owners of capital due to different owners holding portfolios concentrated in different firms—can in principle be eliminated by the dominance of large institutional investors that hold highly diversified portfolios.

3. The postulate of elected representatives from geographically defined districts is adopted for its simplicity and familiarity to our readers. However, there is no logical necessity that the phenomena we discuss occur only in such systems. The minimal institutional conditions that seem necessary in order for the process we describe to occur are as follows: First, property rights must be generally private. Second, the government must not be a perfect dictatorship, lacking any motivation to respond to the political demands of property owners. Such minimal conditions can be met without any form of mass suffrage or a formal legislature. The point is of considerable empirical significance, since extensive

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private property rights often occur in systems lacking mass suffrage or significant electoral competition.

4. Owners whose assets were concentrated in capital of differing vintages—and hence different production costs—could have different political interests in achieving protection analogous to the distinction developed here between interests in old and new regions. We have not constructed a model based on the lack of diversity in capitalists' portfolios, because the current predominance of institutional investors with highly diversified portfolios suggests this is an unlikely assumption for a theory purporting to explain something about current tariff politics. However, for earlier eras, when capital markets were less well developed, an approach to the problem that takes concentrated portfolios as the rule rather than the exception appears more reasonable.

5. Generally, the costs incurred in changing jobs are greatly outweighed by the decrease in earnings arising from the difficulty in fully employing accumulated human capital in a different job (Stern, 1972). Because of this, labor can suffer significant decreases in earnings, even when discharged during periods of tight labor markets, when the costs of finding a new job are not large.

6. There is one important exception to this generalization: if there were no new import-competing industry regions—that is, if there were no significant regional cost differences in a given industry, and the industry's capital stock had been running down—then an increase in tariffs could benefit the old industry regions. This is because any resultant expansion in industry size would have to occur in what had been an old region. Under this circumstance, old industry regions would have some incentive to seek protection, even when existing capacity was fully utilized.

7. A product cycle theory of international trade (Vernon, 1966) provides a basis for believing that industries dominated by old regions will be import-competing, while industries dominated by new regions will be exporting.

8. Normal goods, with income elasticities of demand exceeding unity.

9. Pincus (1975) notes in his empirical study of the U.S. tariff of 1824 that industries with low proprietary income shares did well in obtaining protection. Presumably, this is because firm purchases of labor power and input commodities accounted for a very large proportion of firm disbursements. Such a pattern of disbursements would generate greater regional benefits than those of a firm with an equivalent level of capitalization and rate of profit, but a smaller payroll or more modest needs for input commodities. Pincus also associates the complements of these last-mentioned characteristics with the attainment of protection.

10. An exception, of course, is the worker who

possesses industry-specific human capital and who cannot readily migrate.

11. This follows simply from the observation that profits are credited to investments rather than to sales. A given percentage increase in revenue will increase the rate of profit of a small investment much more dramatically than that of a large investment.

12. Schattschneider also notes several reasons for firms within the same organization taking different stands on tariffs: internal transport costs, product differentiation, and interest groups whose organizing principle is some issue or concern other than tariffs.

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ON THE POSSIBILITY OF FAITHFULLY REPRESENTATIVE COMMITTEES

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By faithful representation we mean the delegation of decision making to a relatively small committee that, using a weighted voting rule, will for each pair of alternatives make sincere choices identical to those that would be made by the society as a whole, and with the same vote margins. We show that for any society, no matter how large, faithful representation is possible by a committee with no more than $m(m-1)/2$ members, where m is the number of alternatives. We also show that for any society, no matter how nonideological the bulk of its electorate, social preferences can be faithfully represented by a committee whose members all have single-peaked or single-troughed preferences. Thus, all societies can be faithfully represented by a committee whose members see the world in unidimensional terms—that is, representatives can share a coherent ideological perspective even though the electorates they represent lack such a perspective. We further show that the usual mechanisms of proportional representation and the modified form of proportional representation recently proposed by Chamberlin and Courant (1983) do not guarantee faithful representation, and we discuss mechanisms that may provide faithful representation, even in a context in which new alternatives can arise.

Faithful representation has long been advocated as the ideal form of representation. As Chamberlin and Courant (1983, p. 718) characterize this view, "Representative democracy is at best a working model of direct democracy and is most successful when it generates . . . decisions as close as possible to those that would be generated in a direct democracy."¹ We shall show

that, contrary to what common sense might expect, even societies having millions of voters (and whose voters have millions of distinct preference orderings over the various alternatives) can often be *faithfully represented* by a relatively small group of representatives: i.e., the representatives' weighted sincere votes will perfectly mirror the directions of all pairwise preferences between alternatives, and the

margins by which the voters prefer one alternative over another. Moreover, we shall show that the means of implementing such faithful representation is not any of the usual forms of proportional representation.

We believe that the idea of faithful representation is of theoretical and practical importance. Direct democracy is rare; usually voters must choose a committee (e.g., a legislature) to decide on their behalf (Black, 1958). The size of electoral margins may influence policies over and above the simple directionality of vote outcomes. Vote margins may affect future policy proposals as well as perceptions of the likelihood of reversal of present decisions.

There are many instances of the importance of vote margins. For example, New York State school budgets are voted on by residents of school districts, but the subsequently proposed budgets are determined by the school boards. Feld and Grossman (1984) show that the size of the budget referendum majorities affects the subsequently proposed budgets over and above the decision to either pass or defeat a particular proposed budget. Journalists, political scientists, and politicians themselves commonly use an elected official's margin of victory as an indication of the size of his or her political mandate.² Observers and participants in legislative decisions use legislative margins as an indication of the stability or changeability of a particular policy direction (Grofman and Uhlaner, 1985). A strong notion of representation requires that a committee not only make the same decision as would the entire set of voters (we shall refer to this as *order representation*), but also that it would make decisions by the same margins, so as to indicate the strength as well as the direction of preference of the voters. We shall refer to the latter form as *faithful representation*.³

It might appear that faithful representation is impossible. Certainly the social

choice literature is filled with impossibility results, and faithful representation seems to be such a strong requirement that an impossibility result would seem almost inevitable. As we shall see, however, although the usual forms of representation (e.g., plurality or list proportional representation) cannot guarantee faithful representation, it is possible to devise a weighted voting scheme that will in general provide faithful representation. A *weighted voting scheme* is one in which each committee member has multiple votes to cast. Such schemes are used in proxy-voting for corporate boards of directors, in voting for labor union officers in labor federations such as the AFL-CIO, in most county governments in the State of New York, in the European Economic Community, and in various other organizations (Brams, 1975; Grofman and Scarrow, 1981, 1982; Straffin, 1980). Moreover, the committee size needed to implement a weighted voting scheme to carry out faithful representation is remarkably small; and the committee can be constructed so that all of its members will have single-peaked or single-troughed preferences with respect to any given linear ordering, even though in the society as a whole only a handful of voters have orderings that are single-peaked or single-troughed with respect to that range of alternatives.⁴ Thus, we have the quite counterintuitive result that we can have faithful representation from a legislature whose members have well-defined ideologies underlying their preferences (i.e., they have single-peaked or single-troughed preferences with respect to a given linear ordering) even though the society they faithfully represent is not at all ideological in character.⁵ Furthermore, a committee that is selected to be faithfully representative for a relatively small set of alternatives can, under reasonable circumstances, continue to be faithfully representative of the electorate over a much wider set of alternatives that may subsequently arise.

Proportional Representation is Not Faithful Representation

Proportional representation in multi-member districts is generally considered an improvement over single-member district-based plurality (or majority run-off) representation, because the legislature or committee chosen is expected to mirror more closely the composition (and thus the preferences) of the electorate. However, list proportional representation (list PR), the most common form of PR, is generally inadequate for faithfully representing pairwise vote margins for more than two alternatives, especially when multiple alternatives cannot be meaningfully ordered along a single continuum.

We shall treat a political party as equivalent to a given preference ordering over alternatives (i.e., each party representative will be expected to vote in accord with the same linear ordering). List PR is inadequate to provide faithful representation, because there are generally too few parties to represent the variety of preference orderings in the electorate, and voters are forced to choose the closest approximation to their own ordering (e.g., relying only on first preferences); the process of approximation can severely distort the outcomes. For example, consider a situation with three alternatives—*A*, *B*, and *C*—and three parties—*X*, *Y*, and *Z*—each associated with the modal preference orderings as follows: *X:ABC*, *Y:BCA*, *Z:CAB*.

Now consider that the population has the full range of preference orderings in the proportions shown in the example below:

Voter Preference Orderings	Votes	Most Likely Party Choice
ABC	23	X
BCA	20	Y
CAB	20	Z
CBA	15	Z
BAC	17	Y
ACB	5	X

In this example we also have shown the party that most closely approximates the preferences of voters with each preference ordering. If parties are given votes in proportion to their choice by voters, then *X*, *Y*, and *Z* have 28, 37, and 35 votes, respectively. No party has a majority on its own; their votes on issues result in the collective choices of *A* over *B* by 26, *B* over *C* by 30, and *C* over *A* by 44. This is in contrast to very different preferences of the voters, *B* over *A* by 4, *B* over *C* by 20, and *C* over *A* by 10. One of three decisions is made in the wrong direction; all of the voter margins are incorrect, and the majority preference ordering of the voters is *BCA*, while that of their chosen representatives is a cycle.

This simple example shows the inadequacy of list proportional representation. With additional political parties the system might work better, but there is no guarantee that the most representative set of parties will emerge; it is probably realistic to expect that there will be a few parties representing the modal preferences, just as in our example. Even if other parties are included, the choices and vote margins might still be distorted as long as some voters are forced to choose a party that does not perfectly represent them. These findings seem to suggest that proper representation requires as many different representatives as there are preference orderings in the population. Indeed, at least one knowledgeable political scientist suggested to us that the search for faithfully representative committees was apt to be fruitless for exactly this reason: He conjectured that if the society was large, only a large committee (presumably one representing most of the preference orderings in the population) would be able to represent it faithfully. For *m* alternatives, there are *m!* different possible preference orderings; for 12 alternatives, everyone in the United States could have a different preference ordering. Even if people had only ideologically consistent orderings (i.e., single-peaked

preferences over the continuum defined by a particular linear ordering), there would still be 2,048 different preference orderings. If any such large number of preference orderings had to be represented directly in a committee, a system of faithful representation would be unworkable. Fortunately, we can show that faithful representation can always be achieved with a relatively small number of representatives (e.g., a maximum of 66 in the case of 12 alternatives), no matter how many voters there are.

Basic Theorems on Faithful Representation

McGarvey's (1953) interest in demonstrating the possibilities of vote paradoxes led him to show that any possible set of pairwise preferences over m alternatives (and thus in particular any cycle) can be reproduced with at most $m(m - 1)$ different preference orderings. Since the task of constructing vote paradoxes is of little more than technical interest, this result is almost unknown even in the social choice literature. However, it is an immediate corollary of McGarvey's theorem:

COROLLARY TO MCGARVEY'S THEOREM.

For any society whose members have preference orderings over m alternatives, there exists a set of at most $m(m - 1)$ representatives whose majority choices between every pair of alternatives perfectly mimic the directionality of the pairwise majority choices of the society as a whole.

We shall call a committee that mirrors the pairwise majority choices of a larger group an *order-representative* committee. McGarvey's result tells us that we can obtain an order-representative committee with at most $m(m - 1)$ representatives. Order representation is often relatively simple to satisfy. For example, if a society has a transitive ordering over alterna-

tives, then a single individual with that set of preferences is order representative for that society. As McGarvey shows, even if the group majority preferences are not transitive, then one can generate a pair of preference orderings that produces the majority for any alternative over any other without affecting the results of any other binary choices. This can be seen by considering five alternatives, A through E , and the decision between A and B . If someone has ordering $ABCDE$ and someone else has ordering $EDCAB$, then together they prefer A to B and cancel out each other's preferences (they are effectively indifferent) over all other binary choices.

This analysis contains the essential logic needed to extend McGarvey's results from order representation to faithful representation. McGarvey's argument can be easily extended to show that faithful representation (of any margins) is possible by weighting each member of the pair of preference orderings used to produce a binary choice ($ABCDE$ and $EDCAB$ in the example above) by half the margin; then, together, they produce the particular margin while all other representatives cancel out one another on this binary choice. This extension of McGarvey's Theorem indicates that a maximum of $m(m - 1)$ different preference orderings are sufficient to represent any group faithfully. We can, however, prove the stronger result that this same representation can always be achieved with half that many, or $m(m - 1)/2$, different preference orderings.

THEOREM 1. *A maximum of $m(m - 1)/2$ different (weighted) preference orderings (i.e., a set of $m(m - 1)/2$ committee members voting sincerely, each in accord with a distinct preference ordering) is sufficient to represent the margins of pairwise preferences over m alternatives that emerge from any*

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group—that is, to faithfully represent the group.

Proof. Two different forms of proof provide insight into this theorem, and so are worth some discussion.

First, reconsider our example. Let us try to faithfully represent this situation with preference orderings ABC , BCA , and CAB . It is easily seen that the margins of preference of A over B are the result of adding up the frequencies of all individual orderings preferring A over B , and subtracting the frequencies of all individual orderings preferring B over A , as shown below:

$$\text{For } A > B: f(ABC) - f(BCA) + f(CAB) = -4$$

$$\text{For } B > C: f(ABC) + f(BCA) - f(CAB) = +20$$

$$\text{For } A > C: f(ABC) - f(BCA) - f(CAB) = -10$$

Rows two and three are similarly based upon the computation of the margins of B over C and A over C , respectively. We have three independent equations and three unknowns that can be solved for the frequencies of each of these preference orderings; all three are necessary to reproduce these particular margins of preference. In this case the solution is

$$f(ABC) = 8,$$

$$f(BCA) = 23,$$

$$\text{and } f(CAB) = 11.$$

This example shows how faithful representation can be produced, and also (as per our previous discussion) that proportional representational procedures can distort decisions, even though faithfully representative weights do exist. As noted previously, in this example, weights derived from a list form of proportional representation give rise to cyclical majorities, even though the electorate as a whole has a transitive majority ordering.

In general, there is one equation for each of the $m(m-1)/2$ binary choice margins, and so the equation set can be solved—with exactly as many equations as unknowns—so long as the equations

are independent. Because it would be undesirable to give a representative a negative weight, if the solutions of the equations turn out to be negative, a representative assigned a negative weight can be replaced with one having exactly the opposite preference ordering who is given a positive weight. (By the *opposite preference ordering* we mean one which reverses each pairwise preference: e.g., the opposite of ABC is CBA .) Because of the independence requirement, not every set of $m(m-1)/2$ preference orderings can be used to faithfully represent a group. On the other hand, the set of preference orderings that can be used to obtain faithful representation is not, in general, unique.

The second form of proof is by construction, using McGarvey's logic to construct a set of preference orderings (and frequencies/weights for each) that reconstruct a given set of margins. Let P_{ij} indicate orderings in which i is to the left of j , which we also denote as $i < j$. There are $m(m-1)/2$ different preference orderings of the form

$$\begin{aligned} P_{ij} = & [i+1, i+2, \dots, j], [i], \\ & [j+1, j+2, \dots, m], \\ & [i-1, i-2, \dots, 1]. \end{aligned} \quad (1)$$

This apparently complex formula is actually a specification of a simple idea that can be illustrated for four alternatives: That is, $P_{1,2} = 2\ 1\ 3\ 4$; $P_{1,3} = 2\ 3\ 1\ 4$; $P_{1,4} = 2\ 3\ 4\ 1$; $P_{2,3} = 3\ 2\ 4\ 1$; $P_{2,4} = 3\ 4\ 2\ 1$; and $P_{3,4} = 4\ 3\ 2\ 1$.⁶

Each ordering and the negative of the next constitute a McGarvey pair; e.g., $2\ 1\ 3\ 4$ and $4\ 1\ 3\ 2$ (the reverse of $2\ 3\ 1\ 4$) cancel each other out, except for their both contributing to the margin of 1 over 3. Thus, McGarvey pairs for the $m(m-1)/2$ margins can be constructed from these $m(m-1)/2$ preference orderings by using each one twice. This proof by construction provides an example of a

set of representatives that can faithfully represent any set of margins.

Just as in the previous proof, the frequencies of each of these preference orderings necessary to reproduce the margins can be negative. Consequently, as above, any preference ordering that requires a negative weight would be replaced with its opposite with a positive weight. This is theoretically important, as we shall show below after we provide an extension to Theorem 1.

Theorem 1 says that one can faithfully represent the margins with $m(m-1)/2$ weighted preference orderings. This procedure does not faithfully reproduce a 25-vote margin of *A* over *B*, however, this might have been 25 out of 2 million in the electorate, and be 25 out of 100 in the committee of representatives. Fortunately, only one additional preference ordering is required to reproduce the proportions as well as the margins themselves.

COROLLARY TO THEOREM 1. *A maximum of $m(m-1)/2 + 1$ different weighted preference orderings is sufficient to represent the proportionate margins of pairwise preferences that emerge from any group—that is, to represent faithfully the group margins and their proportions of the total votes.*

Proof. The first proof of Theorem 1 can be extended to prove this corollary. We merely include one additional preference ordering and add one more equation specifying that the frequencies of all of these preference orderings add up to the total number of votes in the electorate.⁷

Our second proof of Theorem 1, the proof by construction, has further implications. It should be noted that every preference ordering given in the proof above is single peaked with respect to the ordering from 1 to *m* (1 to 4 in the example). The opposite preference orderings are all single troughed with respect to the same ordering. (The opposite of a single-

peaked ordering is always a single-troughed ordering with respect to the same continuum.) Consequently, this second proof constitutes a proof for an even stronger theorem:

THEOREM 2. *With respect to any linear ordering, a maximum of $m(m-1)/2$ different (weighted) single-peaked and/or single-troughed preference orderings are sufficient to represent the group decisions faithfully over *m* alternatives.*

The Preferences of Members of Faithfully Representative Committees and Those They Represent

Since there are various faithfully representative committees, it would be useful to know whether the preference orderings of the members of such committees are necessarily similar to those of the represented population. The answer is there need be no overlap at all—a set of representatives can faithfully represent a group and yet have no overlap with the set of preference orderings of the group it represents.

Consider a society with a single individual with the preference ordering 3 6 5 1 2 4. The committee shown below faithfully represents that society, but has no overlap with it.

Ordering	Weight
6 5 4 3 1 2	1
2 3 1 4 5 6	1
6 5 1 4 3 2	1
3 2 4 5 6 1	1
1 6 5 2 4 3	1
3 4 5 6 2 1	1
1 2 3 6 5 4	1

However, if there are only three alternatives, no committee can faithfully represent a society of a single person unless it includes that person's preference ordering.

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The proof by construction for Theorem 2 showed how one can construct a set of preference orderings that can faithfully represent any group. If we have enough orderings to choose from, we can simply choose our committee so as to exclude from its members any with preference orderings in the society being represented.

COROLLARY TO THEOREM 2. *If a society has no preference orderings which are either single peaked or single troughed with respect to some given ordering, then that society can be faithfully represented by a committee whose preference orderings do not overlap those in the society.*

Proof. Use the second construction method used in the proof of Theorem 1, with the single-peaked ordering based on the single-troughed preferences not found in the society. Q.E.D.

This corollary can also be regarded as a special case of the more general theorem given below.

THEOREM 3. *If a society lacks at least $m(m-1)/2$ linearly independent preference orderings (i.e., orderings that generate linearly independent equations) from among the orderings held by any of its members, then it can be faithfully represented by a committee whose preference orderings do not overlap those in the society.*

Proof. Use the equational method used in the first proof of Theorem 1. (It may be necessary to give some members negative weights.) Q.E.D.

On pragmatic grounds, one would prefer a faithfully representative committee whose members did share preference orderings with the underlying electorate to one that did not. Fortunately, it is always possible to have a faithfully representative committee composed largely or

entirely of preference orderings in the represented electorate.

THEOREM 4. *There is always a subset of no more than $m(m-1)/2$ of the set of preference orderings in a group that can be used to faithfully represent the group as a whole.*

Proof. If the society has less than $m(m-1)/2$ members, we are done. If not, set up $m(m-1)/2$ equations as in the first method used to prove Theorem 1, but using the complete set of n preference orderings in the group. Clearly, this set of equations has a solution, namely, that in which each ordering is given the weight equal to the number of group members holding the ordering. Of course, we have too many variables in our equation set. However, a well-known result in linear algebra tells us that since these equations do have a solution, there will exist at least one subset of no more than $m(m-1)/2$ of these variables that will be sufficient to determine the $m(m-1)/2$ margins,⁸ since the rank of the $m(m-1)/2$ by n matrix specified by this equation set cannot exceed $m(m-1)/2$. Q.E.D.

It might appear that the faithfully representative subset of a group should, as much as possible, be representative of the full range of preferences of the group, but this may be unrealistic, because almost all orderings are held by some members of the society. Also, there is some reason to want a set of representatives whose preferences are more "coherent" than the probably widely divergent preference orderings held by the public. In particular, it would be nice to find a committee all of whose preferences are single peaked over some particular unidimensional continuum, since knowledge of this fact might simplify decision making in the group. We now turn to consider the relationship of single-peakedness to faithful representation.

Single-peakedness, Net Margins, and Faithful Representation

Knowledge of the paradox of voting, combined with the recognition of its empirical rarity, has frequently led researchers to suppose that there is coherence in individual preference orderings. Specifically, it has often been noted that if there is an underlying ordering of alternatives such that all individuals have single-peaked preference orderings with respect to the unidimensional continuum defined by that ordering, then the group as a whole has transitive majority preferences. Unfortunately for political theorists, few groups have individual preferences that are all single peaked. Majority decisions, however, may be transitive even though there are many individual preferences that are not single peaked (Feld and Grofman, n.d.; Niemi, 1969). We believe that the criterion of single-peakedness is too restrictive to be applicable in most actual situations, but that its weaker counterpart at the aggregate level, which we call the *Net Margin Condition*, is much more commonly found.

DEFINITION. *The Net Margin Condition is said to apply to a group's preferences over a set of alternatives, if and only if there is some linear ordering of alternatives such that for every i, j , and k arrayed $i < j < k$ with respect to that ordering, $m(i, j) \leq m(i, k) \leq m(j, k)$, where $m(i, j)$ is the margin by which alternative i is preferred to alternative j (negative if alternative j is majority preferred to alternative i).*

The definition may be made more meaningful with the diagram shown below. The alternatives are listed according to the linear ordering across the top and down the side. The entries in the matrix are just the margins $m(i, j)$. Those below the main diagonal are just the negative of the corresponding one above—e.g., $m(1, 2) = -m(2, 1)$; only those

above the main diagonal are shown. The Net Margin Condition requires that the entries increase (or at least do not decrease) as one moves to the right or down within this half of the matrix of margins.

Matrix of Margins

	1	2	3	4	...
1		$m(1, 2)$	$m(1, 3)$	$m(1, 4)$...
2			$m(2, 3)$	$m(2, 4)$...
3				$m(3, 4)$...
.					
.					

Single-peakedness at the individual level implies the Net Margin Condition at the aggregate level. This can be seen by the implication of single-peakedness that everyone who prefers 1 to 2 must also prefer 1 to 3, so the number of people voting for 1 over 3 must be at least as great as the number voting for 1 over 2.

It is easily seen that the Net Margin Condition can hold while only a single individual has preferences that are single peaked with respect to the linear continuum along which margins are to be ordered; consider that all but one member of a group have preferences that are not single peaked, half of them having preferences opposite those of the other half, thus cancelling one another out on all binary decisions. (If m is large enough, we can easily satisfy this condition.) The one other member determines all of the margins; if that one has single-peaked preferences (i.e., if he has a transitive ordering of his pairwise preferences), then the margins are defined by his single-peaked preferences and therefore must satisfy the Net Margin Condition.

Moreover, the Net Margin Condition does not require any of the individuals to have single-peaked preference with respect to the linear continuum along which margins are to be ordered. For example, the Net Margin Condition for the seven-member committee discussed

above obtains for the ordering 3 6 5 1 2 4, even though none of the members of the committee have single-peaked preferences with respect to that ordering.

The Net Margin Condition permits a special form of faithful representation as expressed in Theorem 5:

THEOREM 5. *A group can be faithfully represented by a committee composed entirely of individuals with single-peaked preferences, if and only if group preferences satisfy the Net Margin Condition. In particular, for groups satisfying the Net Margin Condition, a maximum of $m(m-1)/2 + 1$ different weighted single-peaked preference orderings is sufficient to faithfully represent the group margins and the proportions of the total votes cast on each pairwise choice.*

Proof. We have already proven that single-peaked preferences for all individuals in the society imply the Net Margin Condition for the group. In this situation, single-peaked preferences of the faithfully representative committee imply that the Net Margin Condition applies to the committee margins. Since the committee faithfully represents the group, the committee margins are those of the group, so the group must obey the Net Margin Condition. The other direction is proven by construction, as shown in the Appendix. We show that we can construct a set of single-peaked preference orderings that can faithfully represent any group whose margins satisfy the Net Margin Condition.

The Net Margin Condition is sufficient but not necessary for there to be a transitive majority ordering.⁹ Thus, a group may have a transitive majority preference ordering but not be faithfully representable exclusively with single-peaked preference orderings, because the group fails to satisfy the Net Margin Condition (cf. Feld and Grofman [1986]).

Theorem 5 is important in that it indicates that a group having few single-peaked preferences with respect to any ordering can, if the Net Margin Condition is met, be faithfully represented by a committee whose preferences are all single peaked with respect to some given ordering. We believe that elected representatives are much more likely to hold "ideologically coherent" positions (on, say, a left-right dimension), and thus to have single-peaked preferences, than are their constituents. Empirical studies show that American voters hold a wide variety of preference orderings, including many that are not single peaked. We suggest that the large differences between voters and their representatives in the diversity and degree of ideological consistency of each group's preference orderings provide no obstacles to faithful representation.

The January 1980 National Election Study conducted by the University of Michigan Center for Political Studies provides an empirical example of a situation where the aggregate margins of preference satisfy the Net Margin Condition, even though there were voters who had every possible preference ordering over a given set of candidates. In the next section, we show one way that this electorate could have been faithfully represented by a committee composed entirely of representatives having single-peaked preference orderings.

An Empirical Example

In 1980, the University of Michigan Center for Political Studies undertook a panel study of voters preceding and immediately following the 1980 presidential election. On the first wave of that study, in January 1980, individual respondents were asked to indicate their feelings towards each of the candidates by indicating a number from 0 to 100 (where 100 is the most positive feeling). These data allowed us to determine a preference

Table 1. Proportions Favoring the First Candidate of Each Ordered Pair

Candidates	Candidates			
	Kennedy	Carter	Ford	Reagan
Kennedy	—	36.1	36.5	40.5
Carter		—	48.4	53.7
Ford			—	54.6
Reagan				—

Note: Proportions below the main diagonal are omitted: values would be 100 minus the proportions in the corresponding cell above.

ordering over the candidates at that time. The survey included a long list of candidates; for the present purposes, we confine our attention to the four best known candidates: Carter, Kennedy, Ford, and Reagan. Most voters indicated that they recognized the names of all these candidates.

Based upon our own knowledge of this situation, we determined that if there was an ideological (left-right) continuum, it would have to be Kennedy, Carter, Ford, and Reagan, from left to right respectively. There are a total of 24 different strong preference orderings over four alternatives. All individuals who indicated ties or did not give an answer for one candidate were excluded to simplify the analyses and interpretations. As we show elsewhere (Feld and Grofman, 1985), every one of the 24 possible preference orderings was represented in the sample of voters, and almost half (47%) of the preference orderings were not consistent with single-peakedness on the given hypothesized continuum.

Despite the large proportion of preference orderings that were not single-peaked, the margins of pairwise preferences conformed perfectly to the partial ordering expected to arise from single-peaked preferences, thus satisfying the Net Margin Condition, as shown in Table 1.

According to Theorem 5, if the Net Margin Condition is satisfied, a group can be faithfully represented by

$[m(m - 1)/2] + 1$ single-peaked preference orderings. In this case, the margins in Table 1 can be reproduced by the following frequencies of seven single-peaked preference orderings, as can be determined by following the algorithm described in the Appendix:

36.1% of KCFR
 .4% of CKFR
 4.0% of CFKR
 7.9% of CFRK
 5.3% of FCRK
 .9% of FRCK
 45.4% of RFCK

This provides an empirical example of an electorate not limited to single-peaked preferences that can nevertheless be faithfully represented by a committee whose members all have single-peaked preference orderings.

Procedures to Provide Faithful Representation

As Chamberlin and Courant (1983, p. 718) point out, although there is a sizeable comparative politics literature on the PR vs. plurality debate, "social change theorists have paid almost no attention to methods of selecting representative bodies." This article, like theirs, seeks to remedy that surprising omission. Since it is always possible to faithfully represent the preferences of voters, the question is how this might practically be done. Un-

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fortunately, as we showed with respect to the list form of proportional representation, current electoral systems do not necessarily provide faithfully representative legislatures; they either fail to choose a correct set of legislators or fail to weight committee members properly to produce faithful representation of the population. The same problems apply to the single transferable vote method of PR (STV) (Sugden, 1984) and to the form of PR proposed by Chamberlin and Courant (1983), although Sugden (1984) shows that if all voters have single-peaked preferences, STV will be order representative. The Chamberlin-Courant method may pick a committee that is not even order representative. Consider a seven-member society with preferences among three alternatives, as shown below:

<i>Number of Voters</i>		
3	1	3
A	B	C
B	A	B
C	C	A

The Chamberlin-Courant rule for a committee of size 2 would pick a committee with four votes for A and three weighted votes for C. This makes A dictator. However, as is obvious (because of symmetry), the preference ordering for society is BAC.¹⁰

The same information used to generate an outcome under STV¹¹ or under the Chamberlin and Courant procedure—namely a complete rank ordering of alternatives—may be used to generate a faithfully representative committee by treating each potential representative as “representing” a given preference order and assigning weights in such a way as to faithfully represent the voters. Also, because faithful representation is just that, if voters are asked to submit a rank ordering of alternatives to be used for purposes of generating a faithfully representative committee, they have no strong reason to lie about their true preferences, even if

they were thinking about sophisticated voting in the sense of Farquharson (1969), since the sequence in which alternatives will eventually be paired is unknown.

If there are $m(m-1)/2$ independent representative orderings, then there will always be a faithfully representative committee. If there are more than $m(m-1)/2$ independent orderings in the electorate, then there will always be multiple ways to faithfully represent the voters; if there are multiple faithfully representative committees, then we suggest that the one whose frequencies of preference orderings are most highly correlated with those in the population should be preferred. In any case, we can produce a committee that is at least as representative as that which will be produced by any other voting procedures.

It should be noted that faithfully representative committees can have very reasonable size, especially in comparison to the size of populations and the number of distinct orderings possible. We have indicated that faithful representation does not depend upon the size of the population; for m alternatives, faithful representation can be assured with a maximum of $m(m-1)/2$ weighted representatives, and frequently with fewer than that. Table 1 shows how the size of the faithfully representative committees rises with the number of alternatives. Sometimes, however, we will be able to obtain faithful representation with less than $m(m-1)/2$ committee members, as is shown in Table 2.

Table 2 supports our earlier claim that while every individual in a population the size of the United States could have a unique preference ordering over twelve alternatives, the population could be faithfully represented with a maximum of 66 representatives.

One inevitable problem in any form of representation is the possibility that the representatives will have to choose among alternatives that were not anticipated

Table 2. Maximal Size of a Faithfully Representative Committee

Preference Orderings and Committee Size	Number of Alternatives (m)			
	3	6	9	12
Number of preference orderings ($m!$)	6	720	362,880	479,001,600
Number of single-peaked preference orderings (2^{m-1})	4	32	256	2,048
Maximal size of faithfully representative committee ($m(m-1)/2$)	3	15	36	66

when they were selected. This is, of course, a problem for any representative system, no matter how its representatives are chosen. Certainly, if the new alternatives have no consistent relationship to earlier ones, there can be no assurance that a committee chosen for its faithful representation over some given set of alternatives will faithfully represent the electorate in considering those new alternatives. However, most new issues are related to preexisting ones, and faithful representation may be preserved under some likely conditions.

One possibility is that the underlying choice continuum is preserved by the new alternatives such that each of the new alternatives fits along the continuum in a way that preserves the Net Margin Condition. If the committee members recognize the position of a new alternative along the continuum, and include it there among their own single-peaked preferences, the resulting vote margins cannot diverge from those in the electorate by more than the vote margins of the two preexisting bounding alternatives along the continuum. For example, if W fits in the continuum such that the group meets the Net Margin Condition for $ABCDWE$, then the margins for $m(A,W)$ will be between $m(A,D)$ and $m(A,E)$, and the margin $m(D,W)$ must be between $m(D,E)$ and $m(E,E) = 0$. Thus, if the representatives properly recognize the position of the new alternative along the continuum, they can continue to approximate faithful representation.

Another possibility is that preferences over the new alternatives will be related to preferences over the old alternatives in some (approximately) linear fashion. For example, if individuals who prefer A to B are more likely to prefer A to W than are individuals who prefer B to A , then, to the extent that the preferences over some new alternative are determined by preferences over preexisting alternatives, the margins by which that new alternative is preferred to each of the old ones will be linear combinations of the previous observed margins. If roughly the same relationships hold for all the committee members, then the faithfully representative committee will have the same margins as the electorate over the new alternatives, and continue to be faithfully representative.

If the set of alternatives used to select the faithfully representative committee reflects the range of issue dimensions about which future decisions will be made, then it is reasonable to expect that the committee may be able to represent the electorate faithfully on choices over new alternatives by processes such as those identified above. Furthermore, the problem of continued faithful representation when new issues arise is certainly no greater under a legislature/committee chosen for faithful representation than for any other system of representation. In particular, under procedures for faithful representation, voters have no more reason to lie about their true preferences than they would in a direct democracy.

Conclusion

This paper has laid out the basic characteristics of faithful representation and some considerations for systems designed to produce it.¹² Our intention has been to demonstrate the importance of faithful representation and its practicability in principle. Implementation would require further consideration of various potential problems, especially the determination of a particular set of alternatives upon which representation should be based. However, as noted previously, if faithful representation is guaranteed, then voters have no good reason to lie about their true preferences. Moreover, if we find a committee that will, for a given set of issues or issue dimensions, be faithfully representative of the voters in the sense we have identified, then we also have some reason to believe this committee will as a whole be in sufficient sympathy with the views of the electorate so as to be likely to arrive at decisions that are in substantial accord with societal (perhaps ideally informed) preferences, even on new items for which we could not have an a priori certainty of faithful representation.

We have shown that faithful representation can be achieved with committees of relatively small size, and that it is always possible to faithfully represent a group having any variety of preferences with a relatively small committee, all of whose members have single-peaked or single-troughed preferences with regard to any particular ordering we might specify. Furthermore, if the Net Margin Condition is satisfied, we can find a faithfully representative committee, all of whose members have single-peaked preferences. We suggest that the actual functioning of a democracy often approximates the latter situation.¹³ Representatives are much more likely than the underlying electorate to have single-peaked preferences; yet, they can fairly represent the choices of the majority of the electorate, even though

that electorate is not single peaked. Consequently, large electorates may have only slight tendencies towards single-peakedness, and yet be faithfully represented by their single-peaked representatives. Analysis of legislative voting finds most votes can be scaled along a single dimension (see, e.g., Poole and Daniels, 1985), while the candidate preferences of the electorates in 1980 showed that single-peaked individual preferences were relatively uncommon. We have shown how these apparent inconsistencies in the preferences of elites and masses can, in principle, nonetheless, be fully compatible with faithful representation.

Appendix: Proof of Theorem 5

THEOREM 5. *A group can be faithfully represented by a committee composed entirely of individuals with single-peaked preferences if and only if group preferences satisfy the Net Margin Condition. In particular for groups that satisfy the Net Margin Condition, a maximum of $m(m-1)/2 + 1$ different weighted single-peaked preference orderings is sufficient to faithfully represent the group margins and the proportions of the total votes cast on each pairwise choice. One ordering less than that is needed to reproduce the margins without concern for proportions.*

Proof. Proof of the "only if" part of Theorem 5 is given in the text. The proof of the "if" part of the theorem consists of a procedure by which one can find a committee composed of $m(m-1)/2 + 1$ single-peaked preference orderings that perfectly reproduce the margins and the total votes in the electorate. We can show that the margins alone can be faithfully reproduced with one less preference ordering.

Consider that the alternatives are ordered from 1 to m along the continuum.

Since the margins satisfy the Net Margin Condition, we know that if $N(i,j)$ is the number favoring j over k , and if $i < j < k$, then $N(i,j) \leq N(i,k) \leq N(j,k)$; i.e., within the half matrix above the diagonal, the numbers go up as one moves to the right and down the matrix. We shall pay no attention to entries on or below the diagonal.

We can proceed by accounting for each of the numbers in the matrix sequentially from the smallest to the largest. The smallest number is $N(1,2)$. We begin with exactly $N(1,2)$ of the preference ordering $1\ 2\ 3\ \dots\ m$. Then, we subtract $N(1,2)$ from all of the rest of the numbers in the matrix, to obtain a new matrix. The next smallest number in that new matrix will be in the $N(1,3)$ cell. We use exactly that many of the $2\ 1\ 3\ 4\ \dots\ m$ preference ordering. Then we subtract $N(1,3)$ from all of the remaining cells to obtain a new matrix.

As we proceed at each step subtracting numbers from the cell entries in the preceding matrix, we obtain a new matrix with a sequence of zeroes on the left side of some rows above the main diagonal and with a sequence of nonzero entries, which we shall denote with Ys, on the right side of most rows above the main diagonal. We show below an example of such a matrix for a twelve-alternative voting situation:

	1	2	3	4	5	6	7	8	9	10	11	12
1	X	0	0	0	0	0	0	0	0	0	Y	Y
2		X	0	0	0	0	0	0	Y	Y	Y	Y
3			X	0	0	0	0	0	Y	Y	Y	Y
4				X	0	Y	Y	Y	Y	Y	Y	Y
5					X	Y	Y	Y	Y	Y	Y	Y
6						X	Y	Y	Y	Y	Y	Y
7							X	Y	Y	Y	Y	Y
8								X	Y	Y	Y	Y
9									X	Y	Y	Y
10										X	Y	Y
11											X	Y
12												X

In general, to find out at each subsequent step which preference ordering is to be used and with what weight, we identify

at each step the *corner cell* or cells in the new matrix. A *corner cell* is one with zeroes above and to the left of it. In the matrix above the cells (4,6), (2,9), and (1,11) are corners. If the coordinates of the corners are denoted by (i_1, j_1) for the one with the lowest j value, (i_2, j_2) for the corner with the next lowest j value, etc., then one may specify the preference ordering that should be added to the set as follows:

$$\begin{array}{ll} j_1-1, j_1-2, \dots, i_1 & j_1, j_1+1, \dots, j_2-1, \\ i_1-1, i_1-2, \dots, i_2 & j_2, j_2+1, \dots, j_3-1 \\ i_2-1, i_2-2, \dots, i_3, \text{ etc.} & j_3, j_3+1, \dots, j_4-1, \text{ etc.} \end{array}$$

In the example of the matrix above, we have the sequence 5,4; 6,7,8; 3,2; 9,10; 1,11,12, as the preference ordering. It is clear that any such preference ordering is single peaked. Furthermore, it should be clear that this preference ordering contributes to the frequency of i_1 over j_1 and above, the frequency of i_2 over j_2 and above, the frequency of i_3 over j_3 and above, etc.; i.e., it contributes to exactly those nonzero cells that are yet to be accounted for and to no others.

One uses as many instances of this preference ordering as the lowest of the numbers in any of the corners; i.e., we give the preference ordering 54678329101112 the weight that is the lowest of the cell values in cells (4,6), (2,9), and (1,11).

The procedure continues until all of the matrix entries are accounted for. This takes exactly $m(m-1)/2$ preference orderings, because there are that many cells to account for.

Note that this is sufficient to reproduce all the numbers in the original matrix.

If one wanted also to reproduce the number of voters opposed, one would subtract the number of votes already accounted for from the size of the group and add in that many of the preference ordering $m, m-1, \dots, 1$. That method serves to reproduce the exact number in favor and opposed in each pairwise choice, and so reproduces the desired

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proportions. Thus, to account for proportions we require $m(m-1)/2 + 1$ weighted preferences. Q.E.D.

This procedure is not as complex as the abstract statement above might suggest.

A complete example with four alternatives can be used to illustrate the method: Consider the following matrix based on pairwise choices of 60 voters:

Alternatives	1	2	3	4
1	X	15	20	25
2		X	22	32
3			X	40
4				X

To create this matrix requires

frequency	preference ordering
15	1 2 3 4
5	2 1 3 4
2	2 3 1 4
3	3 2 1 4
7	3 2 4 1
8	3 4 2 1

and, since this accounts for only 40 votes, we also need 20 of the 4 3 2 1 preference ordering in order to total 60.

If one replaces the 15 orderings of 1 2 3 4 and the 20 orderings of 4 3 2 1 with 5 orderings of 4 3 2 1, then the margins are faithfully reproduced, but the proportions are not. If one uses all seven different preference orderings, then both the margins and the proportions are perfectly reproduced. In general, if one were only interested in reproducing the margins, one could include either 1, 2, . . . m or $m, m-1, . . . 1$, whichever was weighted more, because these orderings are opposite and thus cancel each other out (e.g., 20 orderings of 1, 2, . . . m and 15 orderings of $m, m-1, . . . 1$ produce the same margins as 5 orderings of 1, 2, . . . m alone).

Notes

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ences, University of California, Irvine, for manuscript typing; to Dorothy Gormick for bibliographic assistance; and to Kevin Lang and Peter Marsden for comments on an earlier draft. This research was supported in part by the National Science Foundation Decision and Management Sciences Program, grant SES 85-09997. It was completed while Bernard Grofman was a fellow at the Center for Advanced Study in the Behavioral Sciences, Stanford.

1. They then go on to distinguish this view from one suggesting that representatives shall not reflect societal preferences, but what those preferences would be if every member of the society was "ideally informed, ideally expert, and ideally clear about his own interests" (Rogowski, 1981, cited in Chamberlin and Courant, 1983, p. 719). However, in either view "it is direct democracy (either actual or ideal) that is used as a measuring rod" (Chamberlin and Courant, 1983, p. 719).

2. In a multistage process of candidate selection (e.g., winner-take-all primaries or the U.S. electoral college), the failure to represent vote margins at each stage can result in the choice of a candidate who would not be selected by the (party) electorate as a whole. Indeed, in sequential decision processes, faithful representation at each stage is necessary to assure an even order of representation overall.

3. We use the term *faithful representation* differently than it is used elsewhere in the social choice literature (e.g., Murakami, 1968).

4. For a given ordering of alternatives—say *bcdac*—single-peaked preferences with respect to that ordering are those where each voter's utility reaches a peak at some given alternative and then declines as one moves left or right along the ordering away from that most preferred alternative. For example, if a voter had his or her peak at *c*, for the preferences to be single peaked we must have *cPd*, *dPb*, *cPa*, and *aPe*, where *P* denotes "is preferred to." Thus, for voters with peak at *c*, single-peaked preferences could be *cdbae*, *cdabe*, *cdaeb*, *cadeb*, *caedb*, and *cadbe*. In like manner, single-troughed preferences for a given ordering of alternatives along a line are those where each voter has a least-preferred alternative, and utility increases for that voter as one moves right or left along the line from that alternative.

5. Faithful representation does not imply there is a transitive social ordering, but merely that committee members make the same pairwise choices (and by the same margin) as the society as a whole. If societal majority preferences are intransitive, then the same will also be true for the majority preferences of any faithfully representative committee.

6. Alternatively, if we begin with the 1 2 3 4 ordering, we can get the requisite orderings by first moving the 1 successively further down to 2 1 3 4, 2 3 1 4, and 2 3 4 1; then we move the 2 down to 3 2 4 1, 3 4 2 1; then the 3 is moved down to 4 3 2 1. The first and last of these processes are the

same (only reversed), so the first is omitted in the final list of six preference orderings:

2 1 3 4, 2 3 1 4, 2 3 4 1, 3 2 4 1, 3 4 2 1, 4 3 2 1.

7. If a set of weights that reproduces the margins has fewer voters than are in the general population, then one can increase the votes in the committee to the number in the electorate simply by adding equal numbers of one of the preference orderings and its opposite; this adds to the total number of votes cast without changing any of the margins. However, if the committee has more votes than the electorate as a whole, there may be no way to reproduce the proportions without giving some preference orderings negative weights—one cannot substitute a positive weight for an opposite preference ordering in this situation.

8. We may have to use negative weights if the opposite preference ordering is not found in our society and our equational technique assigns a negative weight. However, since there are multiple preference sets to choose from, we usually expect to be able to find a solution with only positive coefficients.

9. A proof of sufficiency is obtained by combining results in Feld and Grofman (1985, 1986). For a counterexample to necessity, consider this eight-voter, three-alternative example:

5 ABC
2 BCA
1 CAB

This society has a transitive majority preference ordering of ABC. There is, however, no linear ordering for which the Net Margin Condition is satisfied. There are three possible cases:

ABC	BAC	BCA
$m(A,B) = 4$	$m(B,A) = 4$	$m(B,C) = 6$
$m(A,C) = 2$	$m(B,C) = 6$	$m(B,A) = -4$
$m(B,C) = 6$	$m(A,C) = 2$	$m(C,A) = -2$

As we see, in none of these cases is the Net Margin Condition satisfied.

10. In their article, Chamberlin and Courant (1983, p. 718) identify two key criteria for representation: (1) representativeness of deliberations, and (2) representativeness of decisions. Chamberlin and Courant (1983) offer a method for selecting representatives (and the weights that each representative is to have in deliberations that are to take place under a weighted voting rule) that is especially attentive to the first feature, representative deliberations. We have provided a method that is based on the second feature, representative decisions. Thus, our work and that of Chamberlin and Courant (1983) can be seen as complementary. However, if there is a conflict between representative deliberations and representative outcomes, we believe that faithful representation is considerably more important than

symbolic representation of the sort that the Chamberlin-Courant method seeks to provide.

It is often possible to find committees that are arguably optional with respect to both criteria: representative deliberations and representative decisions. However, sometimes the two criteria will conflict—i.e., the intersection of the Chamberlin-Courant set and the set of faithfully representative committees will be empty. In the case of conflict we believe representative decisions ought to have priority.

We might also note that the Chamberlin and Courant (1983) definition of representative deliberations is not the only one that might be used. We could use as our litmus test the choice of a minimal k -cover for the set of voters (Harary and Cartwright, 1965). A k -cover is a committee such that, for every voter, some member of the committee is no worse than that voter's k th choice. Nelson Polsby (personal communication, October 10, 1985) has suggested that the members of the APSA Council be chosen in such a fashion that this group will constitute a 1-cover for the political science profession. He has also attempted to formulate selection criteria for the council so as to achieve this end.

11. In STV elections, voters are required to rank order candidates. In some jurisdictions using STV (Wright, 1986), a full ballot must be cast for a ballot to be valid (i.e., a complete preference ordering must be given by each voter). Even if less than a full ballot is cast, a voter's indifference among alternatives that are not rank ordered (and their location at the bottom of his preference ordering) is not unreasonable to assume.

12. It would also be useful to consider various types of approximation to faithful representation, but we leave that task to a subsequent paper.

13. Recall that the Net Margin Condition may be satisfied even though most of the orderings in the group are not single peaked.

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POLICY MOTIVATION AND PARTY DIFFERENCES IN A DYNAMIC SPATIAL MODEL OF PARTY COMPETITION

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We present a model of party competition that produces more realistic patterns of results than those often emphasized in the literature. Reversing Downs (1957), we assume that parties win elections in order to formulate policies, rather than formulate policies in order to win elections. Voters are modeled first as having perfect information about candidate positions, and then under conditions of uncertainty. In simulation experiments we show that policy motivation and voter uncertainty can bring about persistent and predictable party differences in sequential majority rule elections. As the degree of voter certainty decreases, parties diverge towards their optima, whereas increases in voter certainty draw parties towards cycles in which party positions vary, but predictable issue stances are maintained on the average.

Why is it that there seem to be persistent and predictable differences between political parties? Are there incentives for parties to offer meaningful choices without demanding that at least one party insure defeat by moving too far from an opponent's position? We address these questions in the context of a simple model of two-party competition in two-dimensional issue space. Unlike many such models, which assume that parties maximize votes, our model assumes that parties have policy preferences of their own, and that they compete for electoral victory by virtue of adjustments away from their own optimum, or most preferred posi-

tion. This policy motivation induces some persistent differences in party positions and platforms that might not exist if parties simply maximized votes, but the need to compete for swing voters limits the extent of those differences. In our models, party competition provides distinct choices for voters, and the nature of those choices is at least somewhat predictable.

Although persistent and predictable party differences are an element of conventional wisdom about politics, such a pattern is at odds with two of the central themes in the theoretical literature regarding party competition in issue space. One is that candidates of each party tend to converge on the position of the median

voter if the space is unidimensional (Enelow and Hinich, 1984, ch. 2). The other is that under most conditions, there is no such equilibrium in multidimensional issue space (Plott, 1967). Indeed, a person with complete control over the agenda can cause majority rule outcomes to cycle all over the space and even outside of the Pareto set (McKelvey, 1976). As Romer and Rosenthal (1984, p. 472) observe, "The pervasiveness of rather large policy swings poses a puzzle for mathematical models of majoritarian processes. We have seen that these models predict either policy convergence or total policy instability, neither of which occurs in actuality."

In the more realistic setting of two-party competition, Kramer (1977) and McKelvey and Ordeshook (1976) have shown that outcomes are likely to converge to a limited subset of multidimensional issue space. However, these results still do not account for one of the most important stylized facts about electoral politics in the United States and elsewhere, namely, enduring differences between parties (Beck, 1982; Hibbs, 1977; Page, 1978; Tufte, 1978).

Most dynamic spatial models assume parties maximize votes, and this assumption contributes to the resulting predictions of convergence of policy choices in one dimension and instability of policy choices in more than one dimension. Of course, parties have historical identities and internal clienteles that they must please at the same time that they seek votes, and their movement in issue space may be constrained as a result. These identities and clienteles give parties a motivation to achieve policy goals, as well as to win office or maximize votes. Here we conceptualize these multiple motivations as a problem of expected utility maximization.

The model is first developed under the assumption of perfect voter information about the location of parties in issue

space. It is then extended to conditions of voter uncertainty, and finally incorporates such nonspatial variables as the influence of incumbent performance and of other candidate characteristics, such as personality or perceived competence.

The Relevance of Existing Studies

A traditional assumption in spatial modeling has been that parties do not have a stake in policy beyond the bearing of policy choices on electoral prospects. In Downs' (1957, p. 28) words, "parties formulate policies in order to win elections, rather than win elections in order to formulate policies." If policies are chosen in order to maximize votes, parties might be willing to move anywhere in issue space, and a given cluster of voters might be appealed to by any party.

However, other work has recognized that party mobility in issue space is restricted, and that clienteles may have influence over positions taken by parties. For example, Aldrich (1983a, 1983b) extends the calculus of voting construct (Riker and Ordeshook, 1968) into a model of activist participation in parties. This explains how parties may be dominated by individuals with given policy preferences. Because party nominations are somewhat controlled by such activists, parties may have distinct policy identities.¹

Following Coleman (1971, 1972) and Aranson and Ordeshook (1972), Wittman (1973, 1977, 1983) argues that candidates are motivated by policy goals as well as by the desire to win, and that election may well be a means to control policy rather than vice versa. Therefore, candidates might be expected to maximize expected utility, a function of expected policy outcomes as well as probability of winning (Wittman, 1973, p. 495; 1983, pp. 142-43).

Hansson and Stuart (1984) show that

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politicians who maximize expected utility in this way over a one-dimensional issue space achieve a divergent equilibrium—that is, stable and predictable party differences.² Wittman (1983) derives such equilibria in higher dimensional spaces, using the assumption that parties maximize a “weighted mandate,” or plurality-weighted utilities, rather than probability-weighted (i.e., expected) utilities. This choice, plus some rather strong concavity assumptions, allows him to prove the existence of equilibrium in his model, and then to make use of comparative static analyses. In his model, the actual policy resulting from the political process is assumed to be “a weighted average of the political positions taken by the candidates (the weights being the percentage of votes for each candidate)” (Wittman, 1983, pp. 143, 151). In making these assumptions, Wittman abandons an explicit majority voting framework as the process by which electoral results are achieved. The equilibrium result in the formal Wittman model therefore comes at the expense of neglecting to specify the process by which political decisions are made.

We prefer to return to the more conventional assumption of expected utility maximization and to a characterization of the electoral process as a series of majority rule elections. Although Wittman (1983, p. 143) asserts that “substitution of plurality for probability in the maximization of expected policy implementation has no effect on the mathematical properties of the model,” we report some rather different results in this paper. Specifically, like many models of voting in multi-dimensional spaces, ours do not always generate equilibria. Moreover, the cycles that occur provide substantive implications about the nature of the electoral process.

Given problems of analytical tractability that result when the majority voting framework is retained, we have used numerical simulation techniques in our

investigation. While simulation experiments do require the input of specific parameter values and do not yield general theorems, they can illustrate a range of possible outcomes and how outcomes change under alternative conditions. In contrast to Wittman, we find that equilibria may or may not exist in our models (as one would expect in voting models), and we are able to describe some circumstances under which equilibria are likely. Whether or not equilibria are found, persistent party differences occur, on average over time, under conditions of voter uncertainty as well as under conditions of perfect information. Generalizations of some of Wittman’s intuitively plausible comparative static results also seem to hold up in our experiments, even when cycling occurs.

Our models provide a number of potentially important insights. They offer an explanation for persistent party differences in policies. This explanation is also consistent with occasional administration-specific aberrations, like those described in the empirical work of Beck (1982). In our model, these aberrations appear as parties try to lure away voters from the other party’s natural constituency. The models provide an explanation for apparent “stability” in party positions, in contrast to much of the theoretical literature on spatial voting. Incumbent victories and dominant parties are also consistent with the results of our model (in contrast to the results of the traditional Downsian model). Our results also suggest the applicability of an “approximate” median voter result in multiple dimensions, when voters’ optima lie only “approximately” along a single dimension. Finally, they suggest a relation between the sensitivity of voters to candidate policy differences and the extent to which parties will be able to impose extreme positions on the electorate, and the somewhat paradoxical result that cycling could be associated with more,

rather than less, voter control over outcomes.

A Model with Perfect Information

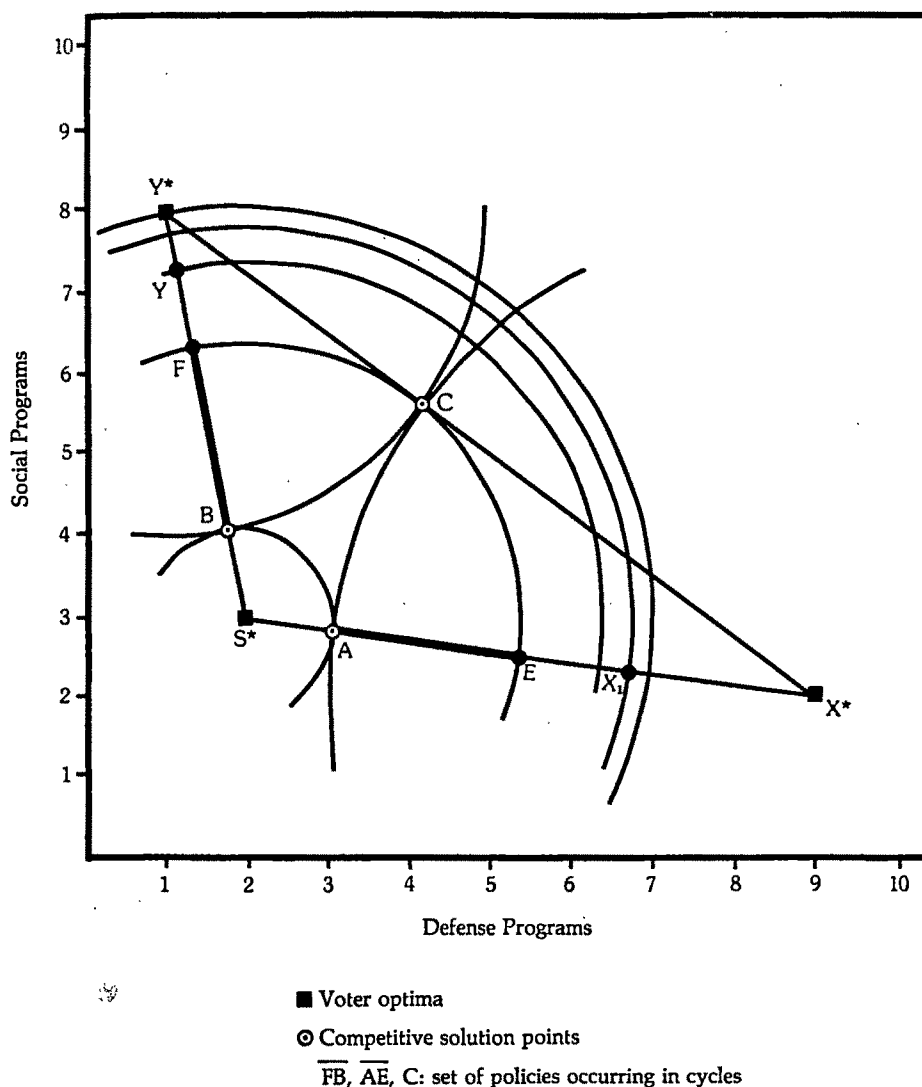
In this section we discuss a simple spatial voting model in which each of two parties has ideological aims. We assume that voters are perfectly informed of party positions and vote for the party that would yield them higher utility.³ We develop the argument first in terms of three voters in a two-dimensional policy space, where the policy issues are levels of spending for social programs and defense. Although our analysis is restricted to the three-voter case, this case is intended to be representative of the situation in which there are large numbers of voters divided into smaller numbers of distinct voting blocs. Voters are assumed to have Type I preferences—that is, each voter has a unique optimum point within the policy space that would yield maximum utility, and he or she ranks all other points in the policy space based on distance from the optimum. This implies that voters have circular indifference curves (there is an underlying budget constraint—voters will not prefer more of both goods when they must pay a share of the taxes required to finance additional purchases). When confronted with a choice between two parties offering differing platforms (i.e., positions in the policy space) voters will choose to vote for the party whose platform would yield the higher utility (or will abstain if the difference between parties is not larger than some very small finite distance).

In order to impose further structure, we make some assumptions about party behavior. First we assume that there exist two parties, *X* and *Y*, which compete to attain office in majority-rule elections. They do so by offering platforms to the voters. Following Downs (1957) and Kramer (1977), we assume that the incum-

bent party's choice of platform is constrained by its record. When they last won, the incumbents proposed a particular platform. In the meantime they have carried out that program and voters in this election attribute the same platform to them once again. Challengers are free to offer any opposing platform,⁴ but here we do not assume that parties choose a program to maximize votes. Instead we assume that each party has a leader with preferences of his or her own, and that in each election the challenging party leader chooses a platform that results in a post-election policy closest to his or her optimum. Except in unusual circumstances, this also results in victory for the challenger.

For simplicity in the presentation, we will assume that two of the three voters have optima coinciding with those of the leaders of the two parties, *X* and *Y*. Those voters are respectively denoted V_X and V_Y . The third voter can be considered a swing voter, and is denoted V_S . Figure 1 illustrates optimal points for these three voters in the policy space: X^* , Y^* , and S^* . The axes are labelled *defense programs* and *social programs*, and units are arbitrarily defined so that quantities lie in an interval from 0 to 10. The illustration shows that V_X prefers a lot of defense and relatively limited social programs, while V_Y prefers less defense and more social programs. V_S takes moderate positions, preferring less of each than the most ardent advocate, but slightly more of each than the most ardent opponent. Significantly, his optimum does not lie on the line segment connecting the optima of V_X and V_Y . This line segment is the contract curve between voters V_X and V_Y , and the fact that V_S 's optimum is not located on it implies that Plott's (1967) sufficient condition for a majority voting equilibrium will not be satisfied. Indeed, there will be no majority rule equilibrium—that is, no platform that can defeat all other platforms in pairwise majority votes. This

Figure 1. Two-Party Competition Under Perfect Information



is the typical result when the issue space cannot be represented in a single dimension.

We will now consider the policies proposed by the party leaders in a sequence of elections. To begin, we arbitrarily assume that in the first election, each party proposes its optimum as its platform. Since the swing voter, V_S , is closer

to party Y's optimum than to X's, he votes for party Y along with V_Y , and party Y takes office. By assumption, in the next election party Y is constrained to retain its position, and the leader of party X will choose a winning policy to maximize his or her utility.

To see what this implies about the policy chosen by party X, we draw V_S 's

indifference curve through Y^* , V_Y 's optimum, which is the prevailing policy. In order to be elected, party X must offer a policy that will induce V_S to defect from the party Y . To do so, while maximizing party utility, party X 's leader will choose a point like point X_1 , which lies on the contract curve between X^* and S^* just inside V_S 's indifference curve through Y^* . By assumption, some small but finite movement closer to S is needed; if the movement were infinitesimally small, V_S would abstain. So party X is elected and policy X_1 is imposed.

Now consider V_S 's indifference curve through X_1 . In the next election, party Y 's leader wishes to maximize party utility by selecting an appropriate policy. To do so, he chooses a point just inside V_S 's indifference curve through X_1 , but on the contract curve between Y^* and S^* . So point Y_1 is chosen, and party Y is elected.

A pattern has been established. In this example, leaders of the two parties are forced by competition to move from their own optima toward the optimum of the swing voter, along the contract curve between them. With each election, the winning policy is getting a little closer to V_S 's optimum. However, that point is never actually reached. Suppose that, after a number of elections, party X wins by proposing point A , and that, in the following election, party Y wins by proposing $B + \epsilon_S$ —that is, a point on the contract curve between V_Y and V_S that lies ϵ units beyond B in the direction of S .

Would party X respond in the next election by moving yet closer to V_S 's optimum? Clearly not, since they could instead choose point C , which would yield the party higher utility, and induce V_Y , by tradition the party Y loyalist, to defect and vote for party X . Party Y could then defeat C by once again bringing V_S into the winning coalition by proposing $F + \epsilon_S$ —that is, by moving to a point along the contract curve between V_S and V_Y that is ϵ units away from F toward V_S .

Party X could then once again court the swing voter by choosing an appropriate point between E and A , and the cycle would repeat itself.

Essentially, an equilibrium cycle develops, and the set of policies in the cycle will all be located in small neighborhoods around line segments \overline{EA} and \overline{BF} and point C in the diagram. Policies for party X are most often located on segment \overline{AE} , while those for party Y are most often on \overline{BF} . Thus, pronounced party differences are observed, on average, even though parties occasionally shift policy dramatically (by going to point C) to attract a potential defecting opposition loyalist.⁵

Points A , B , and C are the points in "the competitive solution," a game theory construct that can provide a benchmark in our analysis (see McKelvey, Ordeshook, and Winer, 1978; and Miller, 1980; for a similar point without reference to the competitive solution, see Petry, 1980). The effort to win an election is seen here as an effort to form a winning coalition. Voter V_S is pivotal between two potential winning coalitions. The movement towards S^* is equivalent to a series of bargains that offer V_S more and more utility up to the limits offered at A and B , where V_S is indifferent between the two coalitions. Similarly, C is the point where each party loyalist is indifferent as between a coalition with the swing voter and a coalition with the opposition party loyalist. Each voter in a winning coalition is rewarded for supporting that coalition by a policy position just close enough to his or her ideal point to assure that he or she would not prefer another point in the competitive solution. Thus, the competitive solution has some appeal as a positive theory of coalition formation under majority voting, apart from the role it plays in the dynamic model we have developed above.⁶

What, then, are the important results emerging from this example? First, cycling occurs, but is limited to a rather small

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subset of the policy space that includes the competitive solution points, and is essentially repetitive. Second, persistent party differences in policy prevail on average. Third, despite the previous finding, occasionally a party leader will propose a policy designed to lure the other party's natural constituency, and in doing so must adopt policies that seem rather close to those typically offered by the opposition, at least on some issue(s).

Models with Voter Uncertainty

The model described above is limited by its simplicity and by its deterministic character. In the real world, voters do not have perfect information about candidates, and they make their decisions on the basis of considerations such as candidate appeal, retrospective evaluation of incumbent performance, and party loyalty, as well as on the basis of policy issues. We have modified the previous model in such a way as to approximate these features of reality.

The central change is the introduction of voter uncertainty about candidate issue positions.⁷ The resulting stochastic model of electoral choice yields qualitatively different results regarding the series of positions taken by the candidates. Also, it provides a basis for introducing other, nonpolicy considerations and showing how they affect the pattern of results.

We continue to work with examples in which three voters and two party leaders have optima defined in a two-dimensional issue space. As before, voters' utilities are lower the farther away current policy is from their optimum. Now, however, we assume that as a result of voter uncertainty about candidates' positions, voters might not always vote for the party whose policy is closest to their optimum. Voters receive or collect bits of information that provide clues to party locations, but they can occasionally draw incorrect

conclusions about which party is closest. The greater the difference in the distances from the voter's optimum to the locations of the two parties, the less likely it is that voters will be incorrect in their assessment of which party is closest. We therefore assume that the probability that voter i will vote for party X is a function of the difference in the distances from i to each of the parties. The closer party X comes to voter i 's optimum (holding party Y 's position fixed) the more likely the voter is to vote for party X . If party X 's policy is much closer to i than party Y 's, then the probability of i voting for X will approach one; if party X 's policy is much farther away from i than is party Y 's, then the probability of i voting for X will approach zero. In these models abstention is ruled out; if i does not vote for X he is assumed to vote for Y .

In our examples we use the following specific functional form to express the probability that voter i votes for party X :

$$P_{iX} = \frac{1}{1 + e^{\beta_i |D_i(x) - D_i(y)|}}, \quad (1)$$

where

x is a vector giving the policy position of party X ;

y is a vector giving the policy position of party Y ;

β_i is a parameter to be described below; and

$D_i(z)$ is the distance to a given vector, here denoted z , from i 's most preferred point in the policy space.

This equation simply says that the probability that i votes for X is given by a logistic function of the difference in the distances to the parties. This specification embodies those properties described in the preceding paragraph.⁸

The parameter β_i measures the sensitivity of voter i to policy differences between the parties. If β_i is near zero, then voter i is not at all sensitive; that is, even if one

party were much closer to i than the other, there would only be about a fifty-fifty chance of i voting for the closer party. When β_i is very large, voter i is almost certain to vote for the closer party, even when the party policies are quite similar.

Once again we assume a Downsian scenario of sequential elections, where incumbents are constrained to run on their records, but challengers are not constrained to do so. The motivation of challenging parties is again to attain favorable policy, and electoral victory is simply a means to that end. More specifically, the challenging party is assumed to minimize the expected post-election distance of the elected party's policy from the challenging party's optimum. This behavior is equivalent to maximizing expected utility, where the challenging party's position in issue space is the control variable.

In the case of party X challenging incumbent party Y , the problem is to:

$$\text{Minimize } L = P_X(x, y) [D_X(x)] + [1 - P_X(x, y)] [D_X(y)], \quad (2)$$

where

$P_X(x, y)$ = the probability that X wins the election, given that party X takes position x and party Y takes position y ;

and

$D_X(z)$ = the distance from party X 's optimum to a given policy vector.

The probability P_X is derived using laws of probability given our earlier assumptions characterizing individual voter behavior. For example, for three-voter examples like those to be described

$$\begin{aligned} P_X(x, y) = & P_{1X}(x, y)P_{2X}(x, y)[1 - P_{3X}(x, y)] + \\ & P_{1X}(x, y)P_{3X}(x, y)[1 - P_{2X}(x, y)] + \\ & P_{2X}(x, y)P_{3X}(x, y)[1 - P_{1X}(x, y)] + \\ & P_{1X}(x, y)P_{2X}(x, y)P_{3X}(x, y), \end{aligned} \quad (3)$$

L turns out not to be a very tractable function. It is not globally convex in x , and can have multiple local minima. It is for these reasons that we perform simulations and solve models numerically instead of proceeding analytically.⁹

In this model, incumbents no longer necessarily lose; there will simply be some given probability of losing. However, if an incumbent is fortunate enough to win several successive elections, that does not change the challenger's optimal policy. When incumbents win, the same policies will be offered repeatedly until the challenger finally succeeds.

Since we are primarily interested in the sequence of policies that will be chosen, and since incumbents presumably will eventually lose, we install the challenger as the new incumbent after every election in our simulations. The reader should be aware that the reported sequences of policies should not be taken to imply that incumbents always lose, however. Repeated incumbent victories are possible, and we will show below that in some circumstances they are likely.

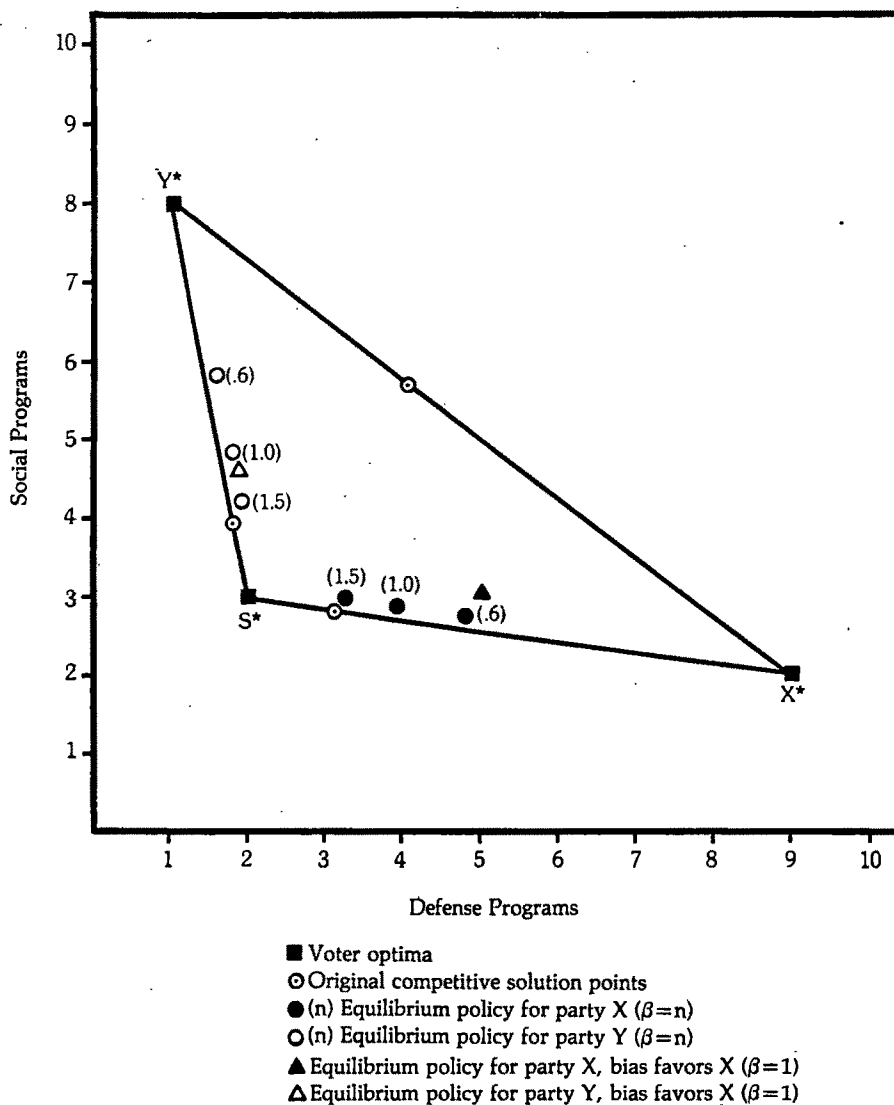
We are now ready to report on the outcomes of sequential policies resulting from electoral competition in simulations of the model described. In all experiments, the "sensitivity" parameters, β_i , are assumed to be identical for all voters, so the subscript is dropped hereafter. We have arbitrarily assumed that party X is initially the incumbent and have selected its leader's optimum as the initial prevailing policy.¹⁰

Varying Voter Sensitivity to Policy Differences

In the first experiment, we have applied the model of elections under voter uncertainty to exactly the same scenario described above in our model of elections under perfect information. Three voters, V_X , V_Y , and V_S , have the same preferences as before for combinations of defense and social programs, and one voter is located

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Figure 2. Two-Party Competition Under Conditions of Voter Uncertainty



at the ideal point of each of the parties; β is set equal to one for each of the voters.

In this first experiment, parties sequentially proposed policies that moved them away from their respective optima toward that of the swing voter. The paths followed were close to the contract curves between party loyalists and the swing voter, but were actually located in the

interior of the Pareto set. Policies did not reach the competitive solution points or begin cycling, however. Rather, the party leaders' policy paths converged to equilibrium points well short of points A and B in the competitive solution. The pair of equilibrium points for this experiment is designated by the label for $\beta = 1.0$ in Figure 2. Note that these points are also

compatible with static equilibrium, in the sense that each party leader is satisfied with his policy choice there, given that his opponent is also at his equilibrium point. At the equilibrium point, the party whose optimum is closer to that of the swing voter has a slightly higher probability of winning.

We have performed a group of similar experiments replicating the one described above, but which imposed values for β ranging from 0.6 to 1.5. Recall that smaller values of β imply that voters are less sensitive to policy differences. In each of these experiments, the qualitative results were like those of the experiment just described: party policies moved toward the swing voter in successive elections, but converged at equilibrium points short of the competitive solution points.¹¹ Equilibrium points for these experiments are labelled for the appropriate value of β , and are superimposed on Figure 2. For higher values of β , the equilibrium pair is located closer to the competitive solution point. Thus, in this group of experiments, greater sensitivity of voters limits the ability of party leaders to impose their own preferences, and improves the welfare of the swing voter. It therefore follows that candidate ambiguity, to the extent it limits voter sensitivity, makes the parties better off in the long run at the expense of the swing voter. One might then view candidate ambiguity as a device that helps to achieve interparty collusion.

In Figure 3 we illustrate some results of an experiment in which voter sensitivity further increased, so that $\beta = 2.0$. In this case, a cyclical pattern similar to that discussed for the case of perfect information (i.e., perfectly sensitive voters) results. This is intuitively plausible. For $\beta = 0$, one would expect the party leaders to ignore the swing voters, who are completely insensitive to policy, and propose their own optima for policies. As β increases, parties must partially accommodate the swing voter, and the equilibrium

points move toward the swing voter's optimum. When β is sufficiently large, and voters very sensitive, the results should approximate those for the perfect information case. The sequence of policies shown in Figure 3 illustrate a repetitive, or equilibrium, cycle that eventually results in this experiment. The equilibrium cycle may not be unique; a "mirror image" of the illustrated cycle could occur if an alternative starting point were chosen for the experiment. In other experiments with yet higher values for β , similar but more symmetrical cycles resulted. As in the perfect information model, party leaders occasionally adopt policies that may appear to be more appropriate for a candidate of the opposition party, but on average, parties' policies are closer to their own optima.

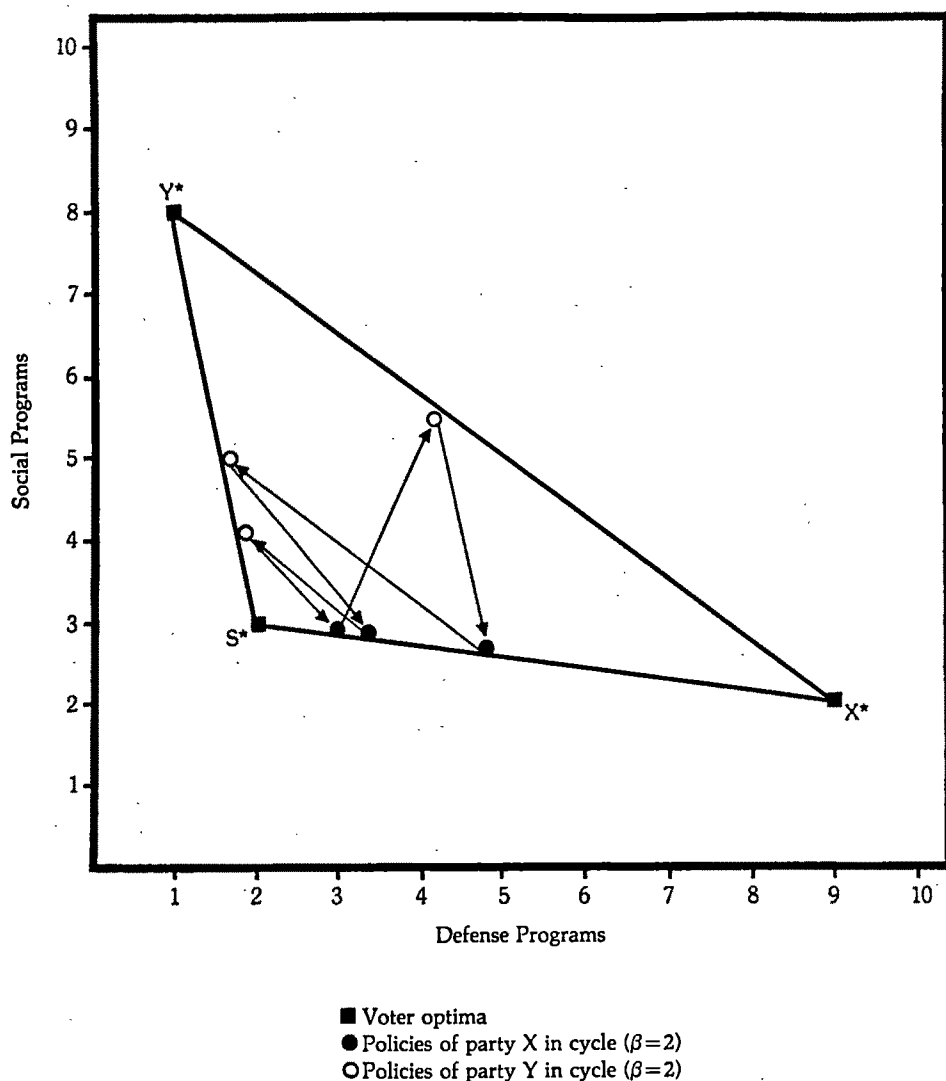
Since cycling and the paradox of voting, of which it is an example, have often been viewed as evidence of collective irrationality, it may seem ironic that greater sensitivity to issue differences leads to cycling. We will return to this issue in the conclusion.

Winning for Its Own Sake and Electoral Bias

In an alternative experiment, we have modified the model so that parties care about winning for its own sake, apart from the policy benefits winning may bring. In the previous experiments, winning an election was simply the way to come as close as possible to achieving policy goals. Now we incorporate the likely possibility that the motives involved in seeking office are still more complex, and that candidates are willing to make some policy concessions in order to win.

This change was accomplished by adding an additional constant utility loss of two units, to be suffered whenever the challenging party fails to win the election. For $\beta = 1$ and for the same voter ideal

Figure 3. A Cycle Induced by High Voter Sensitivity



points, the result was an equilibrium similar to that in the corresponding experiment in which there was no extra concern for winning. In this experiment, however, the equilibrium points were drawn slightly farther away from the parties' optima and into the interior of the Pareto set.

In the experiment just described, with $\beta = 1$, the extra desire to win did not induce a cycle. However, an extra concern for winning was sufficient to induce a cycle when β was set at 1.5 (recall that in the original group of experiments, $\beta = 1.5$ resulted in an equilibrium pair, i.e., no cycle). This indicates, as we would expect,

that the added concern with victory is similar to greater voter sensitivity in making the model perform more like the perfect information case. Both phenomena draw party positions away from party optima and towards swing voters. Both phenomena lead to cycling if they are strong enough, because getting closer to the swing voters makes attracting defectors (and cycling) a more appealing strategy.

We have also run an experiment permitting voter bias. In this experiment, voters may favor one party over another, even if the parties are equidistant from the voter in spatial terms. This could result from retrospective evaluations of incumbent performance, such as those characterizing many recent studies of the effects of the economy on voting. Other sources of nonpolicy bias include party identification and responses to personalities or to demographic attributes.

Our original experiment was modified so that voter bias favors party X. Specifically, each voter now sees party X and party Y as equally desirable to him or her when party X is two units farther away from that voter than is party Y. With β set at 1.0, an equilibrium pair was again generated. One effect of the shift was to increase substantially the probability that the advantaged party would win each election. The bias had a limited effect on the position taken by the disadvantaged party, which moved only slightly towards the swing voter. The effect on the position taken by the advantaged party was much greater. Party X was able to move its position substantially towards its ideal point, while still enjoying an increased prospect of victory (see Figure 2).

Such a pattern is consistent with our expectations for a period of party dominance. The minority party may win from time to time, and the advantaged party does not have to move as far from its preferred policies in order to win as it would in the absence of bias. These results are

also quite similar to those reported by Wittman (1983) in his analysis of voter bias.

Further Comparative Dynamic Experiments

When voters' optima shift, this will of course affect the policies offered by the competing parties, regardless of whether static equilibrium or cycling characterizes the initial state. We have, therefore, undertaken experiments in which a voter's optimum changes, and traced the sequence of policies that followed. One would of course suspect that if a voter's optimum moved in a particular direction, that policies in an equilibrium pair (or the set of policies in a repeated cycle) would tend to move in a similar direction. In several experiments we have undertaken, this is indeed the case. This reinforces the intuitively appealing comparative static results reported in Wittman (1983), and suggests that analogous results typically apply for the majority rule political mechanism we have described, even when there is no static equilibrium pair of strategies. We cannot claim that this is a general result,¹² but we have not yet encountered an exception in several experiments.

A special case of some interest involves the shift of the swing voter optimum toward the contract curve between the two party loyalists' optima. Such a shift implies that the distribution of voter optima more closely approach lying along a single dimension. In such a case, the resulting equilibrium points (or points in the repeated cycle) also lie closer to that single dimension. When voters are sensitive to policy differences, these points will be close to the swing (median) voter. Together these results suggest the applicability of an "approximate" median voter result; median voter theories will be useful predictors, even when the assumption

of a single underlying dimension only holds as an approximation.

Larger Numbers of Voters

Our three-voter examples have helped simplify our analysis, while at the same time approximating the real world case in which there are small numbers of well-defined blocs of voters. In an additional set of experiments, we have considered larger numbers of voters whose most preferred points are scattered over the policy space. More specifically, voter optima were randomly generated from a uniform distribution covering the interval from 0 to 10 for each issue.

Computing exact probabilities of challenger victory is computationally burdensome when there are many voters. However a simple and accurate approximation is possible. First, as before, the probability that a particular voter will choose to vote for the challenger is generated using the previously defined logistic function, given the difference in the distances from the voter to the respective parties. Next, we compute the mean and variance for the random variable Z , which is defined to be the sum of the votes for the challenging party. Z will have mean Σp_i and variance $\Sigma [p_i (1 - p_i)]$, and, by a generalized version of the central limit theorem, is approximately normal for large numbers of voters (see Cramér, 1946, p. 213). The probability of a challenger victory (i.e., that Z exceed $N/2$, where N is the number of voters) is then simply found by retrieving the appropriate value from the normal distribution function.¹³

In other respects, the design of our experiments with many voters parallels those for the three-voter case with uncertainty. The most important features of these experiments are summarized below:

1. With 30 voters, and with the party optima located at (5,0) and (5,10), sequential strategies converged to equilibrium pairs in several experiments. The equilibria were in the vicinity of the center

of the policy space; however, distinct and substantial party differences still appeared. Results of one of these experiments (with $\beta = 2.5$) are depicted in Figure 4. Results were similar in experiments assuming 100 or 500 voters.

2. Replicating the preceding experiment, but increasing β to 5.0, resulted in repetitive cycles that are also pictured in Figure 4. Within the cycles, strategies were limited to the central region of the policy space, and persistent party differences reflecting leader preferences occurred on average once again.

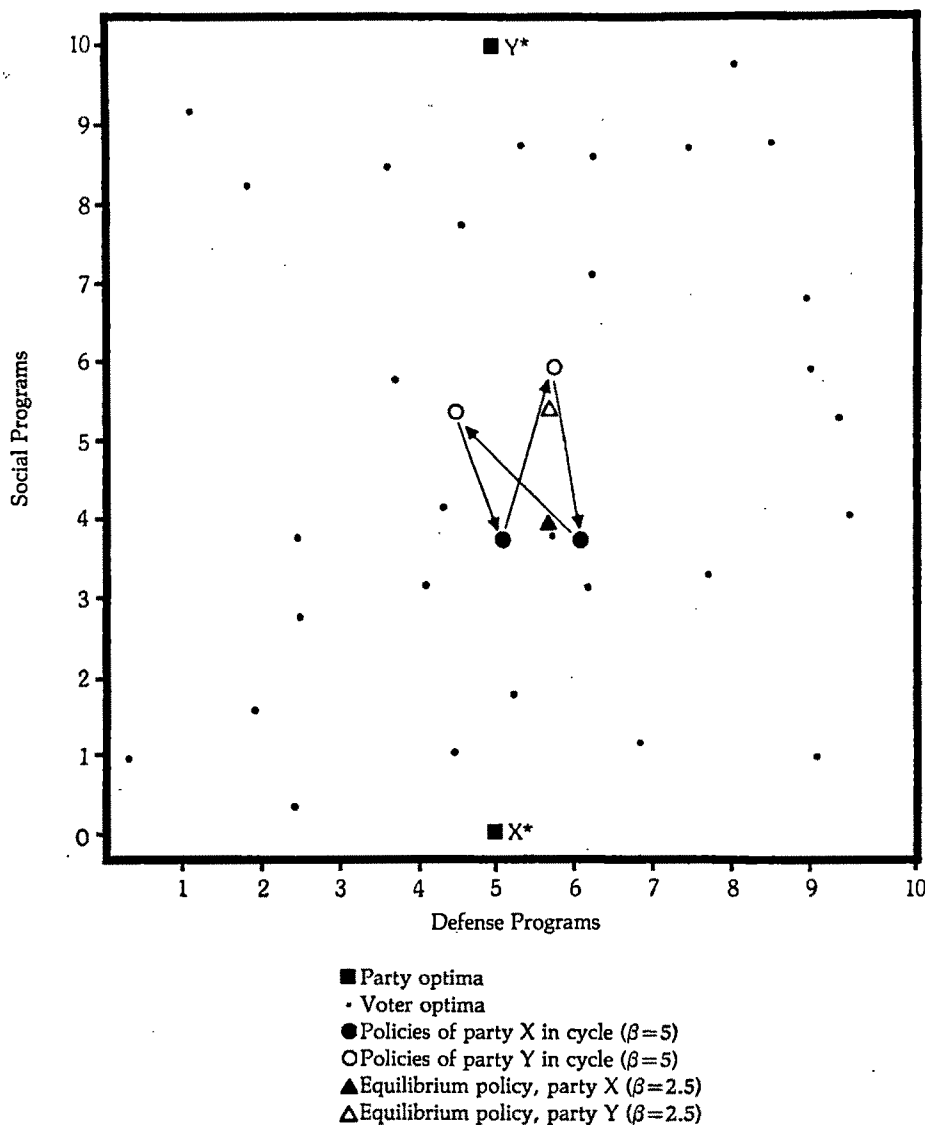
3. Comparative dynamic experiments yielded intuitively plausible results very similar to those reported earlier for the small numbers case. The effects of voter bias and a desire for winning in and of itself were also similar to analogous results for the three-voter examples.

Nonindependent Voter Decisions

Although the preceding experiments with modestly large numbers of voters result in persistent party differences, this may not be the case when the number of voters becomes extremely large. Arguments analogous to those of Tullock (1964) imply that for a very large number of voters, whose optima are distributed uniformly and whose voting decisions are independent of one another (as they are in our experiments above), party platforms would converge on a median position. Indeed, party positions were closer together for our 500-voter experiments than for our 30-voter experiments.

By extension, with millions of voters we might expect the party positions to be virtually indistinguishable. There are several reasons why a general version of our model may still predict party differences, even with very large numbers of voters. First, as we have already mentioned, large numbers of voters may not be uniformly scattered, but instead clustered in a few locations, like the

Figure 4. Two-Party Competition With a Larger Number of Voters



points in our three-voter example. Second, voter sensitivity to issue differences may not be uniform, as we have assumed, but rather increase with distance from the center of the issue space, resulting in greater polarization (see Rabinowitz, 1978, for an argument supporting this view). Similarly, if parties

selectively inform voters about platform choices through advertising, they are most likely to inform those who will react favorably—that is, those located nearby. This is also likely to lead to greater sensitivity of voters located at the party extremes, and more polarization.

Here, we choose to focus on a third

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reason for persistent party differences, even in the case of very large numbers. Contrary to the assumptions of our previous experiments, the decisions of all voters may not be independent of each other.¹⁴ One illustration of possible effects on nonindependence comes from our previous experiments, if we suppose that the "voters" in those experiments are actually blocs of thousands of voters who always vote for the same candidate. Those results would then indicate party differences, even when there are many voters. Although real world voting blocs are not so cohesive, their impact will likely be to preserve the tendency for party differences, because they approximate the situation with smaller numbers of voters.

A lack of independence of voter decisions can enhance differences between policy-motivated parties in another way. For example, if party identification has an impact on voting apart from underlying spatial similarities, or if unanticipated events affect voters within particular groups in related ways, party differences can be enhanced.

Lack of independence of voter decisions can have the same effect, even if an unanticipated event moves all voters in the same direction. For example, a foolish statement or a strikingly poor performance in a presidential debate may move all voters in one direction. Since candidates are policy motivated, the uncertainty about who would be the beneficiary of such an unpredictable event leads them to take positions closer to their optima.

We illustrate this point with an additional experiment, using debate outcomes as a concrete example of a random event that may affect many voters in similar ways. In this experiment, we suppose that the two candidates choose platforms as before, but that after platforms are announced, the candidates will engage in a debate. The debate will influence voters, because it reveals evidence about candidate attributes (perhaps leadership at-

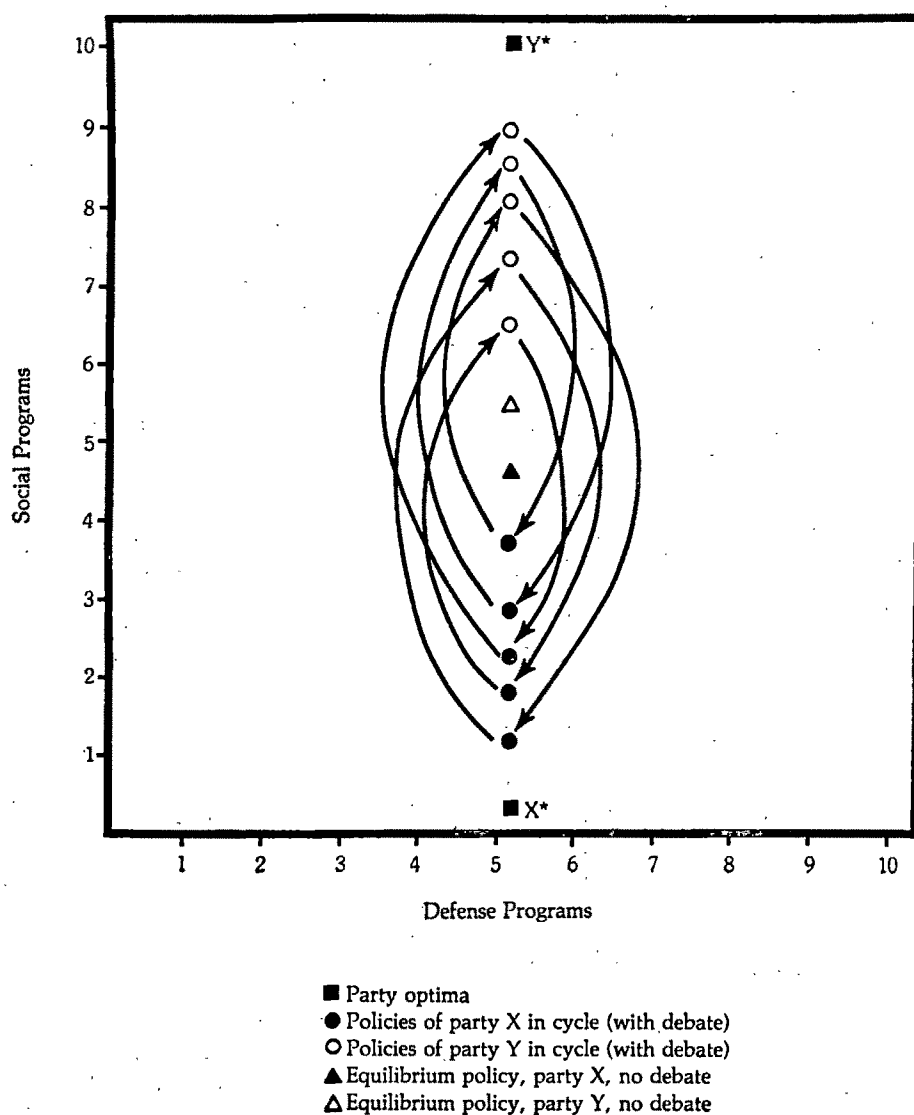
tributes or ability to react to stress) that cannot easily be revealed through other campaign activities. Each candidate is assumed to have a 50% chance of "winning" the debate a priori. After the debate, each voter will vote as if he or she were one unit closer (in spatial distance) to the candidate who won the debate, reflecting one of the ways in which voters' decisions may not be independent.

As a consequence of this process, for any platform a challenger proposes, his or her predebate probability of winning the election is now computed as an average of his or her respective probabilities of victory under the two possible debate outcomes. Although predebate probabilities that voters will vote for a given candidate are not changed by adding the debate institution, the probability that a candidate will win the election given his or her own and the opponent's strategies generally is changed, because of the voter interdependence that has been introduced.

Failure to select a median policy no longer unduly risks defeat in the case of very large numbers of voters, since a candidate may win anyway if he or she is fortunate enough to win the debate. So long as there is an a priori chance of winning, candidates will wish to offer policies closer to their own optima than to the opponent's. Thus, party differences will persist on grounds such as these, even when many voters are evenly dispersed throughout the policy space.

Our experiments confirm this result for large finite numbers of voters. The introduction of debate effects does lead to larger party differences, and these differences do not appear to approach zero when there are very large numbers of voters. Interestingly, as Figure 5 illustrates, cycling can occur. In this experiment, in which 1521 voters were spread evenly over the policy space, cycling was limited to points on (or quite near) the line connecting the optima of the two parties, rather than all over the Pareto set, let

Figure 5. The Effect of a Debate



alone the entire issue space. Our previous findings that cycles are limited in scope and that party differences persist on average are preserved.

These results are of some independent interest with respect to the analysis of debates. Usually debates are viewed simply as a device for making the public aware of existing candidate differences.

Our results show that debates not only reveal such differences, but may actually encourage them.

Conclusions

Two features distinguish our model of party competition in majority rule elections from much of the literature: policy

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motivation on the part of party leaders, and uncertainty on the part of voters. Both features are more realistic than the alternatives. Politicians do seem to care about policy as well as winning, and voters are uncertain about party positions. These features not only add realism; they also lead to results that are more plausible and familiar than those characterizing much of the literature.

The model is rich enough to permit variations in its features, and these produce associated patterns of outcomes that are observed in the political world. Politicians can vary in their balance of concern for policy and winning office, and voters vary in their sensitivity to party differences. Politicians can differ significantly in their attractiveness to voters on nonpolicy grounds, and therefore in their prospects of winning votes with given policy positions. All of these variations can be accommodated in the model, and produce sensible and plausible consequences, enhancing our confidence in the appropriateness of the basic approach. In all variants of the model, there remain persistent party differences, whether in the form of equilibrium pairs, or in the form of cycles that reflect enduring differences on the average.

The experiments show that parties can win while taking positions relatively close to their own optima when voters are not very sensitive to party differences. As voters become more sensitive to these differences, distinctions in party policy become smaller until outcomes begin to cycle, as they do in the perfect certainty case. Similarly, enhanced concern of parties with winning leads to breakdown in equilibrium and to cycling.

At first it may seem odd that enhanced voter sensitivity to issue differences and enhanced party concern with winning should contribute to the apparent "collective irrationality" of cycling. *A priori*, one might expect both to imply more voter control of collective decisions. It may also

seem odd that less voter sensitivity to issues and less vote motivation by parties is associated with more clearly defined and distinct choices between parties. Clarity of choice has typically been associated with popular control, but here the relationship seems to be reversed.

However, as Nicholas Miller (1983) has pointed out, the cycling that seems irrational to the social choice theorist is very similar to the regular turnover of winners and losers that pluralist theorists have praised. What may seem like collective irrationality to the social choice theorist may contribute to the stability of the political system as explained by the pluralists. In any case, the cycling that exists in these models is recognizably similar to what is empirically observed, namely, moderate, but persistent and predictable party differences.

Notes

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1. A continuing issue in political science is whether, and under what circumstances, such activists are oriented more towards electoral victory, towards policy, or towards other personal benefits. See Schlesinger (1975) and Abramowitz and Stone (1984).

2. Calvert (1985) identifies conditions under which policy-motivated candidates will converge to a majority-rule equilibrium in a multidimensional issue space, when such an equilibrium exists.

3. These assumptions are identical to those employed by Wittman (1977).

4. So long as party leaders are included as relevant agents in defining the Pareto set, there is no agenda setter who would have a short-term incentive to propose an alternative outside that set. Therefore, any cycling we find will be limited to the Pareto set, unlike the possibilities identified by McKelvey (1976).

5. We have also analyzed several examples with more than three voters and have found qualitatively similar results.

6. The competitive solution points also have some normative appeal. This appeal derives from the rewarding of all members with a policy they find as desirable as any they could expect from another coalition, in effect giving each member his or her market value. The appeal derives also from the way the solution takes into consideration the desires of the excluded member. Winning coalitions that came closer to equal division between winning members would be less desirable to the losing minority.

7. We could also relax the assumption of perfect information on the part of candidates, but we think voter uncertainty about candidate positions is more central to the electoral process than the reverse, because candidates have more resources and incentives to gain accurate information about voters, and have strategic reasons to take ambiguous positions in order to enhance electoral prospects. See Page (1978, pp. 176-79).

8. It also implies that random variables indicating individuals' voting choices are statistically independent. Some effects of relaxing this assumption are considered below.

9. A FORTRAN program was employed to find the expected utility-maximizing policy strategies in sequences of elections.

10. Qualitative, and in some cases quantitative, results did not change when alternative starting assumptions were made.

11. Convergence to unique equilibria did not vary given alternative starting points for the sequential election process.

12. Given that equilibrium cycles need not be unique, it is conceivable that the intuitively appealing comparative static results could fail to generalize when there is cycling. However we have not found an example of this.

13. We are indebted to George Rabinowitz for suggesting to us this technique for increasing the number of voters in our experiments.

14. That is, the probability distribution functions for the random variables indicating the voters' decisions are unlikely to be statistically independent.

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ON THE STRUCTURE AND SEQUENCE OF ISSUE EVOLUTION

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How do political issues arise, and come to affect political party politics? We develop a theory and model of issue evolution, illustrating both by examining the dynamic evolution of the issue of racial desegregation. Our modeling concerns two central problems: (1) the structure of the evolution—a pattern of dynamic causality between the early policy cues from professional politicians, in Congress in the case at hand, and later mass response, and (2) the sequence of changes in elite behavior, changes in mass perceptions of party issue stances, changes in mass affect toward the parties, and changes in party identifications among citizens. We suggest that the causal process developed for the racial case is quite general for other times, other nations, and other issues. The theory of issue evolution is developed as a general statement of the organic connection between elite and mass behavior, a working model of the dynamics of American politics across time and issues.

Most issues most of the time lie dormant, stirring interest only in those especially informed and in those especially affected. They lend no weight to the color, tone, and meaning of partisan debate. They neither define party systems nor undergird party alignments. But occasionally issues rise from partisan obscurity and become so contentious, so partisan, and so long lasting that they come to define the party system in which they arise, to transform the grounds of debate which were their origin. This joint transformation of issues and party systems, which we call *issue evolution*, is realignment in the ordinary English usage of that term. It is a dynamic

process resulting in the change of issue alignments. But the realignment concept has taken on such multiple and conflicting meanings from both popular and scientific usage as to make it an increasingly dubious vessel for the development of empirical theories of politics.

Mass party realignments, according to some theorists such as Schattschneider (1960), Sundquist (1983), and Riker (1982), may be interpreted as the redistribution of party support associated with the displacement of one political conflict by another. Viewed from this perspective, realignments are precipitated by the emergence of new issues about which the electorate has intense feelings that cut across,

rather than reinforce, the existing line of cleavage between the parties. As the parties respond to the new issue dimension, they redefine the basis of the party cleavage with a new line of political conflict overlaying the old. Finally, a redistribution of partisan support occurs when the mass electorate responds to the new line of conflict represented in the party system.

This process may result in a new majority party. The new line of conflict may, alternatively, simply alter the coalitional structure of the parties. Whatever the ultimate systemic outcome of the process, its primary evidence at the level of the mass electorate is the increasing polarization between partisan supporters on the new issue dimension. But mass issue polarization is actually only the result—the most visible, cumulative effect—of a complex and multifaceted process. The causal nature of this underlying process, which we refer to as issue evolution, remains largely obscure and almost totally unexplored. What is the dynamic causal process that leads to mass policy realignment? Considerable light can be cast on this question, we believe, by examining the structure and sequence of issue evolution. First we outline a general causal model of the process, then look at specific evidence relating to its individual and

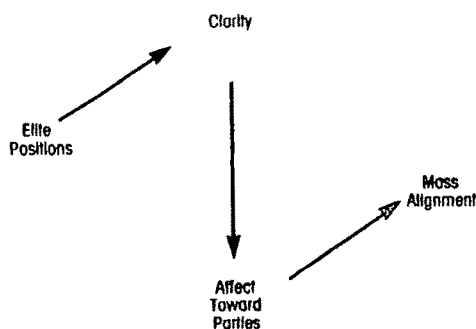
separate elements, and finally conduct a more formal statistical analysis to uncover the dynamic causality inherent in the issue evolution process.

A Model of Issue Evolution

Most policy debate occurs among elected and appointed officials at the center of government; most attracts no significant public notice. When, occasionally, an issue moves from the limited "policy" environment to the larger stage of partisan politics, we naturally look for its partisan origin to those elite actors who framed the issue in partisan terms in the first instance. Figure 1 outlines the ensuing sequence of the issue evolution process. Following elite reorientations on contentious issues, comes a delayed, more inertial reaction in the mass electorate. When the elite polarization in progress is first confronted squarely by the mass electorate, the reaction often takes the general form of a *critical moment*. The critical moment is a mass polarization along the new line of issue cleavage large enough to be noticeable, but considerably less dramatic than the critical election of traditional realignment theory. Partisan conversion and electoral mobilization are the causal mechanisms that produce such rapid change. Critical moments occur, we presume, with some frequency. The new linkages between issues and parties which these critical moments establish often lose electoral relevance as quickly as it was gained. But in some cases the critical moment becomes the signal event for a less dramatic, but more substantial secular redefinition of the issue bases of political life. This secular reorientation reflects the continuing recognition of the changed positions of the parties after the critical moment, and it is driven by normal population replacement.

Changes in elite partisan behavior do not lead directly to mass partisan response. Rather, two intervening steps are

Figure 1. The Sequence of Issue Evolution



necessary to link elite policy shift to mass issue realignment. First, the mass public must alter its cognitive perceptions of the parties with respect to the new issue dimension. Taking its cues from elite partisan actors, the mass electorate must recognize a difference in the positions of the parties on the new issue. But even changed perceptions, by themselves, are not likely to lead to changes in mass issue alignment. For issues to move voters to change their partisan identifications at the critical moment and bias the recruitment of new identifiers thereafter, the issue must also invoke a strong emotional response.

Changing perceptions of the parties must carry with them a heavy dose of affection and disaffection for the parties if they are to weigh against the stubborn inertia of existing partisan identifications. The public must not only perceive a difference in party issue stands, but it must also care about this difference. Only when these two intervening conditions are met—clarified mass cognitive images of the parties and then polarized affection toward them—will issue redefinition among partisan elites lead to new policy alignments among the mass electorate.

Changes in the components of party image, moreover, should be temporally bounded between the elite policy reorientation that is the beginning of the process and mass issue alignment which is its end. Time ordering is critical. Redefinition of the link between issue and party, however tentative and perhaps even unintended it may be, is a process that must begin with elite actors. In an environment where many policy cues are given and most are ignored, the crucial role of the mass electorate is to choose to respond to some cues. Which set of actors—political leaders or mass public—is the most important element of the process is probably an unanswerable question; we shall not in any case attempt an answer. But political leaders are assigned priority in time.

The Evolution of Racial Desegregation

The issue dimension we have chosen to examine is racial desegregation. While numerous issues have crowded onto the political agenda since the New Deal and competed for public attention, political analysts have singled out the issue of race as having the greatest realigning potential (Sundquist, 1983; Petrocik, 1981; Carmines and Stimson, 1981). Race has deep symbolic meaning in American political history and has touched a raw nerve in the body politic. It has also been an issue on which the parties have taken relatively clear and distinct stands, at least since the mid-1960s. Finally, the issue has had a long political life cycle. It has been a recurring theme in American politics as long as there has been an American politics and conflict over race has been especially intense since the New Deal. For those reasons, if a significant issue evolution has taken place in contemporary American politics, it has most likely revolved around the issue of race.

We turn now to the evidence. It will be presented in two parts. First we will see the evidence for the fact of issue evolution. Because the case is made elsewhere (see Carmines and Stimson [1981, 1984, 1986] and Carmines, Renten, and Stimson [1984] for various pieces of the picture, and also Petrocik [1981] for similar conclusions from a different focus), we will be brief. This first section is used to introduce the time series components of the model one at a time. The point to be made here is a simple one: that something happened. Our analysis is accordingly simple. Making minimal assumptions of our data, we will, for each of our interparty difference series to come, present a simple before and after test for the hypothesis that 1964 represents a breaking point in all the series. The congressional series are annual; the others, with minor differences, are biennial.

Part two of the exposition is devoted to the structure and sequence of issue evolution, the central focus of this work. When we turn to the evidence on structure and sequence, we will shift tack to make stronger assumptions by treating all series as annual, to abandon the implicitly linear dynamics of the before and after t test and its arbitrary focus on a single change point, and thus to explore the limits of a refined analysis of causal dynamics.

The Evidence for Issue Evolution

We argue that visible changes in elite behavior serve to redefine party images, to affect emotional response to the parties, and ultimately to realign the constellation of voter issue attitudes and party identifications to reflect earlier changes among the elite. We require indicators of each of these concepts: elite behavioral change, party image, citizen emotional response, and identification/issue alignment.

Party *elite behavior* is many things: acts of presidents, congresses, party officials, the parties in convention, and so forth. We have at one time or another looked at all of these, and each is arguably important in the issue case at hand (as would be the courts, except for their inability to lend partisan structure to issue conflicts). We choose to focus here on Congress, and specifically on roll call votes, because these frequent and public acts present a clean summary measure of what the parties truly stand for. While other elite actors, most particularly presidents, are undeniably important in reshaping party issue stances, none presents a regular pattern of behavior that is objectively and cleanly quantifiable. Our focus on congressional behavior should be seen not as an argument that Congress defines best what the parties support or oppose, but rather that it presents the best lever-

age for developing operational indicators of the messier whole of elite party behavior.

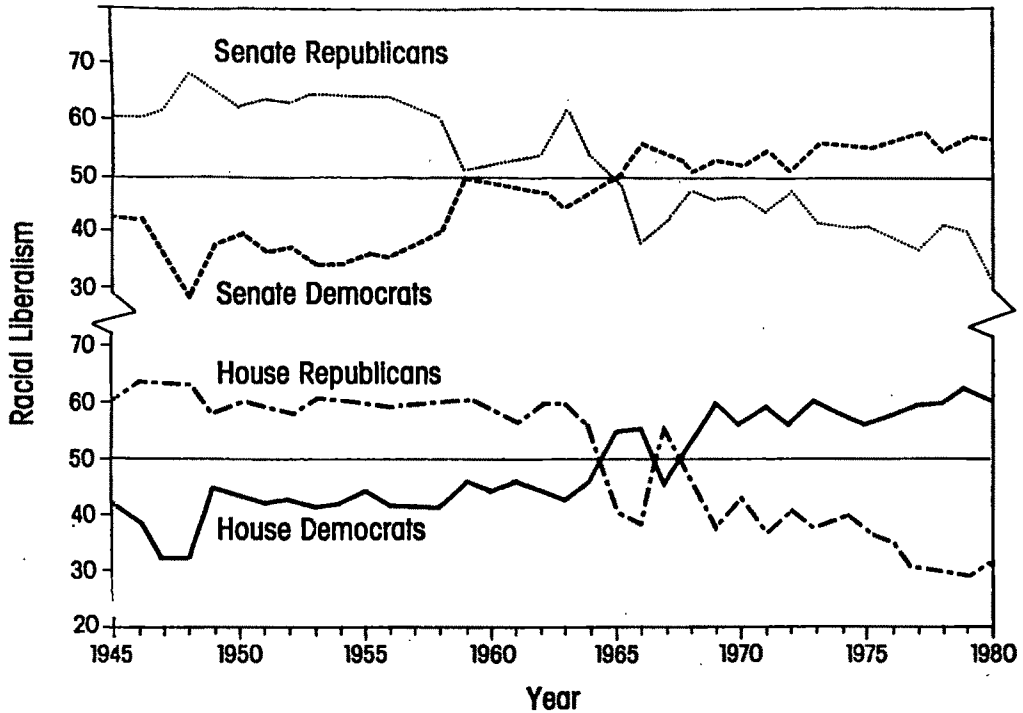
For the purpose of charting elite policy stands, we have used roll call votes cast in every session of both houses of Congress from 1945 to 1980. Following procedures developed by MacRae (1970) and Clausen (1973), we have developed standardized racial desegregation scales for all members of Congress, which are aggregated into party means for the following analysis.¹ These series represent the issue positions of the two parties on racial issues since the beginning of the postwar era.

Figure 2 presents the mean party positions on racial issues in the postwar era in the Senate and House. Examination of the figure reveals quite distinct and very strongly determined patterns of behavior. At the beginning of each series, the Republicans—then the “party of Lincoln”—were clearly the more moderate (and liberal in relative terms) of the two parties. Democrats were disproportionately southern in numbers, leadership, and image; the Democratic party had not yet developed its hard core of northern liberalism that would later become a counterweight to Southern influence, and still later dominate it. By the end of the series the parties had reversed their positions; Democrats were not only considerably more liberal in the aggregate, they were more liberal in all regions. Indeed, even southern Democrats are now less conservative on matters of race than the Republican party as a whole.

When we look at the differences between party positions over time (indicated by simply subtracting the Republican from Democratic mean positions for each year), striking patterns of racial issue evolution are in evidence. Beginning in the late-1950s the behavior of the parties tracks an unmistakable dynamic evolution toward a fundamental redefinition of the grounds of issue cleavage. Three significant movements in the Senate series

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Figure 2. Desegregation Liberalism in the Senate and House of Representatives, 1945 to 1980

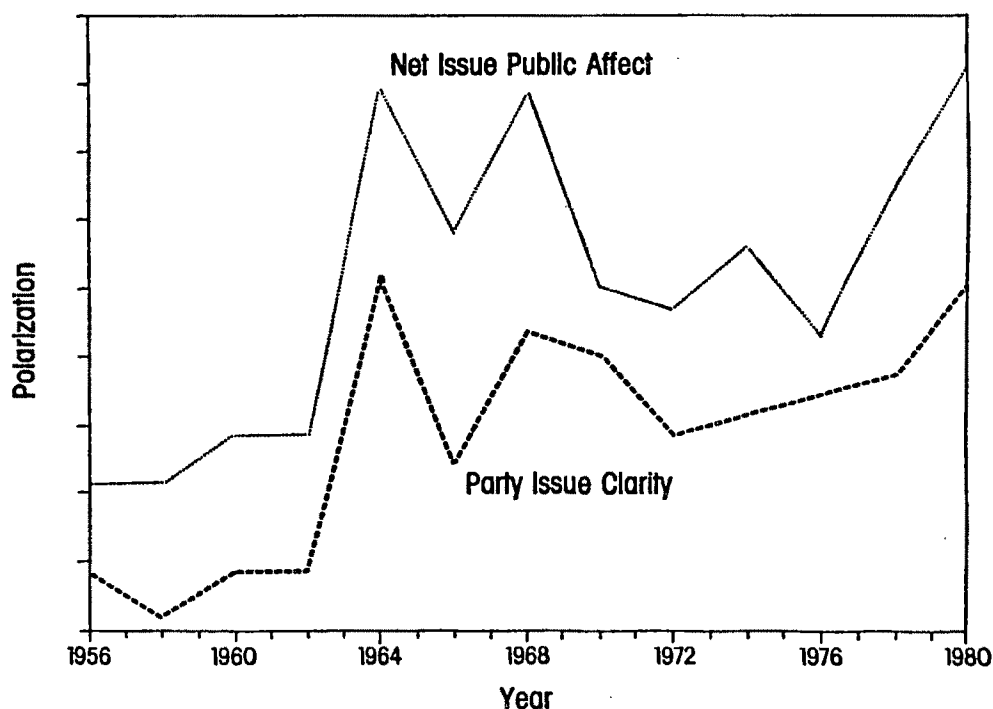


(see Figure 2)—sharp movements in 1959 and 1965, and a gradual growth process beginning around 1970—contribute to the ultimate redefinition of party stands. The House series mirrors the 1959 Senate shifts on a smaller scale and then begins a continuing dynamic growth process in 1965. The 1959 through 1963 movements in both houses are not a new polarization over race, but are movement toward the erosion of the old pattern of greater Republican liberalism, a necessary precursor to new polarization. The politics of the time may be seen as the beginnings of assertion by northern Democrats of a new majority status in their party, a six-year struggle to control the direction of Democratic policy that culminated by 1964 in unquestioned liberal control. Earlier liberal attempts to pass civil rights legis-

lation achieved only limited success at changing the law, but appear to have been highly successful in laying the groundwork for a new Democratic liberalism that would so dominate the later 1960s. All of our indicators will show significant issue evolution, but the congressional series are the most striking.

To establish an indicator of the public perception of the party stances we turn to the University of Michigan Center for Political Studies (CPS) biennial national election series for a data source. Specifically, we examine questions tapping respondent perceptions of where the parties stand on racial questions. The data on perceived party issue positions are in three different question formats spanning three periods, 1956–1958, 1960–1968, and 1970–1980. The most dramatic changes in

Figure 3. Polarizing Clarity of Party Racial Issue Stands and Issue Public Affect Toward the Two Parties



issue perceptions occur, fortunately, within the common 1960 to 1968 format. To insure comparability of data collected under varying formats, we reduced the level of measurement for all years to the categorical variable, "Democrats more liberal," "Republicans more liberal," or "no perceived difference." With suitable manipulation (see technical appendix), all three question formats can be reformulated to yield the three categories.

The measure of mass perceptions of party issue positions—to be called *Clarity* following Pomper (1972)—is the aggregate percentage of each survey sample declaring the Democrats more liberal on desegregation minus the similar percentage of those perceiving the reverse ordering. Peak values are attained when respondents both see the parties as dif-

ferent and achieve consensus about which is more liberal. Lack of consensus or perception of no difference, both of which are common before 1964, reduces the measure. We took considerable care to maintain the conceptual linkages between the early series and the later ones. The empirical evidence bears witness to our success. Figure 3 shows both consistent behavior in the series across format changes and impressive variation within common formats.

The voters of Figure 3 failed to distinguish between the two parties on racial grounds until just before 1964 (before/after $t = 5.5$; $n = 12$) as noted at the time by Converse, Clausen, and Miller (1965). As late as 1960 (and, from other series, probably through about mid-1963), voters saw no difference between the par-

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ties, responding 22.7%, 21.3% and 55.9%—Democrats, Republicans, and no difference, respectively—to a question asking which party “is more likely to see to it that Negroes get fair treatment in jobs and housing.” That lags notably behind the congressional series which show clear signs of movement toward changing party positions following the 1958 elections. It is equally, if less obviously, the case that mass perceptions lag behind the steady party polarization of later years as well, catching up at intermittent opportunities (such as 1980) years after the congressional parties changed.

We have seen direct evidence of the changing racial images of the Democratic and Republican parties. Whether it matters we have not yet seen, for evolving mass party images by themselves are necessary but not sufficient to account for issue evolution. More than clarity of perception is required if evolving party issue positions are to cause systematic issue sorting among the party identifiers. The issue must matter. It must strike home with enough force to influence the emotional ties between citizens and parties. As a first approximation of such emotional links we look to the simple affections and disaffections citizens display toward the parties over time.

To measure *issue public affect* we turn again to the CPS series and pursue a two-step indirect strategy. It is intentionally indirect to sidestep the “rationalization” and “projection” phenomena (Brody and Page, 1972) that are likely to plague any respondent commentary on the links between policy, affect, and party. Respondents who hold distinctive positions on the desegregation issue are isolated in the first step. Then, the positive and negative feelings expressed for each of the parties—without regard to race—by these distinctive issue groups form a summary measure of net issue affect. Our indicators then tap whether racial liberals and conservatives have differential overall evaluations of the parties.

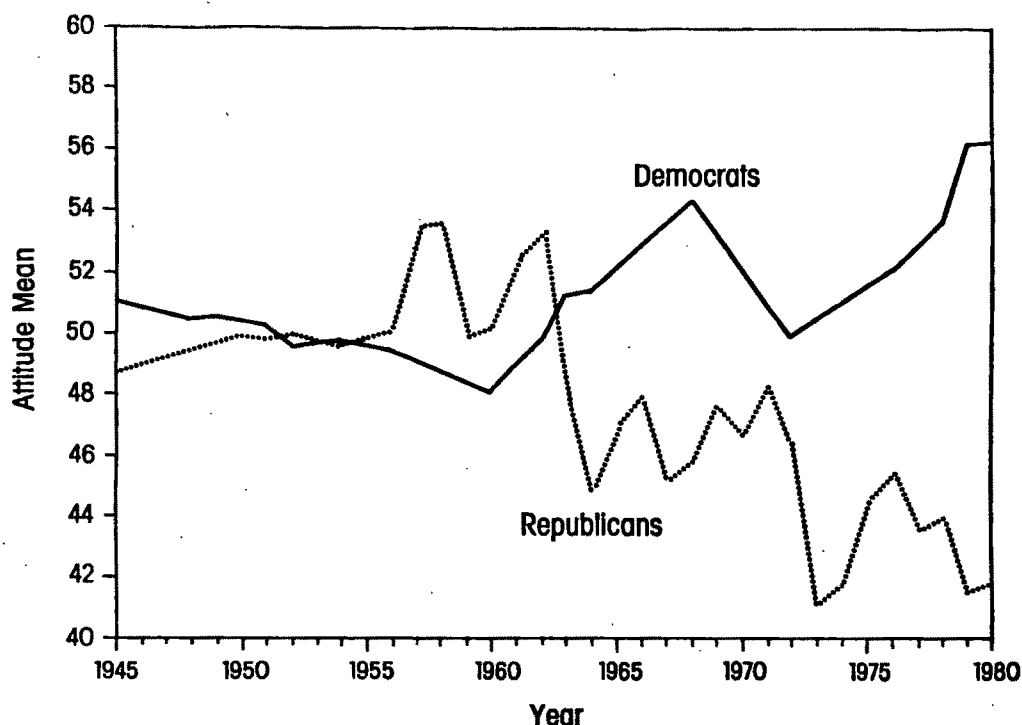
To isolate racial liberals and conservatives, we build a scale of desegregation liberalism for every CPS study where measures of both party affect and racial attitude are available (i.e., every presidential and off-year study from 1956 onward, excluding only 1958 and 1962).² Racial liberals and conservatives are then defined arbitrarily to be the highest and lowest quartiles on the scale. The middle quartiles are excluded on the rationale that “indecisives” on an issue have no grounds for emotional response to party position taking. Our goal here is to develop an aggregate measure of party affection and disaffection among racial issue publics. These relatively extreme quartiles are taken as operational indicators of “issue publics.”

Affection and disaffection for the parties are tapped by the best available valence measures in each study. That, in general, entails the use of open-ended “likes and dislikes” about the parties for presidential studies and feeling thermometer ratings in off-year studies when the open-ended materials are unavailable. Both measures are scored in the Democratic direction—that is, positive scores indicate greater warmth for the Democrats than for the Republicans, and both are adjusted to a standard (50,25) metric for all years.

The net measure allows us to gauge whether citizens with distinctive issue positions reflect their issue biases in their emotional response to the two parties. Figure 3 shows a nonsignificant ordering of preferences before 1964. Racial issue publics—including blacks—liked the two parties about equally well in 1956 and 1960. Affect toward the two parties became clearly related to issue positions after 1964 (before/after $t = 3.4$; $n = 11$). And although the data are altogether independent of the cognitive images of Figure 3, the two patterns in the figure are suggestively similar, a matter we take up more formally below.

Systematic movements, something

Figure 4. Racial Liberalism of Democratic and Republican Party Identifiers



more than year to year fluctuation, would be expected to lead to something, and that is the final link in our analysis. The ultimate demonstration of the existence of an issue evolution is to show significant redistribution of public opinion on a policy issue among party identifiers. The new alignment of issues and party is the final result of the process of issue evolution, and the one that justifies the importance of all the others. It is the semi-permanent redefinition of the grounds of party issue conflict that gives evolving issues an importance considerably beyond the normal grist of electoral politics.³ Its measure here is the simple interparty difference on desegregation issues, the mean position of all Democratic identifiers for a given year less the Republican mean.

Figure 4 displays the causal effect to be explained: the growing racial attitude

polarization of the identifiers of the two parties. Figure 4 plots the desegregation attitudes of party identifiers from the SRC/CPS national election series for 1956, 1958, 1960, 1962, 1964, 1966, 1968, 1970, 1972, 1974, 1976, 1978, and 1980 (and from a Harris survey of November, 1963). Racial attitudes, an equally weighted summation of the survey items available in each cross section, are scaled with a common metric for all cross sections and reconstructed backward in time to create a continuous annual series.⁴

A sharp polarization along racial lines occurred in the turbulent 1960s (before/after 1964 $t = 5.3$; $n = 14$). Democrats became increasingly more liberal while Republicans became steadily more conservative, as some accounts suggest (see particularly Pomper [1972] and Converse, Clausen, and Miller [1965]). What is less

expected—and hence more interesting—is that the polarization continued to grow during the 1970s when racial issues were no longer prominent on the political agenda.

A pattern of growth *following* the decay of the stimuli which created the initial polarization suggests simply that something else was going on. Polarization with a self-sustaining dynamic must be more than a response to visible “events.” By postulating intervening processes that lag behind and then dynamically adjust to the reality produced by initial events, we will account for that self-sustaining dynamic.

To this point we have asserted causal connections between the varied components of issue evolution and presented evidence that is largely visual and intuitive. We turn now to a more formal analysis of our causal assertions, using transfer function analysis to uncover the dynamic causality within and between the various series.

The Evidence for Structure and Sequence

To examine the “sequence” in structure and sequence we will entertain dynamic formulations that allow the expression of causal effects between variables (series) in a manner that incorporates both empirical and a priori specification of delay, resistance, and dynamic adaptation. Our methodology is well developed in the time series literature, but sufficiently distinct from normal political science approaches that some words of explanation are in order. We will present our evidence fairly directly here, with little exposition of the statistical models and modeling techniques. Those matters are taken up in the attached appendix.

Estimating dynamic relationships asks much of our data, and we have chosen to push them to their limit. In some cases that is easily done. Our congressional

series, based upon all the available roll call data for both houses of Congress for 36 years each, present an exceptionally clean look at the aggregate party behaviors. The survey data, on the other hand, are limited by the biennial structure of American elections and the questions survey institutions choose to ask. We employ 15 independent studies to construct our series for *Clarity*, what citizens think the parties stand for; *Affect*, how issue publics feel about the parties; and *Alignment*, the polarization of identifier issue attitudes. For *Alignment* (AL) we reconstruct 1945 through 1951 and odd-numbered years later (where independent surveys are not available) from recalled party identification of survey respondents. The perils of this technique are now well known (Niemi, Katz, and Newman, 1980). They are manageable here because for the period of SRC/CPS coverage, we rarely have to push recall beyond one year. The effect of errors of recall of party identification will be to introduce unwanted “noise” in the year-to-year changes of party identification/racial policy alignment (ΔAL).

Our intervening variables, *Clarity* (of issue perceptions) and *Affect* (of issue publics toward the parties), are measured biennially at best. For these variables we postulate no change between elections (i.e., $\Delta X = 0$) or before the first available surveys. If independent evidence for these cases existed, we would expect to see only sampling variation in the year-to-year change measures between elections. This amounts to the assumption that only election years matter in the evolution of links between issues and parties. The style of our analysis with these variables is much akin to intervention analysis (or to regression with dummy variables) where 0 and 1 values are postulated to stand for absent and present. The difference here is that in place of the 1s, we have empirical estimates of the direction and magnitude of change. The 0s indicate the absence of

expected change. Again, if the assumption is false, the expected effect is conservative; a constant zero will neither be predicted by (when dependent) or aid in the prediction of (when independent) another continuously measured series of first differences. (In the appendix we report confirmatory [static] regression models based only upon the independent surveys where no such off-year assumptions are required.)

Estimation

We begin estimation by examining the dependent variable itself, mass party identification issue alignment (*AL*). This examination serves a statistical, not theoretical, purpose; it erects a benchmark for later explanations by accounting for the variation in *AL* which can be explained by the history of the series itself. To establish an explanatory benchmark we first fit a univariate noise model to the dependent series. As is traditional in Box-Jenkins approaches, we treat this noise model for our dependent series as a matter of no theoretical consequence, but a necessary prior step in order that later causal analyses will not be confounded by this source of extraneous variance. The *AL* raw series is nonstationary⁵—it does not fluctuate around a stable equilibrium—and thus we must model its first differences (Δ), the year-to-year changes that ultimately determine the level of the series.

A moving average process of the second order (MA[2]) fits the univariate *AL* noise series—that component of issue and identification alignment that is best accounted for by the history of the series itself. This is a sensible result given the two-year periodicity of American elections and of the election studies from which these data are constructed. The moving average process is marginally significant ($t = 1.8$) and produces a modest reduction in the residual mean square (a measure of un-

explained variance) from 5.40 for the series modeled with no parameters to 4.97 for the Integrated Moving Average of the second order model. That latter value, the predictive error when the series is modeled entirely as a function of its own history, is a baseline against which the presumed causal explanations may be measured.⁶ It is a conservative procedure in this case to treat the noise model as significant. That way the later causal explanations will not receive credit for accounting for this noise variance in the dependent series.

Elite to Mass Linkages

We introduce the Senate net party difference time series (S_t) in the transfer function analysis of Model 1 (Table 1).⁷ Prewhitened cross correlations between the Senate and mass alignment series suggest an identification of the causal pattern between series that is in accord with what we expected to see. We know a priori (see Figure 5) that the first significant party movement toward new issue alignments in the Senate series is in 1959 (following the very large-scale replacement of racially liberal northern Republicans by liberal Democrats in the 1958 Senate elections) and that the first notable sign of changing mass racial issue alignment appears in 1963, when the Kennedy Administration first embraced the program of the Civil Rights Movement as its own.⁸ Thus we have reason to suspect a four year lag between the two series.

Because both the Senate and mass alignment series evolve toward greater issue polarization, we could not be certain a priori whether a static or dynamic causal connection would exist between the two series. The data indicate a static one. Using the Senate series to predict mass party alignment produces a significant parameter ($\omega_0 = .23$, $t = 5.9$) and reduces the residual mean square on the order of 11% from the benchmark level.

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**Table 1. Explaining the Alignment of Racial Attitudes and Party Identification:
Two Transfer Function Models**

Variables and Fits	Mass Issue Alignment is Dependent Explanatory Variables Are:	
	(1) Senate Alignment (Zero Order)	(2) Senate and House Alignment (Zero Order)
Senate Alignment ^a (Lagged 4 Periods)	.23 (<i>t</i> =5.9)	.12 (<i>t</i> =2.7)
House Alignment (Lagged 2 Periods)	—	.13 (<i>t</i> =3.3)
Moving Average (θ_2)	.94 (<i>t</i> =34.7)	.95 (<i>t</i> =25.9)
<i>Measures of Fit</i>		
Residual Sum of Squares	128.13	91.74
Degrees of Freedom	29	28
Residual Mean Square	4.42	3.28
Improvement Over Benchmark Residual Mean Square (4.97)	11.1%	34.0%
<i>Autocorrelation</i>		
$Q^b \sim \chi^2$	5.9 (<i>df</i> =11)	13.0 (<i>df</i> =11)

Source: Annual time series computed by the authors from the 1952–1980 National Election Studies conducted by the University of Michigan Center for Political Studies, Harris Survey #1285, and from roll-call votes of the U.S. Senate and House of Representatives, 1945–1980.

^aAll series, independent and dependent, are first differences.

^b Q is the Box-Ljung (1976) Q statistic for small samples, distributed as χ^2 :

$$Q = (N(N+2)) \sum_{i=1}^k r_i^2 / (N-i)$$

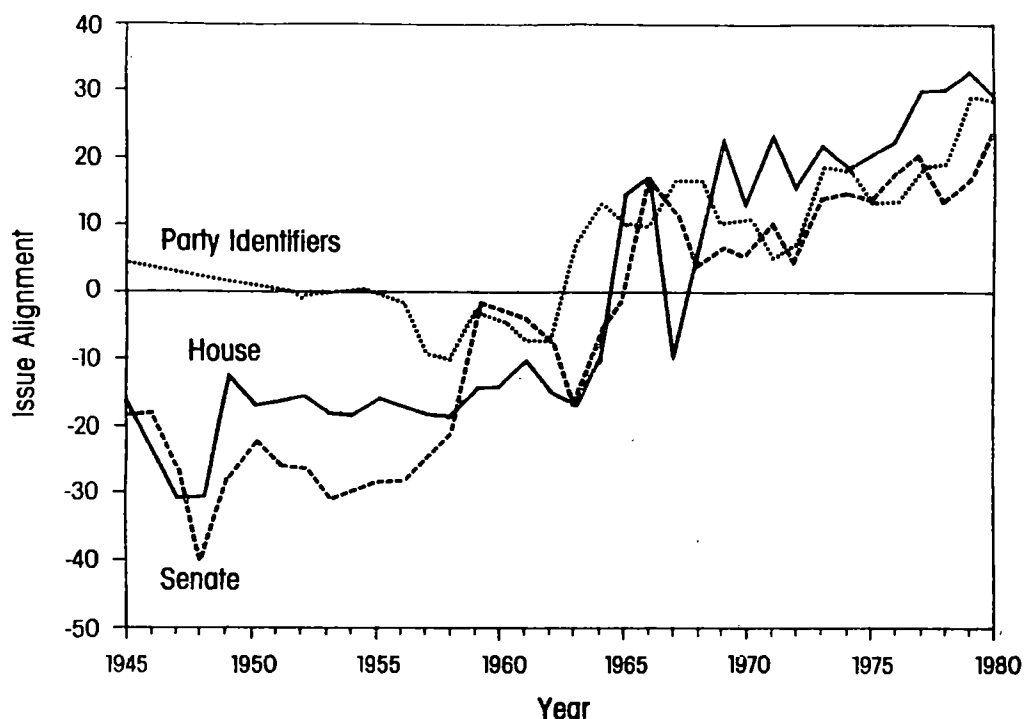
The form of the estimated model is linear because the postulated (nonlinear) dynamic adaptation process is already present in the independent Senate series. And since both track the same basic— S curve—dynamics, no additional between-series dynamic adaptation is present.

The House issue alignment time series (H_t) is introduced as an additional explanatory variable in Model 2 of Table 1 (see also Figure 5). While the raw Senate and House time series show much the same evolutionary pattern, the first dif-

ferences in the two series (ΔH_t and ΔS_t) are only partially collinear ($r = .28$). The House series is much more inertial. It resisted most of the racial polarization that appeared in the Senate for some six years (1959 to 1965). But once the new alignment developed, it was more steadily maintained than in the Senate. Thus, the House offers a partially overlapping and partially different set of policy alignment cues that might have been publicly perceived.

Model 2 of Table 1 shows that the

Figure 5. Senate, House, and Party Identifier Alignments Together
(Net, Democratic minus Republican Positions)



House and Senate series share about equally in explaining mass issue alignment. The combination of the two reduces the residual mean square by about one-fourth over that produced by the Senate alone, and some 34% over the benchmark level. Taken together, the two models of Table 1 offer evidence that elite party behavior, as manifested in our congressional indicators, may cause later mass issue alignment.

Establishing simple causality raises the question whether the *intervening* causal connections we have postulated are the correct ones. That is partly established by theory, for the intervening links are virtual logical requisites. The elite-to-mass issue alignment connection is fundamental. Empirical evidence of that linkage requires theoretical explication of plausi-

ble mechanisms which might translate change at one level into change at another—and perhaps in the process account for dampening and delay. Mass response to changing elite issue positions would seem to require reasonably accurate perceptions of those positions; and the polarization of affect in response to changes in party position requires clarity of issue perceptions.

Intervening Linkages

Working backward from mass alignment, we model the linkage between Affect and Alignment and then between Clarity and Alignment in the analyses of Table 2. Model 1 fits a first-order transfer function between Affect and Alignment. The resulting model produces the ex-

1986 Structure and Sequence

Table 2. Mass Issue Alignment as a Function of the Polarization of Party Affect and the Clarity of Party Positions

Variables and Fits	Mass Issue Alignment is Dependent Explanatory Variables Are:		
	Affect Only	Clarity Only	Affect and Clarity
<i>Affect</i>			
δ_1	.27 ($t=2.1$)	—	.27 ($t=2.0$)
ω_0	.58 ($t=7.3$)	—	.59 ($t=7.2$)
<i>Clarity</i>			
δ_1	—	.35 ($t=2.6$)	—
ω_0	—	.19 ($t=6.3$)	.01 ($t=0.4$)
Moving Average (θ_2)	—	.56 ($t=3.0$)	—
<i>Measures of Fit</i>			
Residual Sum of Squares	69.77	71.67	69.38
Degrees of Freedom	33	32	32
Residual Mean Square	2.11	2.24	2.17
Improvement Over Benchmark Residual Mean Square (4.97)	57.5%	54.9%	56.4%
<i>Autocorrelation</i>			
$Q \sim \chi^2$	11.5 ($df=11$)	15.1 ($df=11$)	11.3 ($df=11$)

Source: Annual time series computed by authors from the 1952-1980 National Election Studies conducted by the University of Michigan Center for Political Studies, and Harris Survey #1285.

pected dynamic response—inertial resistance followed by adaptation—and a handsome improvement in our ability to account for alignment. The residual mean square, 2.11, from this estimation of but two parameters represents a 57% reduction in unexplained variance from the univariate benchmark. As an intervening link between elite party behavior (H_t and S_t) and mass alignment (AL_t), Affect should be more strongly associated with the dependent series and, by quite a margin, it is.

Following Pomper (1972) we postulate

mass clarity of issue perception as a precondition of alignment. We further specify that the effect is indirect, through changing affect toward the parties among those who care deeply about an issue. That implies (a) a bivariate linkage between Clarity and Affect, and (b) a mediated—not direct—link between Clarity and Alignment. We address the simple effect of Clarity on Alignment in Model 2 of Table 2.

The form of the causal impact of Clarity on Alignment, a first-order transfer function, is similar to the Affect to Align-

ment relationship. Clarity differs from Affect by (1) having a considerably smaller initial impact, (2) joined with a larger dynamic adjustment, and (3) thereby leaving a smaller total effect. The Clarity estimation leaves systematic variation in the model residuals, requiring an additional parameter to model an MA(2) process. The Clarity to Alignment linkage is clearly significant, but slightly weaker than the Affect to Alignment relationship. It leaves more variance unaccounted for and requires an additional parameter in the process. The result is a marginally higher residual mean square.

The Clarity to Affect bivariate linkage is postulated to be instantaneous—both series are attitudes easily free to covary within our annual time interval—and thus can be estimated with a linear regression. The two series track one another quite closely even in their differenced form. The regression:

$$\text{Affect}_t = .57 + .40\text{Clarity}_t \quad (1)$$

produces an R^2 of .89. Its slope is easily significant ($t = 16.8$) even in a small sample. Measured for aggregates—and therefore lacking the near automatic covariation from cognitive balance effects expected for individuals—and by very different techniques, the two components of party image retain nonetheless a striking empirical connection.

Whether Clarity works directly on mass alignment or is indirect through Affect is tested in Model 3 of Table 2. The result is unequivocal: the effect of Clarity on Alignment is entirely indirect, through Affect. Model 3 with both series in the estimation equation looks virtually identical to Model 1 (Affect only) with the addition of a substantively trivial and statistically nonsignificant parameter for the direct effect of Clarity on Alignment. That is reflected in a residual mean square higher than that produced by Model 1, a result of a lost degree of freedom without a compensating gain in explanatory

power. The appropriate diagnostic evidence (the cross correlation function between prewhitened Clarity and the residuals of Model 3)⁹ rules out misspecification as a possible explanation of this result; it shows no evidence of any connection between Clarity and the residuals at any positive or negative lag.

The linkage between elite behaviors, here the Senate series,¹⁰ and Clarity can be modeled as a transfer function:

$$\Delta\text{Clarity}_t = .58\Delta S_{t-4} + (1 - .62B^2)a_t \quad (2)$$

which demonstrates significant association ($t = 2.9$) between movements in U.S. Senate voting patterns and perception of party positions, with the same four-year lag we have seen before.

The final causal question we take up is the linkage between party elite behavior and mass alignment, controlling for Affect. Table 1 (showing the simple effect of elite behavior on Alignment) and Table 2 (showing the much stronger effect of the proximate Affect series) suggest that we are likely to observe no unmediated effect. Such is the message of the estimated model:

$$\begin{aligned} \Delta\text{AL}_t = & \frac{.60}{(1 - .24B)} \Delta\text{Affect}_t \\ & - .02\Delta S_{t-4} + .04\Delta H_{t-2} + a_t. \end{aligned} \quad (3)$$

Again, it is virtually identical to Model 1 (Affect only) of Table 2, with the addition of two clearly nonsignificant parameters for the Senate ($t = -.5$) and House ($t = .3$) series. As would be expected, the addition of nonsignificant parameters costs more than it gains, leaving the residual mean square (2.44) notably higher than the Affect-only model. This evidence demonstrates that the intervening causal mechanisms are necessary to connect elite to mass response.

We are left then with evidence strongly in support of the issue evolution model of Figure 1. All of the hypothesized connections are significant. Party behavior

causes mass perception of party positions, causing polarized affect toward the parties among issue publics, in turn leading to alignment along issue lines. None of the longer linkages is significant when modeled with intervening terms in the equation.

We have now traveled the path from observed changes in elite party behavior (the roll call series), to accurate mass perception of party position, to polarized emotional response to the parties based upon issue positions, and back to mass party alignment, which is where we began. We have traced the issue evolution process through each of its empirically distinct, but theoretically interdependent stages.

On Organic Theory and Inadvertent Recognition

We have explicated here a general model of issue evolution. While this model has been used to illuminate the evolution of racial desegregation, which is important in and of itself, our theoretical ambition is greater. We wish to explain, in general, how issue alignments and realignments are driven by mass response to the behavior of participants in national political institutions. Our empirical manifestation of issue evolution is race, but the theory should be equally applicable to other issues, past and future, which combine great salience and longevity with distinctive party movement. So too should it be applicable to other nations. Its assumptions about institutional cues and mass response are not specific to the American political context.

Conventional accounts of party realignment accord a fundamental role to mass electorates, treating institutional actors as responding to more central electoral forces. Such accounts provide a distorted picture of elite/mass dynamics. Tied to the normative democracy and the mechanistic metaphor of the U.S. Con-

stitution, they require political professionals to have knowledge of issues—in both policy and political senses—that is *less* advanced than that of the amateur electorate. The origin of the policy dialogue between politicians and voters must lie, we believe, with the former, who provide definition to a multitude of issue conflicts.

The role of the electorate in issue evolution is to respond to some issues and not to others. The process is analogous to the natural selection of the biological world. Elites provide cues about issue definition. Many in number, complex, and contradictory, most are seeds on fallow ground, ignored by an inattentive electorate. The issue space—that tiny number of policy debates that can claim substantial attention both at the center of government and among the passive electorate—is strikingly limited by mass inattention. Alternatively limited by mass inattention. Alternative issues, or alternative definitions of the same issue, may be seen as competing for a portion of that space—a competition that is highly selective and often unpredictable.

Although elites lead—in the sense of acting first in time sequence—they neither control nor manipulate. However strategic their behavior in developing issue positions as levers to influence masses, the number of policy cues is so large and their effect so unknowable that the process takes on an appearance of randomness. The competition for issue space produces very large numbers of possible issue definitions of party politics. One such definition is occasionally selected when it happens to be well suited to the political environment of the moment. Like organic behavior generally, issue evolutions come to seem sensible, perhaps even inevitable, after the fact. They are almost unknowable before it.

Issue evolution produces representation as a by-product. But unlike the demand-compliance notions that dominate thinking about representative processes, this

representation is inadvertent. It is systemic, not individual. It occurs without any single actor consciously attempting to produce it. Over the span of the desegregation issue, as should generally be the case, we can see evolution from a time when the party system was wholly unrepresentative—offering no coherent positions, no citizen choice—to the current pattern of issue-polarized parties, for better or for worse highly representative of their constituencies in the electorate. This representation is inadvertent because it was produced less by elite response to mass demands than by mass evolution toward existing elite positions.

American constitutional democracy is an exercise in eighteenth-century political mechanics. But the clockworks and balance wheels of its conception seem ill suited to explain its subsequent survival, development, and metamorphosis. For that we suggest that there is much to be gained from the organic thinking of a later century.

Appendix

Notes on First Order Dynamic Specifications

For many of the links in the model of Figure 1 we postulate first-order dynamics. What this entails is that a change in some variable X is followed (after a possible lag of k years) by a series of changes in the effect variable Y . In the model we will entertain, we expect a change in X (ΔX) at t to be followed after k lags by a perceptible change in ΔY (estimated by the parameter ω_0), and a continuing sequence of ever smaller changes in ΔY until it achieves an equilibrium adaptation to ΔX . (But before the equilibrium response to ΔX is achieved, Y is also responding to later innovations [both positive and negative] in X , ΔX_t , ΔX_{t+1} , ΔX_{t+2} , . . .) The decay in the sequence of changes is estimated by the parameter δ_1 (where $0 < \delta_1$

< 1.0) which distributes the causal effect of ΔX to a sequence of ΔY which, though mathematically infinite, in practice decays to a trivial level after a few periods:

$$\Delta Y_t = \frac{\omega_0}{(1 - \delta_1 B)} \Delta X_{t-k} + N_t. \quad (4)$$

If we assume the lack of a systematic noise process in the model residuals—which we do here to simplify illustration, but do not do in the analysis—the model can be written in the more intuitively satisfying form 5:

$$\Delta Y = \delta_1 \Delta Y_{t-1} + \omega_0 \Delta X_{t-k} + a_t \quad (5)$$

which expresses the current change in Y as a function of ΔX lagged k periods, and the previous ΔY , and a random disturbance a_t . Since the previous ΔY is also a function of its previous value and δ must be less than one, then the causal effect of changes in X must decay over time:

$$\begin{aligned} \Delta Y_t = & \omega_0 \Delta X_{t-k} + \delta_1 \omega_0 \Delta X_{t-k-1} \\ & + \delta_1^2 \omega_0 \Delta X_{t-k-2} + \delta_1^3 \omega_0 \Delta X_{t-k-3} \dots \\ & + a_t. \end{aligned} \quad (6)$$

Equation 6 accurately expresses change in Y only as a function of previous change in X —this is not a lagged endogenous formulation—and of random error. The sequence of exponentially decaying change in ΔY becomes growth when ΔY is cumulated back into Y , because it is the rate of change which is decaying.

The first-order model is an attractive conception of dynamic causality. It is likely to find application wherever inertia limits the responsiveness with which effect variables can adjust to changes in their causal environment, a very large class of problems. In the present instance, for example, party identification, if it is to deserve the special status it holds in theories of electoral behavior, cannot be understood as a labile response to the constellation of factors that cause it. The

party identification of our theories is (at least in degree) a lifelong commitment, a standing decision, an ego identification, an ingrained loyalty, a habit, and an expression of solidarity with racial, religious, ethnic, and linguistic peers. The "identification" in party identification gives it a rootedness that makes it not impermeable to changes in the political world, but certainly highly resistant to them.

Policy conflict between the individuals and their respective parties (or attraction to the opposition) over matters of great material or symbolic importance may be understood to produce change in party identification. But the contest between ingrained loyalty and a short-term party attraction or revulsion is unequal; we expect loyalty nearly always to win out. But even as rock erodes under the force of a trickle of running water, sustained for a very long time, so party identification is likely to change in response to long sustained policy cues on matters of personal importance.

Two static approaches to the estimation of such dynamic causal relationships are common. Both are problematic. If we conceive of cause as an event in X at t , the effect of which is distributed over several later values of Y , we can approach the problem statistically by relating cumulative cause with cumulative effect, or by relating change in X with change in Y (perhaps with some lag k). The cumulative approach (e.g., relating the congressional racial time series to the party identification/issue alignment series) will produce stunning levels of apparent covariation, much of which invariably is spurious. If causal connection is present, the cumulative approach will find it, but with very little likelihood of identifying the correct functional form or direction. It is also likely to find it if it is not present. Cumulation—even of purely random variations—induces systematic behavior in time series, any two of which will have high levels of incidental covariation.

The problem of cumulation is the Type I error; it leads to the inference of cause when it is not in fact present. The examination of static relationships between change in X and change in Y has the opposite problem. If the effect of ΔX is distributed over several lags of ΔY , then even choosing the empirically optimal lag between X and Y will still—even with perfect measurement—understate the true relationship. If inertial drag limits responsiveness, then response time becomes stochastic, and we expect to see a distribution around some optimal value of k . No more than a fraction, and perhaps a very small one, of the effect of ΔX , will appear in ΔY_{t+k} . If cause is not present, the method will not find it. It is also fairly unlikely to find it when it is present.

The first-order dynamic model is a middle course. It deals with variations in X and Y , not their cumulations, and hence avoids the spuriousness problem. It is in fact exceptionally rigorous against spurious covariation, an attractive property. But within the constraint of a parsimonious formulation, it also allows for a distributed effect of ΔX on ΔY that is both more realistic than a static formulation and much less vulnerable to Type II errors. It errs on balance toward the conservative side, because true causal effects which do not conform to the constraint of the first-order model do not count for the hypothesis.

Notes

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1. See Carmines and Stimson (1986) for a de-

Table A.1. Regression Respecification of Table 1

Variables	Mass Alignment (ΔAL) Dependent	
	Senate Only	Senate and House
ΔS_{t-4} : Senate (Lagged 4 years)	.15 (SE=.12)	.23 ^a (SE=.13)
ΔH_{t-2} : House (Lagged 2 years)		.21 ^b (SE=.14)
Constant	.64 (SE=1.20)	-.36 (SE=1.31)
<i>N</i>	13	13
R^2	.12	.28
\bar{R}^2	.04	.14

Source: Annual time series computed by authors from the 1952-1980 National Election Studies conducted by the University of Michigan Center for Political Studies, Harris Survey #1285, and from roll call votes of the U.S. Senate and House of Representatives, 1945-1980.

^a $p < .05$ one-tailed test.

^b $p < .10$ one-tailed test.

Table A.2. Regression Respecification of Table 2

Variables	Mass Alignment (ΔAL) Dependent		
	Affect Only	Clarity Only	Affect and Clarity
$\Delta Affect$	1.72* (SE=.33)		1.20* (SE=.58)
$\Delta Clarity$.20* (SE=.05)	.08 (SE=.07)
Constant	.12 (SE=.67)	.47 (SE=.75)	.17 (SE=.67)
<i>N</i>	13	13	13
R^2	.72	.64	.75
\bar{R}^2	.69	.60	.69

Sources: Annual time series computed by authors from the 1952-1980 National Election Studies conducted by the University of Michigan Center for Political Studies, and Harris Survey #1285.

* $p < .05$ one-tailed test.

tailed analysis of the congressional series and for notes on scaling procedure.

2. Items and scales for the presidential studies are the same as those used in Carmines and Stimson (1981) and in Figure 2. For the congressional studies and the more recent 1980 study similar items are utilized.

3. Such issue redefinition by similar logic could be expected to lead also to issue dealignment, a prospect explored in detail in Carmines, McIver, and Stimson (1982), but well beyond the scope of this article. The evidence of that analysis suggests quite clearly that racial (and other policy) attitudes do predict individual movements away from party identification.

4. Each annual scale is composed of racial desegregation policy items that form the first principal component in analyses of all racial items. In general, the items deal with respondent preferences for federal desegregation policy (not including school busing, where the evidence suggests powerful influence of extraneous issue dimensions). The now-controversial reconstruction methodology and its specific application to reconstructing partisan racial attitudes are examined in detail in Carmines and Stimson (1984). That analysis confirms that the recall data are indeed problematic, as is reported "party identification" itself. We have limited the use of reconstruction methods to the period before 1956 and to odd-numbered years thereafter. One particularly crucial odd year, 1963, has been estimated from a Harris survey of November, 1963, completed (but not released) before the Kennedy assassination. See Munger (1977) for more detail on this well-timed exploration of public attitudes. Each of our analyses to come will be performed both on the annual time series, including those reconstructed, and independently on the shorter, mainly biennial series that require no use of reconstructed partisanship. The latter results, presented in Tables A.1 and A.2 in the Appendix, in every case confirm our interpretation of the former.

5. Limitations of space prevent us from presenting either the method of fitting transfer functions, the best source for which remains Box and Jenkins (1976), or the step by step modeling process of our application of the technique. Each of the models presented and discussed is a final estimation—the result of a lengthy sequence of preliminary and intermediary identifications, estimations, and diagnoses. Details of the step-by-step modeling are available in a technical appendix from the authors.

6. Our approach to causal analysis is the sometimes contentious notion of "Granger causality," that (in the simple recursive case) a series X_t may be said to (Granger) cause another series Y_t if the conditional expectation $E(Y_t|Y_t, X_t)$ produces superior predictions than an expectation based only upon the history of Y through $(t-1)$. Thus the univariate Autoregressive Integrated Moving Average

(ARIMA) model for Alignment is the benchmark against which we judge predictive improvement. The residual mean square (the unexplained sum of squares divided by the degrees of freedom) is the criterion. See Freeman (1983) for a lucid far more comprehensive treatment of Granger causality in the context of international political economy.

7. On notation and terminology: Both "regular" transfer functions of the genre proposed by Box and Jenkins (1976), where one dynamic series exerts transfer causality on another, and the more common Box-Tiao (1975) intervention models, where the independent series is a dummy variable, are properly referred to as "transfer functions." But there are important differences in the identification and diagnosis of the two. Henceforth we shall use "transfer function" to mean regular transfer function and "intervention" to refer to the Box-Tiao, interrupted time series (Campbell and Stanley, 1963), or impact (McCleary and Hay, 1980) models.

8. These are only the most visible discontinuities in the series, and such evidence can be only suggestive of the proper lag structure. More generally, the model specifies that every change in ΔS is followed by a response in ΔAL four years later. When the changes take on a more continuous and subtle nature later in the series, connections become much more difficult to see, but the evidence suggests they are no less present.

9. This and other diagnostic evidence from the transfer function identifications and estimations are available in a technical appendix from the authors.

10. Either Senate or House series alone is a significant predictor of Clarity. But the small number of observations and the collinearity of the two series prevent a statistically reliable estimation of their joint effect. A similar analysis, not reported, shows mediated but not direct linkages between the elite series and Affect.

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WAR,
ACCOMMODATION,
AND VIOLENCE
IN THE
UNITED STATES,
1890-1970

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War, postwar demobilization, and economic depression are national crises that ultimately test the state's capacity to respond simultaneously to internal and external challenges. This analysis probes the nexus between crises and domestic violence, investigating how this relationship is mediated by the influence of two variables: the severity of crisis and the presence or absence of government accommodation. Box-Tiao impact assessment models are used to estimate the separate and combined effects of American involvements in wars (the Spanish-American War, World Wars I and II, and the Korean and Vietnam Wars), their postwar periods, and the 1930s depression on economic, social, and political forms of American violence from 1890 to 1970. After establishing historical evidence for the role of national accommodation, I demonstrate that strong, positive associations between severe crises and domestic violence are to be found during the tenure of nonaccommodating administrations. Accommodating governments are associated with either negative or historically weak linkages between severe crises and domestic violence. Overall, the evidence underscores the benefit of using broad theoretical perspectives for understanding the linkages between international and domestic conflict.

Students of political development have always recognized the contribution of environmental crises to the evolution of the state.¹ Crises are disruptive, episodic events challenging and promoting a state's capacity to respond to new problems. Their solutions often lead to the establishment of new institutional structures, powers, and precedents. The more extreme forms, war and major economic depression, are frequently represented as critical turning points in the state-building

process. They are accused of undermining the established rules of resource distribution, altering the relationships between political elites and domestic contenders, and forcing state officials to take on new responsibilities in the face of class tensions and greater social complexity (Moore, 1966; Paige, 1975; Skocpol, 1979; Tilly, 1975; Trimberger, 1978; Wolf, 1969). In short, they have important cross-cutting effects by simultaneously magnifying the capacities of the state and escalating the risks of large-scale internal conflict.

I have analyzed the three-sided relationship between national crises (war, postwar demobilization, and the 1930s depression), state accommodation, and the level of internal violence for the United States from 1890 to 1970. I contend that the observed linkages between crises and domestic conflict depend upon the position of governing elites vis-a-vis the claims advanced by internal political groups. On the one hand, governments willing to accommodate the demands of domestic political contenders generate weak associations between national crises and levels of internal violence. On the other hand, nonaccommodating governments, unconvinced of the potential merits of reforms, are more likely to rely on their coercive resources to suppress domestic challenges. In that event, the linkages between national crises and domestic conflict are expected to be strong and positively associated.

The hypotheses support the view that domestic violence is the byproduct of a sequence of actions and reactions between state officials and dominant and non-dominant political groups. Since government officials have been cited as the primary agents of civil conflict, it follows that their behavior is a crucial intervening variable in the overall relationship between crises and domestic violence. Therefore, the theoretical task is to find out how state officials respond to internal challenges during difficult periods of national management, and to what extent their actions contribute to reducing or escalating internal violence. A brief review of the pertinent literature on the linkages between foreign and domestic conflicts will be discussed initially, in order to set up the subsequent theoretical and empirical analyses.

Previous Empirical Assessments

After reviewing 30 studies on the linkage between foreign and domestic

conflict, Stohl (1980, pp. 312-13) finds only 5—Denton (1966), Denton and Phillips (1968), Flanigan and Fogelman (1970), Sorokin (1937), and Tanter (1969)—that systematically assess the specific relationship between involvements in war and large-scale internal violence.² Based solely on visual impressions of longitudinal data, 3 of these studies—Denton (1966), Denton and Phillips (1968), and Tanter (1969)—assert there is a linkage; the remaining 2—Flanigan and Fogelman (1970) and Sorokin (1937)—declare there is none. To complicate matters further, these studies do not overlap in their choice of samples, data, time periods, and temporal units of analysis. Together they present an excellent snapshot of the larger maze of empirical research in this area.

Theoretically, these five studies have been criticized for specifying overly simple relationships without regard for the role of intervening variables.³ The strategy of reducing the question to in-group/outgroup forms of conflict has made it all too easy to ignore factors such as political integration, repression, regime structure, and internal changes in the distribution of power.

Another problem is the implicit assumption that the theoretical linkages between external and internal conflict will always be positive, when there may be bi-directional relationships or "on-and-off" forms of associations. Despite the presence of demonstrable evidence for this notion, analysts have tended to either ignore it or construe it as an indication of no genuine relationship. Such reasoning, of course, points out the lack of consideration for intervening influences and the weak specification of previous theoretical frameworks.

The most sophisticated illustration of this flaw is Stohl's (1975, 1976) quasi-experimental analysis of American domestic violence between 1890 and 1970. As the most systematic empirical investigation of the effects of war on

domestic violence, Stohl's studies eschew both the ingroup/outgroup propositions and the simple bivariate approaches of other analyses. Nevertheless, they postulate principles in favor of consistent unidirectional relationships. Specifically, Stohl hypothesizes that U.S. war involvements (the Spanish-American War, the two world wars, and the Korean and Vietnam wars) have influenced the mobilization of new domestic groups by enhancing their economic and social positions in American society. This mobilization in turn stimulates general demands for a redistribution of power and resources between lower and upper strata, and generates an escalation of domestic conflict.

Stohl's two studies depart from previous research by quantifying the frequency, duration, and intensity of conflictual events involving 20 or more persons, as listed in the *New York Times* and *New York Times Index*. A total of 2,861 domestic violent events have been identified for the years 1890-1906, 1913-1923, and 1935-1970. Using an interrupted time-series design and an ordinary least squares estimation, Stohl investigates whether nonrandom changes occur in these events at the start, during, and after U.S. war involvements. He finds step-level changes in civil violence both at the start of all five wars and in the postwar periods. However, no uniform pattern (in terms of statistical significance or positive and negative effects) holds for any of the five wars or for any of the dimensions of violence. Despite the unsupportive outcome, Stohl concludes the data generally support the contention that war escalates domestic political violence.

In contrast, I argue that the linkage between war (as well as other crises) and domestic violence is likely to be inconsistent. Whether it is positive, negative, or nonexistent ultimately hinges on government elites. As repositories of tremendous economic and political power, they have

the capacity to influence the course and direction of domestic violence. Although wars can result in mobilizing new and old domestic contenders into the political process, this does not automatically have to end in violence. Elites can ignore or appease these groups in a variety of ways—in particular, compromise, reform, imprisonment, or execution, all of which may stimulate or diffuse domestic conflict. Even in cases where state officials are weak, their ineffective responses will have a substantial impact on civil conflict. Therefore, the following hypotheses will inextricably tie the behavior of state officials into the theoretical relationships between crises and domestic conflict.

Hypotheses

The Role of the State as an Intervening Variable

If war is classified as one form of national crisis, like postwar demobilization and depression, then the general question centers on how crises influence the political context in which government elites view internal challenges. Does the appearance of a crisis facilitate the development of flexible political environments wherein government elites make concessions to subordinate domestic groups? or does it sustain environments in which governments are determined to pursue status quo policies, regardless of public demands? Within either of these possible outcomes, the linkages between crisis and internal violence can be either positive, negative, or nonexistent.

For instance, crises inducing internal reforms may subsequently preempt or nullify internal violence. According to Block's (1977, 1981) and Skocpol's (1979, 1980) analyses of structural change in political systems, wars, postwar demobilization periods, and major depressions free governing elites from the typical constraints of dominant political groups.

Since they have a fundamental role in maintaining order and political peace, which crises jeopardize, state officials are apt to grant concessions, even though they may be at the expense of dominant groups. Such actions reflect the government's primary interest in extracting economic resources, recruiting military personnel, and diffusing potential violence during a difficult period of national management.

National crises can also transform political environments by producing elites who are willing to support reforms because of their motives to obtain elected office or other forms of domestic support. The Great Depression's effect on electoral realignment in the 1930s is a useful example (Ladd and Haley, 1978). It shows how the appearance of sympathetic elites in the White House, Congress, and the Democratic Party contributed to reforms in social security, labor, and farm policies.

In either event, the development of accommodating political regimes may account in one instance for negative or nonexistent linkages between crises and domestic violence. On the other hand, such regimes can be equally responsible for positive linkages if new political and social rights derived from recent concessions catalyze the mobilization of large numbers of people and economic resources into political organizations. With a new or renewed sense of efficacy and the perception that the government favors their claims, these groups may challenge other political elites not willing to embrace them (Tilly, 1978, pp. 145-46). Meanwhile, sympathetic officials, reluctant to take punitive measures against these emerging constituencies, are likely to resist the demands of dominant groups seeking repression. If the effect leads to direct confrontations between the contenders, the probability of conflict will increase and result in a positive association between crisis and domestic violence.

Since the internal effects of political

reforms are difficult to foresee, governments may be conservative about undertaking major internal reforms when their positions in the international system are threatened. Instead, they may take repressive measures (e.g., deportations, arrests, censorship, and restrictions on public assemblies) against the leadership of opposition groups posing potential sources of challenge and instability. If the repression is consistently and effectively applied, officials will be able to wear down the position of domestic groups by making the costs of engaging in collective action too high (Tilly, 1978, pp. 101-6). In this case, the intervening effects of a non-accommodating political environment can account for a negative or nonexistent relationship between crisis and internal violence.

However, if the government's coercive sanctions are weak and poorly directed, the effect may be one of intensifying antagonism and conflict among groups that fail to be inhibited by threats of retaliation (Gurr, 1970, pp. 239). The act of committing military forces abroad may weaken the government's capacity to suppress internal challenges. These circumstances could become revolutionary if domestic groups continued to press their claims by seeking the overthrow of the established order (Tilly, 1978, pp. 209-11).

Finally, circumstances may contribute to the rise of conservative policies after a time of political and economic reform; this is typical of postwar demobilization periods. During the war years, governments are able to expand the resources and powers of the state, and in the interest of preserving domestic peace may extend new economic resources and political concessions to internal groups. Yet during the postwar recovery period, those resources may contract—particularly if there is a recession—leaving officials with the responsibility of reconciling the commitments made to old and new domestic

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groups. In that event, they are faced with two choices: (1) increase the state's coercion against segments of the population in order to maximize the yield of resources for reallocation, or (2) break the commitments that will incite the least dangerous opposition. Either step is likely to lead to a defensive mobilization by internal groups and a subsequent escalation of domestic conflict (Tilly, 1978, p. 207). Although this generalization could apply to cases of victory or defeat, military defeats are apt to represent the worst cases of contraction. Moreover, this situation could describe major periods of economic depression.

All of these scenarios illustrate how domestic political regimes mediate the effects of national crises on the series of actions leading to internal violence. By understanding how crises act upon the political contexts and perceptions of governing elites, one can isolate the political processes that account for the levels of domestic violence, but the array of possible linkages complicates the problem of generalizing the directional relationships between war (or any other crisis) and domestic violence. It is difficult to make propositions when the variation in the intervening variable—namely, the presence or absence of government accommodation—can be responsible for the same empirical outcomes.

An alternative approach is to start with the presumption that crises stimulate the mobilization of domestic political contenders who decide to press their claims at a propitious moment. Since the claims advanced by these contenders are likely to be at odds with at least one other domestic group (not excluding government actors), the preconditions for internal violence are not only established but intensified in comparison with noncrisis periods. As central actors in the political system, state officials play a crucial role both in resolving these claims and in countering the effects of domestic insta-

bility. Whether they decide to repress these challenges through the coercive capacities of the state or to make concessions through government-sponsored reforms, the consequences are likely to result in some levels of domestic violence. The fundamental issue is the degree of severity. In other words, under what circumstances do crises generate levels of domestic violence atypical of previous noncrisis periods?

Nonaccommodating political regimes, it has been contended, are most liable to be associated with these kinds of severity, because of their greater propensity to rely on repression, which has been linked empirically with escalating civil conflict (Snyder and Tilly, 1972; Stohl, 1976). In contrast, reform-oriented regimes are less likely candidates, because they have a better chance of diffusing what could be serious quantities of internal violence by creating a political climate that disfavors attacks on domestic challengers. The actions of governing elites and the political context influencing the flexibility and range of potential policy responses are the keys to understanding the linkages between crisis and internal violence.

Therefore, the explicit hypotheses under investigation are formalized in the following statements:

1. The presence of an accommodating national administration will account for weak, insignificant linkages between crises and internal violence.
2. Given a context of nonaccommodation, there will be strong, substantial linkages between crises and internal violence.
3. Crises that sustain nonaccommodating political administrations will be associated with higher levels of repression than crises that induce reform-oriented regimes.

Given the variation in crisis attributes, it

is impractical to assume all crises will have similar impacts on internal violence. However, the lack of available data and the inherent constraints in this kind of a study made it infeasible to isolate these effects and the role of accommodation at the same time.

Nevertheless, the additional influence of crisis intensity can be addressed in a roundabout way by distinguishing American war involvements as either *intensive* or *less intensive* affairs. *Intensive* wars are fought on a larger scale and result in the major expansion of new domestic institutions and state powers. Linkages between external and internal conflicts are expected to be strongest for these wars, because the foreign and domestic agendas will bring governing elites into more serious opposition with domestic groups over basic aspects of the economic, political, and social arrangements of their society. On the other hand, *less intensive* wars present fewer problems of domestic mobilization for governments; hence, they are expected to produce, at the most, weak conflict linkages.

If the two types of wars generate similar linkages, crisis intensity will no longer be an important conditional variable, and the role of accommodation becomes the central explanatory variable. In the event the less intensive wars engender consistently weak (or nonexistent) conflict linkages and the intensive wars yield mixed linkages, the subsequent analysis will concentrate on connecting the presence or absence of accommodation with the observed linkages of the intensive wars. Thus, crisis intensity will be held constant as the variation in the intervening variable is left to account for the fluctuations in the conflict linkages.

Operationalizing national accommodation is a relatively more difficult task in lieu of obvious quantitative measures. Alternatively, the appropriate historical records will be gleaned for evidence of reform activity undertaken by the execu-

tive, judicial, and legislative institutions of the U.S. government. Substantive reforms will indicate an accommodating national administration; their absence will denote nonaccommodation. Ultimately, the findings will comprise two components: (1) a qualitative analysis of national accommodation during the relevant U.S. political administrations, and (2) a quantitative assessment of the effects of war, postwar demobilization, and the 1930s depression on American domestic violence from 1890 to 1970.

Global versus Interstate Wars

Although identifying the most intensive wars is not a clear-cut process, one would expect them to be the most extensive—in terms of the geography encompassed and the number of major actors actively participating—and the most costly in terms of both lives and money. Furthermore, they would have the greatest historical consequences for internal changes in national expenditures, revenues, debt levels, the number of governmental agencies and personnel, and the general involvement of state intervention in everyday life. Recent studies consistently demonstrate an empirical distinction between interstate wars (the Anglo-American, Mexican, Spanish-American, Korean, and Vietnam wars) and global wars (World Wars I and II) regarding their short- and long-term influences on American state-building and processes of economic growth from 1792 to 1980 (Rasler and Thompson, 1983, 1985a, 1985b).⁴ Global wars have statistically significant and much stronger permanent influences than interstate wars, which have temporary negligible impacts.⁵ Other historical studies have also noted the growth in the United States' national administrative capacities, and the government's expanding role in resolving broad economic, political, and social issues during World Wars I and II and the Great Depression

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(Brody, 1980; Hawley, 1979; Skocpol, 1980; Skowronek, 1982; Wilensky, 1975). Consequently, it is hypothesized that as the most serious crises, World Wars I and II, their postwar periods, and the 1930s depression will be associated with significant conflict linkages; the interstate wars and their postwar periods will have weaker, less significant associations.

Research Design

Data and Indicators

Fortunately, the presence of data on American violence in Stohl's (1976) analysis makes the following tests possible. However, the annual domestic violence data cover the periods 1890-1906, 1913-1923, and 1935-1970. The lack of a continuous time series requires slicing the data into prewar and postwar observations without strong theoretical guidelines. The outcome can seriously compromise the validity of empirical findings. For instance, the choice of 1935-1941 as the pre-World War II period, which bypasses the violence related to labor and unemployment in the early 1930s, may result in overestimating the level of violence from the prewar to postwar years.

Meanwhile, a separate analysis of the three data sets complicates making historical comparisons over time. The central question is not only how wars influence the level of internal violence, but whether the change in the level of the violence is truly different from all other nonwar years also characterized by excessive levels. The most effective way of escaping these problems is to employ data and methodological approaches that depend on long, sustained records of violence. Since Stohl's (1976) data collection procedures are outlined clearly, the task of expanding the annual data for the periods 1907-1912 and 1924-1934 is relatively straightforward. The present

analysis will use an annual measure of the magnitude of domestic violence. The frequency of all violent events is multiplied by their duration to yield four time series under investigation: *economic*, *social*, *political*, and a combined measure of all three labelled *total violence*. Plots of these series are provided in Figures 1 and 2.

The decision to maintain Stohl's economic, social and political classifications is based on the view that conflict linkages may be contingent on the type of domestic challenge being made. As an example, the state's wartime interest in preserving economic stability for the production of war materials may lead to concessions for labor unions; at the same time, other groups considered to be unacceptable threats to the state (e.g., anarchists, International Workers of the World, and the Communist party) receive different forms of response—namely, repression. Such discrimination will inevitably affect the observed conflict linkages.

Stohl (1976) identifies economic-related violence by the types of issues (e.g., collective bargaining, wage increases, unemployment) at stake and the groups participating in the conflict (e.g., management versus labor; landlords versus tenants; farmers versus bankers; and agricultural laborers versus landowners). Social-related violence comprises racial, ethnic, religious, and educational disputes (e.g., whites versus blacks, Indians, and Orientals; native Americans versus foreign immigrants; and students or faculty versus university administrators). Finally, politically-related violence involves disagreements over governmental policies or ideological differences (e.g., Republicans versus Democrats; right- versus left-wing groups; government actors versus antiwar protestors; Communists versus Socialists; and government actors versus Communists and Socialists).

In addition to these dimensions, Stohl (1976) distinguishes among pro-system

Figure 1. U.S. Economic and Political Violence, 1890-1970

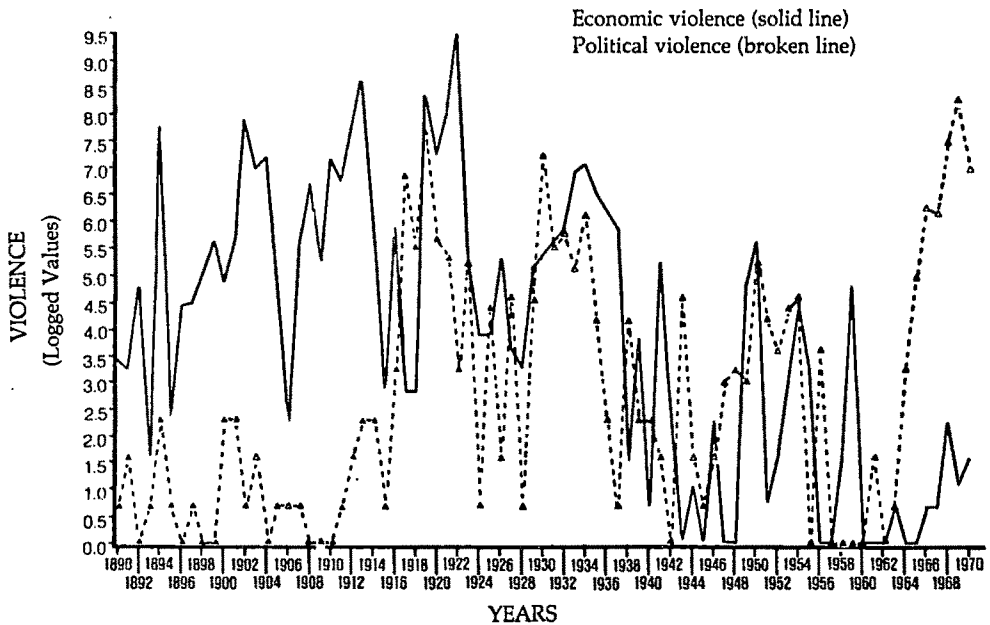
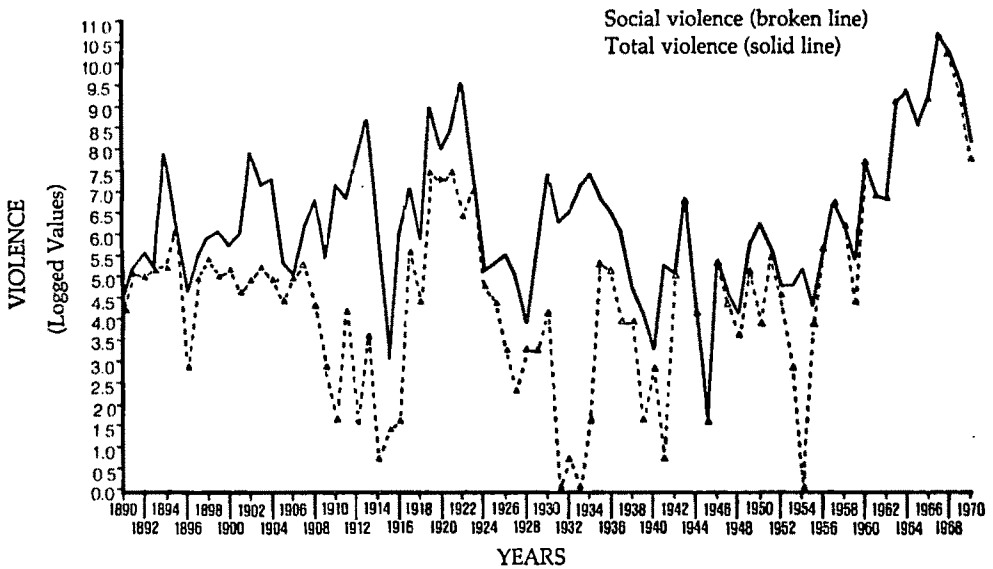


Figure 2. U.S. Social and Total Violence, 1890-1970



(events initiated by status quo groups), anti-system (events initiated by groups seeking to reform or challenge the status quo), and clash (events unidentified as either pro- or anti-system) activities. Because a large proportion of the data are identified as clashes, an extensive examination of each series is unnecessary. Nevertheless, the magnitude of pro-system violence will be used to discern whether repression (initiated by either the government or other dominant groups) is indeed higher in crises involving nonaccommodating political regimes than in crises involving conciliatory regimes.⁶

Statistical Approach

The development of Box-Tiao impact assessment models has made the utility of quasi-experimental designs a more attractive venture. They are designed to control the effects of autocorrelation and trend as the net effects of a theoretical change in the level of a time series are estimated. Moreover, they yield a direct measure of the impact of war by providing a numerical estimate of the differences between war and nonwar observations. Previous time-series studies (e.g., Stohl, 1975, 1976) have relied on indirect assessments by comparing prewar to postwar observations, or confounding the war years with either the prewar or postwar series. Therefore, Box-Tiao models will be employed to estimate the separate influences of war, postwar demobilization, and the Great Depression on domestic violence. The general Box-Tiao impact model can be summarized as

$$Y_t = f(I_t) + N_t. \quad (1)$$

The intervention component, $f(I_t)$, is represented by one or two transfer function parameters, ω_0 and δ , which respectively estimate the initial impact of an event on $Y_t(\omega_0)$ and the rate of growth or decay in the level of the time series after the impact (δ).⁷ The omega parameter (ω_0)

is an estimate of the difference between pre- and post-intervention levels of Y_t ; the delta parameter (δ), constrained between -1 and $+1$, reflects how quickly the post-intervention series reaches equilibrium. When δ approximates 1, the post-intervention series returns to equilibrium very slowly.⁸ Conversely, when δ approximates zero, the post-intervention series returns to equilibrium very quickly. The noise component (N_t) encompasses the random and deterministic effects of trend and autocorrelation. It is modelled through an ARIMA approach, which estimates a linear filter(s) that transforms the observed time series (Y_t) into white noise.

After the initial step of identification and estimation of the noise model (ARIMA structure), the intervention component is selected based on whether the impact is abrupt or gradual and permanent or temporary.⁹ The intervention parameter(s), in addition to the ARIMA parameter(s), are then estimated in a full model through iterative, nonlinear estimation procedures (Box and Jenkins, 1976; Box and Tiao, 1975).¹⁰ Any nonstatistically significant parameters are removed before a final model is reestimated. Eventually, the goodness-of-fit of the model is judged by the degree of correspondence between the observed and predicted output series values, the residual mean square error, the statistical significance of the model parameters, and the "white noise," or random character of the residuals.

Specification of the Interventions

The impact of U.S. war involvements on domestic violence will be estimated for each violence time series in the form of three interventions: one encompassing the combined effects of the Spanish-American, Korean, and Vietnam wars, and a second and third reflecting the separate influences of World Wars I and

Table 1. The Impact of War on U.S. Domestic Violence, 1890-1970

Violence Series	Intervention Component ^a			Noise Component			
	Interstate Wars ω_0	World War I ω_0 δ	World War II ω_0	ARIMA Parameters	Type of Noise Structure ^b	Goodness-of-Fit ^c χ^2	RMSE
Economic	.19 (1.1)	-3.7* (2.6)	-.69 (.66)	$\theta_1 = .76^*$ (9.5)	(0,1,1)	16.1	2.9
Social	-.14 (1.1)	2.1* (2.4)	.29 (.56)	$\theta_1 = .49^*$ (4.4)	(0,1,1)	13.8	2.8
Political	.51 (1.5)	4.1* (2.7)	-1.1* (3.6)	$\phi_1 = -.51^*$ (5.1)	(1,1,0)	9.4	3.1
Total	-.15 (.35)	-.07 (.87)	-.88 (1.2)	white noise	(0,1,0)	19.3	1.9

Note: All data have been logged to achieve variance stationarity; *t*-values are reported in parentheses; an asterisk indicates statistical significance at .05 alpha level.

^aAll intervention models are abrupt, temporary with exception of abrupt, or permanent models for economic violence during World War I.

^b(0,1,1) denotes differenced, first-order moving average process;

(1,1,0) denotes differenced, first-order autoregressive process;

(0,1,0) denotes differenced process.

^cChi-square values indicate residuals are white noise with 11 degrees of freedom; RMSE represents residual mean square error.

II.¹¹ The influences of the interstate wars are combined, because of the expectation they will not have strong statistically significant associations with internal violence, either separately or jointly.¹² In those cases where the parameter estimates indicate otherwise, the separate influences of each war will be calculated; the same approach will be used to estimate the postwar effects. Each postwar demobilization period will be equal to the number of years involved in actual warfare.¹³

Meanwhile, a separate estimation of the Great Depression's influence will be made for each of the four time series. An examination of the annual expansion and contraction periods of U.S. gross domestic product from 1886 to 1980 shows the years 1930-1933 to be associated with one of the worst declines in the U.S. national economy (The Economist, 1982, p. 24). Hence, these years will be used for specifying the intervention effects of the domestic conflict series.

Data Analysis

The Impact of War on Domestic Violence

Table 1 summarizes the statistical outcomes of applying Box-Tiao impact assessment models to the four continuous series of domestic violence. The collective impacts of the interstate wars are expected to yield statistically insignificant impact parameters, although the two global wars will be associated with statistically significant estimates. Considering that some levels of violence occur in any political system, statistical significance is an important criteria for identifying historical deviations from the norm. Even though the short term consequences of wartime influences on internal violence are important, primary interest is whether these influences are responsible for unusual levels of severity in comparison to nonwar years. The results shown in Table 1 indicate that the only significant impacts of war are associated with World War I.

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With the exception of economic-related violence, the direction of these impacts shows positive increases for the remaining forms of violence.

My initial strategy was to isolate the effects of global and interstate wars on internal violence. Mixed results, if any, between the global wars were to be accounted for through the intervening effects of government accommodation. Since the evidence does reflect contrasting conflict linkages between World Wars I and II, below I will interpret parameter estimates for economic, social, and political violence on the basis of the wartime policies pursued by national elites. As an aggregate measure, the total violence series will yield parameter estimates that either support the overall findings of the other violence series or show, as in Table 1, how aggregation can distort important underlying relationships. Any further analysis of the series is unnecessary.

Economic Violence. Ordinarily, the omega (ω_0) estimates in Table 1 express the immediate impact in terms of the numerical units of the dependent variable. Where the data have been logged, the numerical estimates are converted to percentages to reflect the change in the level of a series.¹⁴ In light of the log transformations herein, the impacts of World Wars I and II are accompanied by a 98% and 50% decline, respectively, in economic-related violence. Although World War I is associated with the only statistically significant decline, the findings suggest that economic violence—a large proportion of which derives from labor-management disputes—did not increase at a time when national officials would have been most vulnerable. According to labor historians, the two world war experiences offered extraordinary opportunities for American labor leaders—who were normally locked out of the political process—to gain access to the inner circles of national decision making.

During the world wars, the Wilson and Roosevelt administrations made a concerted effort to enlist labor's cooperation to smooth and sustain the process of economic mobilization. Their policies led not only to major reforms on behalf of labor, but also to the sponsorship of labor participation in the national government.

Both Wilson and Roosevelt established a National War Labor Board (NWLB) involving the participation of prominent labor officials, for the purpose of formulating a comprehensive set of rules to guide labor-management relations during the war. Many of the new principles involved significant labor reforms. During World War I these included the recognition of workers' rights to join independent trade unions, the preservation of the union shop and union conditions where they already existed, an eight-hour workday, and equal pay for women (Dubofsky, 1975, p. 122). During World War II, they disposed of any remaining barriers to collective bargaining, and extended labor-management issues beyond wage topics to include fringe benefits, wage-rate inequalities, piece rate computation, and geographical differentials (Brody, 1980, pp. 112-16). Meanwhile, the A.F.L. (in World War I) and the C.I.O. (in World War II) agreed to no-strike policies, actively squashed strikes, and aided the government in purging disloyal, subversive elements—members of the International Workers of the World (IWW) and of the Socialist and Communist parties—from the labor movement.¹⁵

Although the war years were characterized by greater government flexibility regarding labor issues, they did not avoid a substantial rise in labor strikes. Between 1914 and 1916 an average of 2,195 work stoppages occurred, in comparison with 4,450 in 1917 and 3,353 in 1918 (U.S. Bureau of the Census, 1975, p. 179). Yet empirical findings indicate economic violence declined significantly during World War I. This phenomenon can be

attributed partially to the efforts of federal mediation in labor-management disputes, the influence of A.F.L. officials in persuading workers to return to their jobs, and, in cases where these options failed, government repression of union leadership—for example, in the IWW-led strikes in copper, lumber, and food grains (Dubofsky, 1975, pp. 115-21).

The pattern is similar in World War II, though less dramatic. The average number of work stoppages in the four years preceding the war (1938-1941) increased from 3,045 to an average of 4,107 during the war years (1942-1945) (U.S. Bureau of the Census, 1975, pp. 179). Nevertheless, there is an empirical decline, albeit weak, in economic violence. Again, the harmonious labor-management relations fostered by the government's support of collective bargaining is probably a crucial factor. In the final analysis, the political contexts of World Wars I and II, in addition to the quantitative evidence, contribute support for the hypothesis that accommodating political regimes will contribute to weak or nonexistent conflict linkages.

Social Violence. The empirical evidence in Table 1 indicates that World War I is associated with a statistically significant (717%) increase in social violence; World War II is affiliated with a statistically weak (34%) increase. Bearing in mind that 95% of the social violence events for the 1890-1970 era involve racial violence, the role of national accommodation will be analyzed on the basis of the Wilson and Roosevelt administrations' wartime record on black civil rights issues.¹⁶

During World War I, the Wilson administration failed to undertake significant reforms on behalf of blacks. It ignored basic demands by blacks for the elimination of military desegregation, federal employment discrimination, and Jim Crow practices in the federally controlled railroad system (Scheiber and

Scheiber, 1969, pp. 454-57). Prewar discriminatory practices by employers and white union officials were sustained during the war (with the approval of the federal government), and black workers had little or no opportunity to advance from unskilled to semiskilled occupations (Foner, 1982, pp. 133-34). An examination of the U.S. Supreme Court civil rights decisions from 1876 to 1955 reveals a significant decline both in the volume of cases and the number of pro-civil rights outcomes between 1916 and 1920 (McAdam, 1982, p. 85). Needless to say, national political elites were far from accommodating the political and social interests of blacks in American society.

The concomitant rise in social violence during this period is attributed in large part to the conflict between blacks and whites over urban issues of housing, transportation, recreational facilities, and the competition for jobs and wages. Despite pleas from black leaders, Wilson refused to take public positions denouncing the practice of lynchings and the failure of local authorities to protect black citizens (Scheiber and Scheiber, 1969, pp. 455-57).

The Roosevelt administration, on the other hand, undertook major reforms during World War II to accommodate the interests of blacks. In 1943, the War Labor Board outlawed wage differentials based on race; the U.S. Employment Service reversed existing policy by refusing to honor requests specifying the race of applicants; and the National Labor Relations Board stated it would refuse to certify unions excluding minority groups. By the end of the war, the federal government had increased the number of its black employees from 60,000 to 200,000, and more often in higher classifications (Polenberg, 1972, p. 116). Finally, the volume of U.S. Supreme Court decisions on civil rights cases—many of which were aimed at striking down Jim Crow practices—and the number of favorable out-

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comes increased from 1941 to 1945 (McAdam, 1982, p. 85). In short, the political accommodation during World War II is associated with a statistically weak conflict linkage; the lack of accommodation in World War I is linked with a statistically strong conflict linkage.

Political Violence. The empirical evidence indicates that World War I is associated with a strong (603%) increase in political violence, and that World War II has a weak negative (9% decline) association. In the case of World War I, there is a noticeable unstable effect on political violence. The Box-Tiao results in Table 1 show statistically significant omega (ω_0) and delta (δ) parameters. Ordinarily, when values equal or exceed 1.0, the delta parameter is dropped from the equation and then the omega parameter is estimated alone. However, this procedure results in a weaker and statistically insignificant omega estimate.¹⁷

An examination of Figure 1, which plots the political violence series from 1890 to 1970, reveals that the substantial increase in 1917 is followed by a subsequent decline in 1918. This contributes to the unusually large, negative δ , and to a statistically insignificant estimate of ω_0 without the presence of the δ parameter. Nevertheless, the magnitude of political violence in 1918 is considerably higher than any of the prewar observations from 1890 to 1916. Therefore, despite the appearance of misspecification, the earlier estimates are maintained as the appropriate findings, and more than likely reflect the problem of a short intervention series that has failed to establish equilibrium (McCleary and Hay, 1980, p. 159).

Much of the political violence between 1917 and 1918 is the result of actions taken by government agencies and other status quo groups against leftists, farmers, and pacifists protesting the war and selective service laws (Peterson and Fite, 1975; Stohl, 1976, pp. 91-95). Members of the

Socialist Party and the International Workers of the World were prominent targets, since their organizations took strong public stands against the war. Their rallies, parades, and assemblies often precipitated attacks at local levels. The IWW was an especially popular object of repression by local and national authorities, who used its antiwar position as a justification for eliminating its disruptive labor union operations (Peterson and Fite, 1975, pp. 48-60).

The 9% decline in political violence during the World War II years is attributed to the impact of Pearl Harbor on public consensus, the absence of major violence in interning the Japanese-American population, and to the presence of the Soviet Union as a major American ally, which helped to unify leftist support (Brooks, 1979, pp. 314-15). The lack of opposition to the war, in contrast to the 1917-1918 years, makes it difficult to pin down the role of accommodation for the Roosevelt administration. Since national elites were never required to respond to any large constituency, it is not possible to link the position of government officials to the weak, negative relationship between World War II and political violence.

The only other significant finding in this section is the strong positive impact of the Vietnam War on political violence. Since the estimation of the collective influences of interstate wars yielded contradictory estimates, the intervention components were respecified to calculate the separate influences of the Spanish-American, Korean, and Vietnam wars on political violence. The results indicate that the Vietnam War is accompanied by a statistically significant (949%) change in the level of the series. This finding supports earlier evidence of a positive association between the escalation of U.S. troop levels and the rate of increase in the number of antiwar protests from 1964 to 1968 (Tanter, 1969, p. 557). However, the

Table 2. The Impact of Postwar Demobilization on U.S. Domestic Violence, 1890-1970

Violence Series	Intervention Component ^a				Noise Component			
	Interstate Wars ω_0	World War I ω_0	World War I δ	World War II ω_0	ARIMA Parameters	Type of Noise Structure ^b	Goodness-of-Fit ^c χ^2	RMSE
Economic	-.37 (.73)	3.9* (2.3)	-1.0* (3.6)	.58 (1.1)	$\theta_1 = .63^*$ (6.2)	(0,1,1)	17.4	4.1
Social	.78 (1.5)	3.6* (2.1)	-.97* (3.9)	.31 (.56)	$\theta_1 = .46^*$ (4.2)	(0,1,1)	12.3	2.8
Political	-.76 (1.3)	1.4* (2.0)	—	.69 (1.1)	$\phi_1 = -.45^*$ (4.1)	(1,1,0)	4.5	3.0
Total	.13 (.20)	3.2* (2.3)	-1.0* (2.5)	.22 (.30)	white noise	(0,1,0)	19.8	2.0

Note: All data have been logged to achieve variance stationarity; *t*-values are reported in parentheses; an asterisk indicates statistical significance at .05 alpha level.

^aAll intervention models are abrupt, temporary with exception of abrupt, or permanent models for economic violence during World War I.

^b(0,1,1) denotes differenced, first-order moving average process;

(1,1,0) denotes differenced, first-order autoregressive process;

(0,1,0) denotes differenced process.

^cChi-square values indicate residuals are white noise with 11 degrees of freedom; RMSE represents residual mean square error.

violence data fail to cover the total period of American war involvement, making it premature to analyze the statistical results any further.

The Postwar Impact of Demobilization on Domestic Violence

Table 2 presents the results of estimating the Box-Tiao impact assessment models for the collective and separate influences of the interstate wars and World Wars I and II on domestic violence. A preliminary overall evaluation indicates that the post-World War I period (1919-1920) is associated with statistically significant increases for all forms of domestic violence; the individual and aggregate postwar periods of World War II and the interstate (i.e., Spanish-American and Korean) wars show positive but statistically weak increases. A full interpretation of these results is reserved for the following sections.

Economic Violence. The parameter estimates in Table 2 show that the post-

World War I period (1919-1920) is accompanied by a statistically significant (603%) increase in the level of economic violence, whereas the post-World War II era (1946-1949) is linked with a statistically weak (79%) increase. The large negative value of the post-World War I δ reflects the similar unstable patterns of political violence during the war. An examination of Figure 1, which plots the logged values of the series from 1890 to 1970, reveals that the postwar impact has its strongest influence in the initial postwar year (1919). The level of violence is not sustained in 1920, but is still considerably higher than the 1917-1918 levels. The short intervention series coupled with the decline in 1920 yields another statistically insignificant omega estimate without the δ parameter.¹⁸ Therefore, the initial parameter estimates are retained.

In comparison with the major reforms on behalf of labor during World War I, the postwar period was characterized by a return to national conservatism. By

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mid-1919, labor unions lost many of the reforms gained during the war. Both the U.S. government and management no longer endorsed and maintained agreements on wage levels, the length of the workday, and codes on industrial safety. Most significantly, collective bargaining was no longer embraced by governmental officials (Dubofsky, 1975, pp. 122-31; Hawley, 1979, pp. 45-52).

Therefore, when labor-management disputes occurred between 1919 and 1920, the resulting labor strikes generated an increase in the level of economic violence. It is difficult to overlook the decline in economic violence accompanying the large number of work stoppages during World War I (\bar{x} = 3902), when a relatively equal number of work stoppages in 1919-1920 (\bar{x} = 3521) is associated with a statistically significant increase. The absence of a national government promoting and enforcing policies of collective bargaining removed an important avenue for diffusing potential violence.

The post-World War II years (1946-1949) are also characterized by a large number of work stoppages (\bar{x} = 3926); and though there is an increase in economic violence, the change is statistically weak from a historical perspective.¹⁹ Like the 1919-1920 years, the post-World War II era also saw a return to public and Congressional conservatism regarding the economic position of labor unions, federal spending, and American foreign policy abroad. Nonetheless, national administrative elites went against these trends in their efforts to regenerate the American economy and supervise the European Recovery Program.

Truman made every effort to coopt major American labor leaders in an attempt to obtain internal economic stability and enlist their aid in mobilizing domestic support for the Marshall Plan (Freeland, 1972; Godson, 1976; McClure, 1969). Stimulated by the reasoning that future

economic growth would produce abundance and material gain for all classes without a redistribution of economic power, C.I.O. and A.F.L. labor leaders endorsed the administration's policies (Hamby, 1974, p. 66; Milton, 1982, pp. 154-55). They compromised on labor-management battles designed to expand their role in management decisions, and accepted the proposition of pegging wage increases to cost-of-living and economic productivity levels (Brody, 1980, pp. 173-213). As representatives of the U.S. government, they also participated in selling the merits of the Marshall Plan to the West European trade unions. Eventually, they would succeed in splitting the European labor movement between Communist and non-Communist elements, and contribute to the development of European political institutions on a more centrist political path (Maier, 1978, pp. 40-46). According to labor historians, these actions would lead to labor's loss of political independence, and insure labor peace over the course of the next 25 years (Brody, 1980, pp. 229-55; Milton, 1982, pp. 154-67). This position is supported by the quantitative evidence in Figure 1, which shows a decrease in the overall level of economic violence from 1946 to 1970.

Social Violence. The Box-Tiao estimates in Table 2 indicate that the post-World War I era is accompanied by a statistically significant (522%) increase in the level of social violence; the post-World War II era is linked with a statistically weak (36%) increase. The political climate for blacks in 1919 and 1920 was far from favorable. Blacks in America went into the war with high morale generated by the belief that a new democratic order would be achieved on the home front. Instead, black soldiers met with discrimination and segregation, and when they came home, they faced Jim Crow and unemployment. They also found the Klu Klux Klan revitalized, a

denial of suffrage and residential segregation as before, and a gathering of racial tensions that led to the "Red Summer" of 1919. In one year, 77 blacks were lynched and 26 American cities suffered race riots (Scheiber and Scheiber, 1969, pp. 455-58).

Despite the post-World War I years, America's rise to world leadership status after World War II had unintended consequences for internal policies of racial discrimination. Locked in what was perceived to be an ideological struggle against Communism, national leaders viewed racial discrimination as a major handicap in the Cold War competition for influence over nonwhite Third World nations (Berman, 1974, p. 186; Bernstein, 1974, p. 58; Myrdal, 1970, p. 35). Furthermore, the strategic location of black voting blocs in northern states with large electoral votes made blacks an important element in Truman's bid for the presidency in 1948 (Berman, 1974, p. 186). These international and domestic realities prompted Truman to take increasingly progressive actions that led to (1) the desegregation of the armed forces, (2) the appointment of a Civil Rights Committee, and (3) the establishment of a Fair Employment Board within the Civil Service Commission. In February 1948, Truman introduced the first comprehensive civil rights program to the Congress since the post-Civil War years, and three years later he closed out his term by establishing a Committee on Government Compliance, aimed at preventing discrimination in employment by private firms holding government contracts (McAdam, 1982, pp. 83-84).

Table 2 also indicates that the influence of the interstate wars on social violence is associated with a 118% increase in level. Due to the unusually large value, the separate postwar effects for the Spanish-American and Korean wars were estimated. The only statistically significant result is the 232% increase that occurs with a one-year lag of the post-Korean

War (1955-1957) impact. The substantial rise in social violence follows the 1954 Supreme Court decision in *Brown v. Topeka Board of Education* on the issue of public school segregation. Southern state officials enacted a flood of pro-segregation legislation in reaction. When the federal government failed to enforce the Supreme Court's ruling, black political resistance gained momentum throughout the South, and eventually resulted in an escalation of violence (McAdam, 1982, pp. 144; Piven and Cloward, 1977, pp. 207-8).

Political Violence. According to the Box-Tiao estimates in Table 2, the post-World War I years are charged with a statistically significant (290%) increase in political violence. The post-World War II years, on the other hand, are associated with a statistically weak (99%) increase.

The substantial levels of post-World War I political violence are due, in large part, to the sustained concern of national administrative elites with repressing groups and individuals espousing communist, socialist or anarchist views. Public distaste for radical groups such as the Socialist party and the IWW intensified with exaggerated threats of bolshevism spreading from Europe to the U.S. Public demonstrations and assemblies by radical organizations continued to be attacked by local populations with the tacit approval of local and national officials (Peterson and Fite, 1975, pp. 285-306).

The role of national accommodation during the post-World War II years is difficult to discern closely, because, as with the war years, there is very little organized political activity protesting or making public demands for or against government policies. Although the political climate became more antagonistic toward Communists (or alleged Communists), the reactionary elements of anti-communism did not dominate public attitudes until after 1947, with the inten-

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Table 3. The Impact of the Great Depression on U.S. Domestic Violence, 1890-1970

Violence Series	Intervention Parameters ^a		ARIMA Parameters ^b	Type of Noise Structure ^c	Goodness-of-Fit ^d	
	ω_0	δ			χ^2	RMSE
Economic	1.2* (2.0)	.79* (4.7)	$\theta_1 = .75^*$ (9.3)	(0,1,1)	12.8	3.9
Social	-3.7* (3.5)		$\theta_1 = .39^*$ (3.5)	(0,1,1)	10.2	2.5
Political	.33 (.51)		$\phi_1 = -.46^*$ (4.9)	(1,1,0)	7.6	3.1
Total	.35 (.49)		white noise	(0,1,0)	19.3	1.9

Note: All data have been logged to achieve variance stationarity; *t*-values are reported in parentheses; an asterisk indicates statistical significance at .05 alpha level.

^aThe intervention model is gradual, permanent for economic violence; abrupt, permanent for social violence; and abrupt, temporary for political and total violence.

^bThe impact is lagged one year for social violence.

^c(0,1,1) denotes differenced, first-order moving average process;

(1,1,0) denotes differenced, first-order autoregressive process;

(0,1,0) denotes differenced process.

^dChi-square values indicate residuals are white noise with 12 degrees of freedom. RMSE represents residual mean square error.

sification of the Cold War. Truman's national security concerns vis-a-vis the Soviet Union shaped domestic priorities toward a preoccupation with radical subversion, both abroad and at home. The federal employee loyalty program and the trials of American Communists during these years fostered rigid conservatism among national elites and contributed to the rise of Joseph McCarthy in the 1950's (Theoharis, 1970, pp. 197-232). Nevertheless, there is no sustained level of organized political activity similar to the Socialists and Wobblies (IWW) during and after World War I. Consequently, national accommodation cannot be linked in any obvious way with the empirical findings.

The Impact of the Great Depression on Domestic Violence

Economic Violence. Table 3 reveals that the depression years (1930-1933) are ac-

companied by a statistically significant increase in the level of economic violence. The first year (1930) is linked with an initial change (232%) in violence that continues to escalate through 1933.

Much of the economic violence in the early years (1930-1932) of the depression involved the resistance of industrial workers to substantial wage cuts and substandard working conditions. Meanwhile, the Hoover administration opposed all federal proposals dealing with the problems of unemployment (e.g., public works programs, unemployment insurance, federal relief policies) and labor reform. The only labor legislation Hoover signed, with obvious reluctance, was the 1932 Norris-LaGuardia Act designed to prevent federal officials from issuing injunctions prohibiting labor strikes, union membership, picketing, and other actions restricting labor activity. The source of the bill came from Congress, and Hoover, and his administration

worked secretly, though unsuccessfully, to block its passage (Bernstein, 1960, pp. 397-415, 505-8).

However, the situation changed in 1933, when Roosevelt enacted Section 7a of the National Industrial Recovery Act (NIRA), endorsing labor's right to organize and bargain collectively with management over issues of wages, working hours, and industrial safety codes. The unanticipated consequence was a significant rise in economic violence, owing primarily to employer resistance to collective bargaining. In the final analysis, the rise in economic violence from 1930 to 1932 is associated with a nonaccommodating political administration, and the sustained escalation of violence in 1933 is linked with a reform-oriented administration. Although the level of the 1933 violence does not support theoretical expectations, it can nevertheless be attributed to the conservative positions of business and corporate elites who, because they believed the NIRA to be unconstitutional, decided not to comply (Bernstein, 1970, pp. 172-216; Milton, 1982, p. 31). Between 1934 and 1941, 51% of the work stoppages were over the issue of union recognition; by the end of World War II (1942-1945), the average was down to 21%.²⁰ There was an obvious time lag between the federal government's endorsement and its acceptance by non-governmental elites, who did not fully embrace the concept until the procedures of collective bargaining were formalized via the National War Labor Board during World War II (Brody, 1980, pp. 173-214). Therefore, the rise of economic violence in 1933 can still be linked with a nonaccommodating political environment, despite the position of the federal government.

Social Violence. The Box-Tiao estimates in Table 3 indicate a statistically significant (98%) decline in social violence from 1931 to 1933. This is an unexpected result, considering the lack of action by the

Hoover administration to alleviate the plight of blacks during the depression. Although blacks received limited political and economic gains from the Roosevelt government, the absence of violence in 1933 is not sufficiently different from the state of affairs in 1930-1932 to attribute it to national accommodation.²¹

More likely, the decline is attributable to the participation of blacks with whites in Communist-organized hunger marches, public demonstrations, and labor strikes—many of which ended in violence (Foner, 1982, pp. 188-203; Piven and Cloward, 1977, pp. 48-91). Therefore, much of the violence involving blacks during these years took place within the contexts of labor-management disputes (reflected in economic violence) and political opposition between governmental officials and the unemployed public (reflected in political violence).

Political Violence. The statistical estimates in Table 3 reflect that political violence shows a statistically weak (39%) increase from 1930 to 1933. A substantial amount of this violence is the result of the mobilization of large numbers of people, primarily unemployed, who demonstrated and clashed with state and national authorities, protesting the lack of jobs and public assistance. Largely organized by the Communist party and other radicals, the public engaged in massive hunger marches, resisted tenant evictions, and stormed government offices (Piven and Cloward, 1977, pp. 49-60). Although there was very little accommodation by the Hoover administration during the first three years of the depression (1930-1932), the level of political violence does not reach a critical threshold.

Linkages Between Repression and National Accommodation

The last hypothesis in this study associates nonaccommodating political

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Table 4. A Comparison of the Average Magnitude of Pro-System Initiated Violence during Crisis and Noncrisis Years

Noncrisis Years	Pro-System Violence	Crisis Years	Pro-System Violence
1900-1909	6	1917-1918 ^a	183
1910-1916	29	1919-1920 ^a	1215
1921-1929	54	1930-1932 ^a	134
1934-1939	143	1933 ^b	168
1940-1941	23	1942-1945 ^b	89
1950-1959	35	1946-1949 ^b	58

^aDuring this period there was no national accommodation toward major economic, social, and political groups (with the exception of reforms on behalf of labor during 1917-1918).

^bNational accommodation toward major economic and social groups occurred during this period.

administrations with higher levels of repression than reform-oriented regimes. Nonaccommodating governments are more likely to use coercion in order to suppress domestic challenges, thus escalating the level of internal violence. Reform governments, on the other hand, are less likely to rely on repressive measures, thereby reducing potential levels of violence. A test of the hypothesis is based on a comparison between the levels of violence initiated by pro-system groups during World Wars I and II and their postwar periods, and during the Hoover (1930-1932) and Roosevelt (1933) administrations during the Great Depression. Unfortunately, the outcome will not be conclusive, inasmuch as the pro-system violence includes the actions of both governmental elites and dominant political elites. However, if nongoverning elites take cues from the behavior of national officials regarding appropriate and inappropriate responses to new contenders, there is apt to be a correlation among their actions, despite the obvious exceptions (e.g., the response of corporate elites to Section 7a of the NIRA in 1933, and the reaction of southern officials to the Supreme Court decision on public school segregation in 1954). Therefore, pro-system violence is still a useful

measure for comparing levels of repression among U.S. administrations.

Table 4 tabulates the average magnitude of pro-system violence for each decade from 1900 to 1959 (including the crisis years) in comparison with the average magnitude of pro-system violence for each crisis period. The evidence indicates two things. First, the overall levels of repression are higher during crisis years than in noncrisis periods, regardless of accommodation. This is not unexpected, in view of the greater insecurity among governing and nongoverning elites during critical periods.

Second, a rank order of the repression during the crisis periods shows that two of the three nonaccommodating regimes (1917-1918 and 1919-1920) are associated with the highest levels of repression; two of the three conciliatory regimes (1942-1945 and 1946-1949) are linked with the least amount of repression. The Hoover and the Roosevelt administrations present the anomalies. The data suggest that the Roosevelt administration's first year, though more accommodating than Hoover's, was associated with more repression. However, this result is more the consequence of the activity of business elites, who refused to recognize collective bargaining, than of governmental action.

Table 5. The Association of U.S. National Accommodation and the Linkages between Crisis and Internal Violence

Type of Contenders	Accommodation		Nonaccommodation	
	Crisis	Conflict Linkage ^a	Crisis	Conflict Linkage
Economic	World War I	Strong, negative	Post-World War I Depression ^b	Strong, positive
	World War II	Weak, negative		Strong, positive
	Post-World War II	Weak, positive		
Social	World War II	Weak, positive	World War I	Strong, positive
	Post-World War II	Weak, positive	Post-World War I Depression ^c	Strong, positive
				Strong, negative
Political	World War II	no test	World War I	Strong, positive
	Post-World War II	no test	Post-World War I Depression	Strong, positive
				Weak, positive

Note: U.S. administrations that implement major reforms on behalf of nondominant economic, social, and political groups are labelled as accommodating; otherwise, administrations are considered non-accommodating.

^aBased on Box-Tiao parameter estimates indicating statistical significance (strong/weak) and the directional impact of war, postwar demobilization, and the Great Depression on economic, social, and political forms of violence.

^bNonaccommodation during 1933 reflects the resistance of dominant corporate and business elites to Roosevelt's policy on union recognition.

^cNonaccommodation during 1933 reflects the resistance of local elites to Roosevelt's policies of equal distribution of public welfare aid for blacks and whites.

The real question is why the level of repression is lower than one would typically surmise for the Hoover years. Perhaps public sympathy for the plight of the unemployed and homeless made the use of repression a less palatable option for everyone. If that is the case, then the public's approval of the claims advanced by a particular segment of the population can go a long way toward influencing the acceptable parameters of repression.

Conclusion

This study shows that an overemphasis on finding positive statistical associations between three types of crisis (war, postwar demobilization, and depression) and internal violence can overlook substantively important evidence that has wider theoretical implications. Particu-

larly, it aims at demonstrating how the relationship between crisis and internal violence is conditioned by two potential factors: the severity of the crisis and the presence or absence of governmental accommodation. Table 5 shows that the isolation of these variables during the critical periods of World Wars I and II, their postwar eras, and the 1930's depression reveals 10 out of 13 cases where crises are associated with increases in American domestic violence. However, the crucial test involving the statistical significance of Box-Tiao parameter estimates indicates that 6 of these 10 cases depict historical periods of unusual levels of domestic violence, all of which are linked with non-accommodating political environments. Conversely, 5 out of the 6 cases indicating a decline or weak positive increase in domestic violence are associated with accommodating U.S. political administra-

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tions. The overall evidence supports the hypotheses that link nonaccommodating and reform-oriented regimes with statistically significant and insignificant conflict linkages.

Although the levels of repression tend to be higher during crises than in noncrisis periods, nonaccommodating U.S. political administrations are associated more frequently with higher levels of repression than are conciliatory ones. The theoretical implications suggest that conservative political regimes are likely to generate higher amounts of repression during crises, thus escalating the magnitude of internal violence. When governmental elites recognize the legitimacy of the claims advanced by domestic groups (in at least a symbolic fashion), they are less prone to use repression, thereby breaking the cycle of intensive violence. When elites are willing to negotiate the claims of domestic contenders, conflict is likely to be preempted or substantially reduced, as demonstrated by the decline in economic violence with the presence of government-supervised collective bargaining in labor-management disputes during World Wars I and II.

Nevertheless, the reform policies of accommodating elites can also have the unintended consequence of stimulating more domestic violence. Two prominent illustrations are the 1933 National Industrial Recovery Act recognizing union representation in labor-management disputes and the 1954 Supreme Court decision that failed to endorse segregation in the public school system. Both events catalyzed the mobilization of labor and blacks against resisting forces. The subsequent attempts by labor and blacks to consolidate their new political gains brought renewed violence through clashes with nongovernmental elites and state and local officials who did not support national policies or views.

Consequently, decisions to support political reforms—no matter how limited

they may be—can be very risky. If there is concern with maintaining internal stability during critical periods of management, national decision makers are faced with making crucial policies that may open up a Pandora's box of unexpected outcomes. Their tendency to rely on status quo policies and repression becomes more understandable. Therefore, the context that ultimately supports political change becomes an important issue. Under what conditions are crises likely to support governing elites who are willing to sponsor political reforms? As the evidence indicates, the presence of a serious crisis alone does not automatically ensure political concessions, regardless of the magnitude of domestic violence.

Crises that coincide with fundamental transformations in domestic political environments are more likely to facilitate important long-lasting concessions. For instance, there is the political realignment in the 1930's that fostered the Democratic Party's capture of electoral support from the lower socioeconomic strata of the voting population. Although this event by itself was not directly responsible for New Deal social welfare innovations (like the Social Security and Wagner Acts), it did force political leaders to overcome both administrative weaknesses in the national government and a hostile seniority system in Congress in order to act on welfare issues through the federal government rather than through regular party channels (Skocpol, 1980, pp. 186-201). While the concessions won by the public were mediated by the presence of both an accommodating government and internal violence, they are less likely to have been sponsored without the political realignment (Piven and Cloward, 1977, p. 31). Another example is the culmination of the long-term growth in the northern black electorate that coincides with major reforms on behalf of blacks during and after World War II (Dalfiume, 1974, pp. 181-84; McAdam, 1982, pp. 81-86).

So the lesson is that governments become sensitized to the issues put forth by challengers, but are only motivated to support them when the political environment makes it advantageous to do so. In short, concessions come when governmental officials receive something in return, whether it is labor peace, elected office, or support for international policies. Therefore, Block's (1977) and Skocpol's (1980) thesis that crises open up political space for new reforms appears to have some validity, but only if their timing intersects with other fundamental changes in domestic social and political arrangements. At least that appears to be the case in the American experience.

The most significant aspect of this study has been the theoretical benefits derived from linking the behavior of governmental officials with the empirical relationships between crises and internal violence. It reconfirms the necessity of thinking about domestic violence in terms of a process of actions and reactions among multiple actors (governmental and nongovernmental), and about how the process is affected by the appearance of crises like war, postwar demobilization, and depression. Finally, the extent to which the evidence herein is a reflection of other, non-American, experiences depends on future investigations that also see the advantages of using "processual" perspectives.

Notes

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1. Some studies that emphasize these themes are

Almond, Flanigan, and Mundt (1973), Binder, Coleman, LaPalombara, Pye, Verba and Weiner (1971), Grew (1978), Skocpol (1979), Skowronek (1982), and Tilly (1975).

2. Although Stein (1978) investigates the relationship between war and domestic violence, the issue is subsumed within the broader framework of the effects of war on government centralization, industrial productivity, unemployment, and taxation. Violence is a minor consideration, and is used as an indicator along with crime rates and strikes to form a composite variable of social disunity. Consequently, his analysis is not included in the subsequent review.

3. Specific criticisms about the absence of intervening factors have been made by Skolnick (1974), Mack (1975), Stein (1976), and Bar-Siman-Tov (1983).

4. See Modelski (1974) and Thompson (1983) for conceptual distinctions between interstate and global wars.

5. These findings are not restricted to American international war involvements. The Civil War (1861-1865) is also associated with permanent effects similar to global wars.

6. Starting in 1935, Stohl (1976) subdivides pro-system violence into government-initiated and dominant group-initiated activity. In order to preserve the continuity from 1890 to 1970, the distinctions are not recognized in this analysis.

7. The intervention (I_t) is represented as a binary variable that ranges between zero (denoting the absence of the event) and one (denoting the presence of the event).

8. McCleary and Hay (1980) suggest that when the δ is equal to or exceeds 1.0, the level of the series changes by the quantity ω_0 in each post-intervention moment. In other words, before the intervention, the series is trendless; after the intervention, the series follows a trend with the parameter ω_0 interpreted as the slope. Such a radical change (from a state of equilibrium to a state of growth) is more than likely due to a post-intervention series that is too short to capture the equilibrium state of the process.

9. See Leng (1984), McCleary and Hay (1980), and McDowell, McCleary, Meidinger, and Hay (1980) for more details and illustrations.

10. The Pack (1977) computer program is used to estimate the final time series models.

11. The intervention components are coded to reflect the war years for 1898 (Spanish-American War), 1917-1918 (World War I), 1941-1945 (World War II), 1951-1953 (Korean War), and 1965-1970 (Vietnam War). Since America's entry into World War II began in December, 1941, the effects on domestic violence are not expected to have full realization until 1942.

12. Moreover, there are mathematical constraints that make it impossible to estimate five separate

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intervention components with a time series of only 80 observations.

13. Therefore, the intervention components are coded to reflect the postwar years of 1899, 1919-1920, 1946-1949, and 1954-1956. Although there are no strict theoretical guidelines for setting the parameters of postwar periods, this procedure is considered the most conservative approach.

14. McCleary and Hay (1980, pp. 171-85) advance the following formula for translating log metric omega (ω_0) parameters into percentage of change intervention estimates:

$$\text{Percent Change} = (\epsilon^{\omega_0} - 1)100,$$

where ϵ^{ω_0} is the ratio of post-intervention to pre-intervention equilibrium. However, in models with significant delta (δ) parameters, the incremental post-intervention movement of the process requires the analyst to exponentiate the asymptotic change ($\omega_0/1 - \delta$), in the process level.

15. For more details see Grubbs (1968), Larson (1975), and Milton (1982).

16. The social violence data exclude lynchings.

17. Attempts at estimating a higher order compound model that incorporates both the temporary changes in 1917 and 1918 also yield statistically insignificant omega estimates.

18. Estimation of a higher order compound model yields statistically insignificant omega estimates for the temporary changes in 1919 and 1920.

19. The work stoppage statistics for the post-World War I and post-World War II eras are based on figures obtained from the U.S. Bureau of the Census (1975, p. 179).

20. The figures are obtained from the U.S. Bureau of the Census (1975, p. 179).

21. Roosevelt's first administrative year, 1933, is characterized by largely symbolic gestures at this point, when New Deal leaders and agencies (e.g., the Federal Employment Relief Administration, Work Projects Administration, National Youth Administration, and Civilian Conservation Corps) made strong efforts to insure fair treatment of blacks. However, most of these and other programs faced major obstacles from local laws and customs, and federal officials were forced to capitulate on practical grounds (Fishel and Quarles, 1970).

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Erratum

Erratum in Robert Axelrod, "Presidential Election Coalitions in 1984" (March 1986, pp. 281-84):

Table 2, column 2, p. 283. The percentage contribution of Whites to the Republican coalition in 1980 was 98%, not 90%.

Forthcoming in December

The following articles and research notes have been scheduled tentatively for publication in the December 1986 issue:

Robert Axelrod. "An Evolutionary Approach to Norms"

Jonathan Bendor and Terry M. Moe. "Agenda Control, Committee Capture,

and the Dynamics of Institutional Politics"

Bruce Bueno de Mesquita and David Lalman. "Reason and War"

Gregory A. Caldeira. "Neither the Purse nor the Sword: Dynamics of Public Confidence in the Supreme Court"

Robyn M. Dawes, John M. Orbell, Randy T. Simmons, and Alphonse J. C. van de Kragt. "Organizing Groups for Collective Action"

Michael Doyle. "Liberalism and World Politics"

James D. Morrow. "A Spatial Theory of International Conflict"

John R. Schloz and Feng Heng Wei. "Regulatory Enforcement in a Federalist System"

Joel Schwartz. "Freud and Freedom of Speech"

CONTROVERSY

DEFENDING THE WELFARE STATE

An ethical defense of the welfare state may, as Robert E. Goodin has argued, adumbrate the notion that responsibility to provide for "needy strangers" flows from the same source as family responsibility. But this view of the moral responsibility for protecting vulnerable people is open to challenge. In this Controversy, Jeffrey Obler takes issue on this point, and Goodin replies.

Goodin (1985c) seeks to defend the welfare state by showing that just as we have responsibilities for the welfare of our families, we also have responsibilities for the welfare of poor strangers. To make his case, he uses the method of "reflective equilibrium," whereby he posits the general moral principle that accounts for a widely held moral intuition, and then applies it to an analogous situation for which there is no definite sense of what is right or wrong. He argues that we firmly believe we do have responsibilities for the welfare of our families (accepted moral intuition), because they are vulnerable to us (general moral principle). Our relations to poor strangers (for which there is no set moral intuition) are similar, in that poor strangers are also vulnerable to us. Given this essential similarity, he continues, we must also have responsibilities for poor strangers, which can be best discharged through state welfare.

While this defense is provocative and imaginative, it is not convincing, for, contrary to Goodin's claim, relations within families and among strangers are not analogous. There are moral reasons why we do not treat strangers the same way we treat our families, even when both are

vulnerable. Our relations to family members are morally distinctive, because we often voluntarily assume the obligation to care for them, and we often feel affection and love for them that are kindled by a biological bond and nurtured by sustained reciprocal caring. Vulnerability alone cannot account for our familial duties. It is not a sufficient condition. Nor is it a necessary condition; we do not abdicate our responsibilities to family members who are not vulnerable. The analogy, therefore, is not valid, and without this analogy, Goodin's defense of the welfare state crumbles.

Vulnerability Is Not a Sufficient Condition for Family Responsibilities

Children

Reflecting on children's responsibilities for parents and siblings, Goodin dismisses the argument that we have familial duties because we voluntarily assume them. He reminds us that "you do not ask to be born; you do not choose your parents or siblings" (Goodin, 1985c, p. 777). Given the apparent irrelevance of self-assumed obligation, he concludes that vulnerabil-

ity must be the sole source of our responsibilities. However, another conclusion is more plausible: in the absence of self-assumed obligations, children's duties are far weaker than those of parents and spouses. Our moral intuitions deny that siblings have a duty to provide each other financial support, and under some circumstances, children's duty to support their parents is doubtful, though there is little doubt that parents must care for their young offspring and spouses must care for one another. Public policy mirrors this difference. When the federal government determines eligibility for Supplementary Security Income, it takes into account the resources of parents and spouses but ignores those of children and siblings (*Social Security Handbook*, 1984, p. 308).

This is not to deny that children have a greater responsibility for parents and siblings than for strangers. This may be explained, however, by feelings of friendship and gratitude that have been fostered over years of reciprocal giving. Goodin rejects reciprocity as the basis for responsibility, pointing out that what children owe their parents does not depend on what they have received from them. Children's duties, he says, do not vary according to how long they remained at home. Nor can they repay debts the way they pay off a mortgage. "A favor and a return-favor do not cancel each other out and dissolve the relationship; instead they strengthen it" (Goodin, 1985c, p. 778).

Precisely! And if favors are never returned, the relationship atrophies. English (1979) says that children have no responsibilities for estranged parents. She says that parents who have nothing to do with their son because he married outside their religion cannot expect his support if they should need help. Embedded in ongoing relationships, she says, children's obligations cannot be taken for granted. For Blustein (1982), children owe their parents a debt of gratitude for having car-

ried out even minimal parental duties. Parents sacrifice this gratitude when they neglect their duties; nothing is owed them for simply having borne a child. Contrary to Goodin's claim, then, there is a connection between what parents have done for their children and what children owe their parents.

Parents

Many kinds of people can and do care for small children. Yet our moral intuitions tell us that natural parents have a special responsibility for the children they bear. The basis of this is self-assumed obligation. In a society where natural parents are expected to rear their offspring, people who decide to have children, or decide to accept the risk of having children, voluntarily assume the responsibility to care for them until they achieve independence. For Blustein (1982, pp. 147-48), the guiding moral principle is that "people are responsible for the foreseeable consequences of their voluntary acts." The contractual quality of the bond is underscored when biological parents sign an agreement to relinquish their rights over and duties toward their child, and adoptive parents sign an agreement accepting responsibility for the child's care.

While he does not directly address this issue, Goodin, given his position on responsibilities, would surely reject the argument that biological parents have special obligations. This argument, he would say, mistakenly confuses "causal" and "task" responsibilities. He says that people who create a difficulty are not necessarily required to resolve it (Goodin, 1985c, p. 780). This task should fall on those best equipped to deal with the difficulty, whether or not they caused it. He presumably would argue that while indigent parents bring vulnerable children into this world, the responsibility for providing for the children's material support

should be assigned, at least in part, to more prosperous taxpayers. In fact, Aid to Families with Dependent Children does shift at least part of the caring responsibility from poor biological parents to more affluent strangers.

To accept that the state has a duty to care for defenseless children, however, is not to concede that the source for this duty is the same for the state as it is for parents. Adoptive and natural parents differ from the state in that they voluntarily agree to rear children. As O'Neill (1979, pp. 30-31) puts it, "Only if the state controls and mandates procreation—as in Plato's *Republic*—could one hold that the state acquires the same sort of obligations as persons do by deciding to procreate, so undertaking to rear." The fact that the state cares for children who have been abandoned by their parents does not exonerate the parents from moral guilt. This guilt is meted out because parents have a special responsibility, rooted in their choice to bear children. If the state has a duty, its source is the children's vulnerability, but vulnerability alone cannot account for the parents' responsibility. Goodin's analogy simply does not work.

Spouses

Goodin's case seems particularly weak with regard to responsibilities for spouses, for after all, people do accept the obligation to care for each other when they voluntarily choose to marry. Yet Goodin insists that the marriage contract falls short of an ideal legal contract, inasmuch as its provisions are set by the state and not by the marriage partners. Even if this is so, self-assumed obligation is still the basis of responsibilities between spouses. A person cannot deny the duties assigned to him by an agreement he has voluntarily signed, on the grounds that he did not write it. Having composed a contract is not a legal or a moral condition for honoring it.

Indeed, Goodin concedes that the deci-

sion to marry establishes matrimonial duties, but he argues that it does not specify the content of these duties; for this, he says, one must rely on his vulnerability model. He offers no justification for this view, which is not supported even by the most cursory view of marriage. Thus, two vulnerable people do not assume mutual obligations until the day they marry, and in most cases, the obligations end the day a divorce is granted. In fact, after a bitter divorce, spouses may derive a perverse satisfaction from their former partner's heightened emotional or financial vulnerability.

Public policy and the law recognize the contractual quality of the marriage bond. Whether a person is eligible for a share of the spouse's Social Security benefits depends not on one's dependency, but on how long one has been married; a rich person who had been married for 10 years receives a pension, while the poor person who had been married for only 9 years receives nothing. In most states, a person loses the right to support if he or she is even partially responsible for ending the marriage. A vulnerable adulterer is normally not entitled to alimony.

Vulnerability Is Not a Necessary Condition for Family Responsibilities

For Goodin (1985c, p. 779), "A is vulnerable to B if and only if B's actions and choices have a great impact on A's interests." Thus we have responsibilities for our families, because what we do affects their interests. Goodin's definition, however, is at odds with common usage in that it ignores the vulnerable person's condition in relation to other persons. According to Cassel's *Modern Guide to Synonyms & Related Words* (1971, p. 652), "Vulnerable always stresses a lack of protection against physical or mental harm." In ordinary usage, the vulnerable person is defenseless, assailable, un-

protected, easily wounded, and susceptible to injury. This discrepancy bears on where one locates the source of family duties. Consider a millionaire son whose parents must decide whether to leave him their sizable fortune. According to Goodin's definition, the millionaire is vulnerable (his parents' choice will have a great impact on his interests), and accordingly, this vulnerability explains why the parents should decide in their son's favor. If we set aside the bequest's possible emotional ramifications, however, it seems bizarre to consider the millionaire as vulnerable. Even if he does not inherit a dime from his parents, he would not be defenseless or unprotected, and it is highly unlikely his parents fear that, in the absence of the bequest, their son would be susceptible to injury. In this case, whether the parents bequeath their estate to their son will depend not on his vulnerability, but on the affection and love they may or may not feel for him, or on their sense of family loyalty and continuity. Vulnerability, at least as the term is commonly used, is not a necessary condition for family responsibilities.

Goodin correctly recognizes that vulnerability is an important source for state responsibility for the poor, but he incorrectly believes that this responsibility would become more palatable if people would appreciate how it has the same basis as familial duties. His effort is bound to fail, because most people understand that family responsibilities are rooted in many complex considerations other than mutual vulnerabilities.

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New Right opponents of state welfare provision are anxious to shift the burden back to the private sector. The only thing

they are keener on than "family responsibility" is "self-reliance," which at their hands comes to one and the same thing (Goodin, 1985b).

In my ethical defense of the welfare state, I attempted to turn the New Right fondness for family responsibility to strategic advantage (Goodin, 1985c; see also Goodin, 1985a, pp. 70-92). There, I argued that our responsibilities to provide for needy strangers derive morally from the same source as our responsibilities to provide for members of our own families. If so, then opponents of the welfare state can hardly repudiate the former while conceding (indeed, celebrating) the latter. To maintain an inconsistent attitude toward these two sorts of responsibilities, savoring the one while shunning the other, New Rightists would have to resist my argument about their shared moral basis in vulnerabilities.

Obler seems to concede that responsibilities toward needy strangers might derive from their vulnerability to our actions and choices, but he argues that responsibilities toward family members derive from different sources; vulnerability is neither a necessary nor a sufficient condition of our having such responsibilities. Furthermore, if this proposition is to do the work cut out for it in the New Right's larger argument against the welfare state (and it is unclear whether Obler wants to join them in that argument), this different basis must be such as to make family responsibilities substantially and systematically stronger than responsibilities to aid needy strangers based on vulnerability.

To differentiate family responsibilities from broader social ones, Obler points out that the former are assumed voluntarily, infused with affection born of biology, and sustained by reciprocal caring. This may be characteristically true of most family relationships, but is not necessarily true of any of them, and, at least in part, can never be true of some of

them. The filial relationship cannot be voluntarily assumed, yet surely children are morally (and were, until recently, legally) responsible for caring for their aged parents. Obler concedes that example, saying that affection born of reciprocity can substitute for voluntary assumption of responsibilities. Thus, Obler seems to suppose that these factors can work independently to generate moral responsibilities.

Recall that Obler's self-defined task is to explain the "moral distinctiveness" of family responsibilities. To do that, he must invest each of the factors he nominates with some independent moral importance. Then, and only then, will they give rise to family responsibilities directly, in and of themselves. Then, and only then, will he have succeeded in offering an alternative account of the moral basis of family responsibilities.

While saying much to explain why those factors might matter psychologically, Obler says virtually nothing to explain why any of them should matter morally. In "reciprocal caring," it cannot be the reciprocity alone that matters. If so, children who leave home early would owe less to their aged, needy parents, and once they repaid their debts their responsibilities would end. Neither is true. In "affection born of biology," the biological link, although perhaps crucial psychologically, cannot possibly carry any moral clout. As for the "caring" and "affection" that run through both of these explanations, surely it cannot be right to say that we have moral responsibilities with respect to—and, self-assumed obligations apart, only with respect to—people toward whom we harbor warm feelings. Again, it may be good psychology, but the morals of the matter are something else.

The only really credible counterproposal is Obler's suggestion that family responsibilities are morally distinctive because they are (usually) assumed volun-

tarily. What it is about voluntary assumption that is morally special, Obler does not say. Others, however, point to personal autonomy, the will binding itself, etc. However, our responsibilities to children conceived accidentally—through leaky condoms or diaphragms, for example—are no less strong than our responsibilities to children whose existence we have fully willed. Furthermore, if the will binding itself is what really matters morally, then people's wills ought to be allowed to converge on any marriage contract they like. In practice this is not the case; there are certain "essential incidents" of a marriage contract (such as the duty of support) that cannot be legally waived, even by mutual consent (Goodin, 1985c, p. 777). The point is not that having composed a contract yourself is a precondition for your being morally bound to honor it; rather, if consent (the will binding itself, etc.) is what matters morally, then what is made by consent should be changeable by consent, and if the marriage contract cannot be changed, even by mutual consent, then it is scarcely credible to claim that the real moral force behind the contract lay in consent (voluntary agreement, acts of will, etc.) in the first place.

Obler's phenomenology is fine. Family responsibilities are often (if not always) assumed voluntarily, infused with affection, and sustained by reciprocal caring; it certainly feels as if the moral importance of family responsibilities is somehow tied to these characteristics. None of them, however, is itself the ultimate source of morally-charged family responsibilities. Instead, all merely point to mechanisms by which vulnerabilities might be engendered, and any moral importance attaching to those considerations is derivative from the moral importance of protecting the interests of vulnerable others.

People who have entered into voluntary agreements with one another, or who are emotionally involved with one

another, or who are in a relationship of reciprocity with one another, will naturally rely upon (and hence be vulnerable to) one another in various respects. It is the sheer fact of this vulnerability, rather than how the vulnerability came about, that imparts moral force to the responsibilities of the relationship. For proof, note that responsibilities arising even under voluntary agreements diminish virtually to the vanishing point in situations where the promisee is in no way vulnerable to the promisor's defaulting: courts award purely nominal damages for breach of contract in situations where the promisee is in no way relying upon the promised performance (Goodin, 1985c, p. 777). So too with divorce courts: they refuse awards of support to ex-spouses capable of supporting themselves, imposing them only where one ex-partner or a child was necessarily relying upon the other for provision of reasonable necessities (Goodin, 1985a, p. 77). It is the absence of reliance, rather than the mere dissolution of the contract, that relieves marriage partners of their obligations to one another.

People have "emotional interests" every bit as strong as their material ones (Goodin, 1985c, p. 779). The resulting emotional vulnerabilities largely explain the "responsibilities of gratitude" Obler says children owe non-neglectful parents (Goodin, 1985a, pp. 104-7). They also explain why it is better for parents to take principal responsibility for feeding their own children, rather than leave the task to richer neighbors (Goodin, 1985c, pp. 778, 785), and why Obler's millionaire parents ought (prima facie, at least) to bequeath their fortunes to an already-rich son: his feelings, if not his standard of living, would be hurt if they left the money to the servants instead.

Such emotional vulnerabilities, if

strong, can give rise to responsibilities that are relatively strong, but not necessarily so strong that they should always trump responsibilities growing out of material vulnerabilities of a more ordinary sort. In the case of the already-rich heir, we can surely imagine a set of servants so penurious we would all agree that, on balance, the parents' stronger responsibility would be to give their money to the servants instead of to their own child: however badly his feelings would be hurt, their starving bellies would hurt even more. Emotional vulnerabilities thus count for something, but not everything. The responsibilities growing out of them do not systematically trump the responsibilities to aid needy strangers in the way that the "family first" policies of the New Right would require.

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RESEARCH NOTES

ARE SOCIAL BACKGROUND MODELS TIME-BOUND?

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In this research note I seek to determine whether a significantly predicting social background model for analyzing the votes of Supreme Court justices is time-bound. I argue that an affirmative result poses serious questions for past uses of such models, none of which has controlled for the possibility that time is a confounding variable. A model that significantly predicted the votes of the justices in the Court's 1993-1968 terms was constructed. Analysis with this model for two periods—from 1903 to 1935, and from 1936 to 1968—established that the model was not time-neutral. Appropriate theoretical implications are drawn.

In introducing Justice Harry Blackmun recently, Norman Dorsen (1985, p. 13), professor of Law at New York University, explained the alleged metamorphosis in Blackmun this way: "We must never forget that the boy is father to the man, that the seeds of the fully mature person are long embedded in his character. One need not embrace Freudian psychology to conclude that early experience and training will be reflected in later actions and decisions, and that flexibility and open-mindedness are themselves the product of what has gone before."¹ Dorsen's comment is about as concise a statement of the social background theory as could be penned, and it reminds one of Fred Rodell's remark about Roger Brook Taney: "There is not one of his decisions as Chief Justice . . . but can be traced, directly or indirectly, to his big-plantation birth and background" (Schmidhauser and Gold, 1963, p. 484).

The fundamental proposition underlying the social background models employed in the judicial area is that past social experience, broadly defined, significantly influences the decisions that judges will make when confronted with socio-legal conflicts in their courts. Although tests of this proposition with federal and state judges through the years have produced mixed results, a number of studies have reported interesting findings (Goldman, 1966, 1975; Heike, 1978; Nagel, 1961, 1962; Schmidhauser, 1959, 1961, 1962; Schmidhauser and Gold, 1963; Ulmer, 1962, 1973a; Walker and Hulbary, 1978). The most ambitious of these studies (Tate, 1981) has reported recently that from 70% to 90% of variance in the voting of Supreme Court justices in a 30-year period was accounted for by seven variables: political party identification, the appointing president, prestige of prelaw education, appointment from elective office, the region of

appointment, extensiveness of judicial experience, and type of prosecutorial experience.² Yet, as impressive as these studies may be, skeptics have not been lacking (Adamany, 1969; Bowen, 1965; Grossman, 1967; Hensley and Dean, 1984). Some critics have reported negative results with social background models. Others have articulated theoretical weaknesses in the model. None, however, have explored what I consider to be a major shortcoming in past applications: the failure to control for the possibility that such models are time-bound.

In a good play, one act is as good as any other. Similarly, other things being equal, if the past experiences of judges explain their decisional behavior, these explanations should hold in any and all segments of the time span examined. The relationship between social backgrounds and the decisions of judges should not depend on the particular composition of courts, nor on any other variables for which time is a surrogate. If that is not the case, what has been offered as theoretical results may be merely descriptive of particular courts at particular points in time. In such an event, the ability of social background theory to account for judicial behavior would be undermined. A proper assessment of such models, therefore, requires attention to time as a possibly confounding factor. In the remainder of this paper, the question posed in the title is explored by (1) constructing a social background model that explains a significant percentage of the variation in justice voting, (2) examining the model to see if it is time-bound, and (3) assessing the implications of the results.

Research Design

Let us begin with the assumption that governmental authority is an attitudinal

object, with justices differing as to whether such authority should be maintained when in conflict with individual liberty, and at what cost (Ulmer, 1973b; Ulmer and Stookey, 1975). Our first question is whether the support given governmental authority by Supreme Court justices has varied appreciably across justices in the Court's terms from 1903 to 1968.³ Tables 1 and 2 provide an affirmative answer to that question.⁴ Four versions of a social background model are hypothesized to account for the observed variation.⁵

Predictor Variables

To assess the constancy of the model, analyses are carried out for two different time periods: 1903-1935 and 1936-1968. In effect, we compare two equal time periods, each of which is longer than that examined by Tate (1981). The analysis is pursued via several predictor variables.

The first predictive factor is whether the justice is an only or first-born child. In the last 20 years the scholarly community has produced over 400 articles and dissertations on birth order (Weber, 1984). In general, this work suggests that social and personality development is in part a function of birth order, mediated of course by the experiences one has with other family members as a consequence of being the oldest, middle, youngest, or only child. Such experiences are thought by some to influence the choices of marriage partner and vocation, and to have something to do with level of achievement.

Weber has applied birth order analysis to 102 Supreme Court justices. He reports that first-borns "tend to imbibe parental norms to a greater extent than later children, are conscientious, *anxious to please authority*, ambitious, and high achievers. . . . They . . . either prefer and/or have a high tolerance for tightly structured situations" (Weber, 1984, p.

1986 Social Background Models

Table 1. Supreme Court Justices' Support for the Government in Government vs. Underdog Cases, 1903-1968 Terms, by Court, Individual Justice, and Level of Government

Justice	State and Local Government				Federal Government			
	Court Support (%)	Justice Support (%)	Above or Below Court	N	Court Support (%)	Justice Support (%)	Above or Below Court	N
Harlan I	73.4	71.8	—	323	69.1	58.0	—	217
Fuller	71.9	69.0	—	288	68.5	54.6	—	181
Brewer	71.5	66.1	—	260	64.5	58.6	—	162
Brown	73.1	66.3	—	119	61.3	51.2	—	80
White, E.	72.5	70.8	—	768	67.4	65.9	—	613
Peckham	72.4	65.4	—	243	65.3	59.3	—	150
McKenna	71.3	69.9	—	916	65.9	65.4	—	831
Holmes	70.4	71.9	+	1273	65.6	63.3	—	1292
Day	72.3	70.4	—	812	68.1	64.6	—	678
Moody	71.3	65.8	—	164	77.8	53.5	—	99
Lurton	76.4	72.2	—	216	72.0	58.0	—	200
Hughes	71.8	69.6	—	831	66.6	63.2	—	1012
Vandevanter	70.1	67.4	—	1283	64.6	61.9	—	1466
Lamar, J. R.	74.0	67.8	—	246	71.6	57.2	—	215
Pitney	72.0	71.1	—	478	67.8	64.3	—	460
McReynolds	69.2	64.6	—	1255	64.2	58.0	—	1474
Brandeis	69.8	73.5	+	1086	64.1	64.7	+	1353
Clarke	72.7	73.0	+	256	69.7	62.7	—	290
Taft	66.7	63.8	—	423	64.9	61.7	—	544
Sutherland	69.1	66.3	—	782	63.4	57.9	—	996
Butler	68.5	63.5	—	840	63.3	58.1	—	1084
Sanford	65.5	63.2	—	348	65.0	59.3	—	448
Stone	68.1	70.6	+	926	65.2	65.7	+	1421
Roberts	67.1	65.1	—	629	64.7	57.4	—	1005
Cardozo	71.2	74.4	+	309	61.3	68.3	+	354
Black	47.1	44.6	—	1039	64.2	58.5	—	1882
Reed	60.4	62.3	+	523	66.3	68.1	+	1163
Frankfurter	56.7	57.3	+	624	65.2	65.7	+	1430
Douglas	45.1	30.1	—	956	64.0	50.4	—	1727
Murphy	60.4	55.2	—	288	67.9	61.5	—	663
Jackson, R.	58.6	55.8	—	324	65.3	55.3	—	685
Rutledge	58.3	48.1	—	187	66.0	61.1	—	435
Burton	56.0	61.4	+	298	62.2	65.2	+	719
Vinson	59.1	57.5	—	186	64.1	63.3	—	395
Minton	58.3	65.4	+	139	65.2	68.7	+	342
Clark	40.1	51.7	+	529	62.3	70.6	+	893
Harlan II	31.8	53.7	+	525	60.8	61.9	+	739
Warren	33.3	25.4	—	547	61.6	54.1	—	829
Whittaker	46.8	50.3	+	141	62.5	58.6	—	283
Stewart	31.5	41.4	+	466	63.0	60.6	—	592
White	27.5	42.3	+	352	65.1	73.1	+	358
Goldberg	20.1	18.6	—	134	65.3	54.6	—	150
Fortas	27.3	25.2	—	194	57.3	57.4	+	178
Marshall	32.2	28.0	—	121	72.6	57.5	—	73
Brennan	33.2	25.4	—	515	61.8	55.3	—	712
Byrnes	—	—	—	—	65.9	64.6	—	82

Table 1 (continued)

Justice	State and Local Government				Federal Government			
	Court Support (%)	Justice Support (%)	Above or Below Court	N	Court Support (%)	Justice Support (%)	Above or Below Court	N

Table 2. Supreme Court Justices' Support for the Government in Government vs. Criminal Cases, 1903-1968 Terms, by Court, Individual Justice, and Level of Government

Justice	State and Local Government				Federal Government			
	Court Support (%)	Justice Support (%)	Above or Below Court	N	Court Support (%)	Justice Support (%)	Above or Below Court	N
White, E.	92.9	91.4	—	70	73.2	69.0	—	71
McKenna	92.9	92.9	—	85	69.3	67.0	—	88
Holmes	90.4	92.5	+	135	67.1	63.3	—	161
Day	93.3	92.0	—	75	75.7	71.4	—	70
Vandevanter	89.0	86.6	—	127	63.7	62.5	—	179
McReynolds	81.8	83.3	+	132	59.7	58.0	—	186
Hughes	75.6	75.6	—	74	63.5	62.7	—	118
Brandeis	87.0	86.0	—	115	59.7	56.8	—	176
Taft	89.1	89.1	—	55	58.1	58.0	—	62
Sutherland	86.2	84.0	—	94	61.2	58.9	—	139
Butler	83.3	81.2	—	96	59.7	55.5	—	144
Stone	65.0	68.5	+	143	55.8	60.1	+	206
Roberts	58.3	67.7	+	96	53.6	53.5	—	153
Black	32.3	25.6	—	401	47.0	32.3	—	421
Reed	45.2	58.9	+	146	54.2	66.5	+	212
Frankfurter	43.7	47.2	+	199	50.0	46.7	—	312
Douglas	31.8	11.4	—	393	45.9	27.4	—	405
Murphy	41.6	23.5	—	89	47.5	26.7	—	101
Jackson, R.	50.5	60.9	+	105	52.7	48.8	—	129
Rutledge	44.4	22.2	—	63	54.1	40.5	—	74
Burton	50.5	67.8	+	109	52.1	67.8	+	165
Vinson	57.6	65.6	+	66	57.3	70.6	+	75
Clark	32.2	49.1	+	236	46.6	65.4	+	249
Harlan II	25.0	51.1	+	268	41.2	61.9	+	226
Warren	24.4	14.7	—	272	43.0	27.7	—	249
Whittaker	38.2	50.9	+	55	47.4	57.8	+	95
Stewart	22.6	37.1	+	234	41.1	54.7	+	168
White, B.	20.9	41.8	+	191	37.2	54.6	+	86
Fortas	22.3	17.3	—	121	35.1	40.3	+	57
Brennan	24.7	15.8	—	259	41.0	28.7	—	212
Sanford	—	—	—	—	55.6	51.8	—	54
Minton	—	—	—	—	62.8	75.6	+	78
Goldberg	16.9	12.3	—	65	—	—	—	—
Marshall	19.4	17.9	—	67	—	—	—	—

Note: Cases involving litigants characterized as black, alien, subversive, or labor union members are excluded from the data.

561, *italics mine*). Although controversially, it has been contended that first-borns, since they tend to be conservative and authoritarian, do not empathize with underdogs.

According to Weber, first-born Supreme Court justices, 56 in number, significantly exceed the expected number of 38—the approximate percentage of first-borns in the general population. Using Tate's ranking of 25 justices on support for civil liberties, he also reports that no significant correlation was found between birth order and support for civil liberty claims. However, I have calculated the mean rank for first-borns and compared it to the mean rank for those who were middle- or last-born. The respective means were found to be 15.33 for first-borns and 10.84 for non-first-borns. Consequently, we surmise that first-born Supreme Court justices will significantly favor government (authority) over underdog and criminal litigants in the Court's cases matching the government against such litigants.

The second factor to be considered is whether a justice's father was at any time a state or federal judicial officer, a legislator, or an executive officer—elected or appointed. This choice is based upon the assumption that government service promotes a favorable attitude toward government and its needs. It is consistent with Tate's (1981) finding that justices who had been prosecutors were more conservative than nonprosecutor judges, and with my earlier finding that federal administrative experience was positively related to support for government in criminal cases (Ulmer, 1973a). Our expectation is that Supreme Court justices whose fathers had governmental experience imbibe pro-government sentiments, and thus significantly favor the government in cases in which it is matched with underdog and criminal litigants.

The third and last predictor is the political party affiliation of the justices. One

may note earlier findings that, in limited settings, Democratic judges have significantly favored the claims of the injured and the unemployed (Nagel, 1961, 1962; Ulmer, 1962) and of the defendant in criminal cases (Nagel, 1961). Democratic judges have also been found to be more likely than Republican judges to find a constitutional violation in criminal cases (Nagel, 1961) and to be more liberal than Republican judges (Goldman, 1975; Goldman and Jahnige, 1985). At the Supreme Court level, Tate has shown that party is a significant predictor of the justices' votes in civil liberty cases, which means, among other things, that Republicans favor government in such cases while Democratic justices tend to favor underdog and criminal litigants.

Such findings, of course, do not tell us whether party shapes attitudes or attitudes are shaped by party choices, or whether other experiences in life determine attitudes and party choice. We surmise that, unlike some experiential variables, party choice is a function of multiple experiences, and thus a surrogate for the aggregate of attitudes these multifarious experiences produce. It may not be strictly logical, therefore, to argue that party "causes" or influences judicial votes. It is logical to view the variable as a surrogate for background factors that, for the present, remain unidentified. Our general expectation is that when government is matched against underdog and criminal litigants, Republican justices will favor government significantly more often than Independent justices, and Independents more often than Democrats.

These predictors are employed with four versions of the same model, each of which incorporates the same dependent variable—support for government. Table 3 summarizes and distinguishes the four versions of the model (hereinafter referred to as Models 1, 2, 3, and 4). In each case, we suggest that the three predictors will account for a significant portion of the

Table 3. Four Models for Predicting Variation in Support for Government from Backgrounds of Selected Supreme Court Justices, 1903–1968 Terms

Model	Predictors	Criterion
1. Cases matching state or local government against underdogs	Father a state officer; party of justice; justice a first-born	Support for state or local government
2. Cases matching federal government against underdogs	Father a federal officer; party of justice; justice a first-born	Support for federal government
3. Cases matching state or local government against non-underdog criminals ^a	Same as 1	Support for state or local government
4. Cases matching federal government against non-underdog criminals ^a	Same as 2	Support for federal government

^aCases involving criminals characterized as black, alien, subversive, or labor union members are excluded.

variation in the support the justices give state/local or federal governments, as appropriate.

Dependent Variable

To construct a measure of the dependent variable, we first determine the support each justice gave to government during his entire career by taking support for government as a percentage (*A*) of all cases falling in the prescribed category. A similar percentage (*B*) is taken for each Court (the majority vote) in the same time period. The ratio of these percentages (*A/B*) is then calculated for use as the criterion in an ordinary least squares regression (OLS) analysis.⁶

For each of the models summarized in Table 3, the first question is whether the three independent variables are effective predictors of the criterion appropriate for each model. An ordinary least squares analysis is then applied to each of the four models for the Court's terms from 1903 to 1968, using only those justices who participated in at least 50 cases on the dimension being analyzed.

Findings

The results for Models 1, 2, and 4 may be quickly summarized. These models

explain an insignificant and miniscule proportion of the variance in the justices' voting behavior. Thus, the expectations expressed for these models are not realized.

For Model 3, the picture is somewhat different. Several regression analyses have been run for this model. The equation for the first analysis is

$$y = a + b_1FAST_1 + b_2FB_2 + b_3PARTY_3 + e, \quad (1)$$

where *y* is the ratio of the percentage of support for government by the justice and the Court (justice/Court), and *FAST*, *FB*, and *PARTY* refer to the justice's father as a state officer, the justice as first-born, and the justice's party affiliation, respectively. Each of the first two variables is recorded as 1 if present and 0 if absent. *PARTY* is recorded as 2 for Republicans, 1 for Independents, and 0 for Democrats. Due to coding procedures, the signs of all regression coefficients are expected to be positive. The results of this and related analyses are provided in Tables 4, 5, and 6.

Table 4 shows that the model explains a significant percentage of the variance in support for government. The question that must be addressed, however, is

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Table 4. OLS Regression of Father as State Officer, Justice as First-Born, and Political Party of Justice on Support for Government, 1903–1968 Terms

Variable	Regression Coefficients	Standard Error	Significance
Father as state officer	144.97	42.76	$p < .01$
Justice as first-born	12.85	20.45	n.s.
Party of justice	5.94	10.80	n.s.
Constant = 92.70			
$R^2 = .359$			

whether this finding is valid or merely an artifact of the time dimension used in delimiting our data base. To assess that possibility, we need to know whether the effect of time on slopes b_1 , b_2 , and b_3 undermines the inferences to be drawn from the first analysis. The equation needed is

$$y = a - b_1 \text{FAST} + b_2 \text{FB} + b_3 \text{PARTY} + b_4 \text{TIME} + b_5[(\text{FAST})(\text{TIME})] + b_6[(\text{FB})(\text{TIME})] + b_7[(\text{PARTY})(\text{TIME})] + e, \quad (2)$$

where *FAST*, *FB*, and *PARTY* are as defined for equation (1); *TIME* is a dummy variable that equals 0 for the 1903–1935 terms and 1 for the 1936–1968 terms; and the model includes several

multiplicative terms involving *TIME*. This model allows for the possibility that the effects of *FAST*, *FB*, and *PARTY* on a justice's support for government during the first time period, when *TIME* = 0, differ from the effects in the second period, when *TIME* = 1. In particular, b_1 , b_2 , and b_3 represent the slopes of the effects of *FAST*, *FB*, and *PARTY*, respectively, on support for government during the pre-1936 period. In contrast, b_5 , b_6 , and b_7 measure the differences in slopes of effects from the pre-1936 period to the post-1935 period. For example, b_5 represents the extent to which the slope of the effect of *FAST* on support for government is greater in the second time period than in the first.

Table 5 portrays the unstandardized

Table 5. OLS Regression of Father as State Officer, Justice as First-Born, Political Party of Justice, and Time on Support for Government, 1903–1968 Terms

Variable	Regression Coefficients	Standard Error	Significance
Father as state officer	-2.25	44.69	n.s.
Justice as first-born	2.16	24.10	n.s.
Party of justice	1.34	13.18	n.s.
Time	1.65	28.18	n.s.
[(Fast)(Time)]	289.97	64.30	$p < .001$
[(First-born)(Time)]	-10.46	31.65	n.s.
[(Party of justice)(Time)]	16.33	17.20	n.s.
Constant = 97.57			
$R^2 = .729$			

Table 6. OLS Regressions of Father as State Officer, Justice as First-Born, and Party of Justice on Support for Government, 1903-1935 and 1936-1968 Terms

Variable	Regression Coefficients	Standard Error	Significance
<i>1903-1935</i>			
Father as state officer	-2.25	5.10	n.s.
Justice as first-born	2.16	2.75	n.s.
Party of justice	1.34	1.50	n.s.
Constant = 97.57			
R ² = .184			
<i>1936-1968</i>			
Father as state officer	287.72	58.33	<i>p</i> < .001
Justice as first-born	-8.29	25.90	n.s.
Party of justice	17.67	13.95	n.s.
Constant = 99.22			
R ² = .723			

slope coefficient estimates for the equation. The most dramatic differences in effects across the two time periods is for the variable *FAST*. *FAST* has almost no effect on government support for the pre-1936 period, as its slope coefficient is almost zero (-2.25). But the slope coefficient for *FAST* during the post-1935 period is 289.97 units higher than the slope in the first time period, and this difference in slopes is statistically significant, as the coefficient for [(*FAST*)(*TIME*)] is significant at the .001 level. While there are also differences in the slopes of the effects of *FB* and *PARTY* on support for government across the two time periods, the standard errors of the coefficients for [(*FB*)(*TIME*)] and [(*PARTY*)(*TIME*)] indicate that differences in slopes are not statistically significant.

In Table 6, what are essentially the results of the analysis in Table 5 are put in a different form for emphasis. It will be noted that the OLS regression for the Court's 1903-1935 terms accounted for 35.9% of the variance when time was ignored, but 72.9% when time was incorporated in the analysis. In Table 6 the

regression is run first for the period 1903-35, and compared with the results of a regression analysis run on the 1936-68 terms. The significant disparity between variance accounted for in the first period (18.4%) and that accounted for in the second (72.3%) underscores the impact of time period when the effect of *FAST*, *FB*, and *PARTY* on voting ratios is determined. One can only infer that in the initial 66-year analysis of support for government, the impact of social background factors on the justice's voting decisions was time-bound.

Summary and Discussion

This paper asks whether social background models for explaining the votes of Supreme Court justices are time-bound. Adopting a research design that made it unlikely that social background would explain variation in justice voting across a 66-year period, a three-predictor model was identified that accounted for a statistically significant percentage of the variance: 35.9%.

Incorporating a time dimension in the

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analysis, however, we found a statistically significant difference in the impact of the three predictors in the Court's 1903-1935 terms when compared to its 1936-1968 terms. Indeed, when regression analyses were run separately for these two periods, the model accounted for 72.3% of the variance in the second period, but only 18.4% in the first. The result for the 1936-1968 terms compares favorably with the most successful use of social background models in studying justice voting. Such a result would be eminently publishable by past standards. The result for the first period, on the other hand, would not likely be considered worth reporting to the discipline. The conclusion to be drawn, in any event, is that the social background model examined in this paper is not time-neutral.

Although our results do not speak directly to past applications of social background models, our analyses suggest the possibility that all past applications in the judicial area have been time-bound—that is, they work only for particular sets of judges at particular points in time.⁷

If this fact were established, it would tend to undermine a major tenet of social background theory, namely, that social background is a relatively unattenuated program for future behavior. At least it would seem essential that future users of the model explore nonbackground variables that possibly condition the impact of social attributes on subsequent behavior—variables for which time is possibly a surrogate.

None of our findings establishes necessarily that social background models in general are time-bound, although they do suggest that such a question must now be raised about any such model that claims to explain Supreme Court decisions. The reasons are two. First, we have investigated particular sets of cases in particular time periods. Our results may not hold for other cases or other time spans. Second, and more important, the failure

of our models or others to predict voting behavior does not establish that social background has nothing to do with shaping attitudes toward objects. Certainly it does not establish that attitudes toward objects are irrelevant to voting behavior, for we are left with the possibility that, while social backgrounds do shape attitudes, and attitudes influence behavior, held attitudes will not always be reflected in behavior that would be consistent with them. Just as a racist, if appropriately circumscribed by law, peer pressure, likelihood of censure or sanction by relevant others, etc., may withhold racist behavior, a Supreme Court justice whose political loyalties would normally induce him to support the president who appointed him may not do so given significant countervailing pressures. This point is illustrated, perhaps, by Chief Justice Burger's vote in *United States v. Nixon* (1974), and that of Oliver Wendell Holmes in the *Northern Securities Case* (1904).

All this suggests the need for new research addressed to (a) the direct relationship between social attributes and attitudes, and (b) the conditions under which established attitudes will lead to specific behavior among judges. The second of these can only be addressed satisfactorily after the first has been established. To reach solid inference about the first for Supreme Court justices is admittedly a difficult challenge.

I would distinguish attribution of attitudinal objects from inferences about such objects derived by such reliable procedures as interviews and survey instruments. The latter investigatory tools are simply not available for assessing attitudes in the Supreme Court. A possible strategy here would be to investigate the validity of social background linkages to attitudes by focusing on lower court judges who will give interviews and, possibly, even respond to questionnaires. Establishing the validity of the general model in that rather direct fashion would

presumably then lend greater credence to more indirect efforts to account for the behavior of Supreme Court justices by examining their social backgrounds.⁸

Notes

I wish to express my appreciation to Jim Gibson of the University of Houston for helpful comments on an earlier draft of this paper, and especially to my colleague Bill Berry, whom I consulted on the methods section of the paper. Any deficiencies remaining are the sole responsibility of the author.

1. Dorsen's comment is only a recent manifestation of a belief held by many who have studied Supreme Court justices over the years. Murphy's biographer (Howard, 1968) noted that Justice Murphy was especially attentive to claims based on unorthodox religious belief, and that Murphy's attitudes toward obscenity, family law, and divorce were influenced by his religion. Sutherland's biographer suggested that Mormon influences solidified Sutherland's Spencerian ideology (Pascal, 1951). Alpheus Mason (1956) notes the influence of an Amherst teacher on Justice Stone's philosophy. McGrath (1963), writing about Waite, concluded that the justice's frontier experiences in Ohio prior to coming to the Court left him with a faith in people that translated into support for popular legislatures once Waite became a judge. Systematic study of the relationship between the social background of judges and the decisions they make can be traced as far back as the thirties (Ewing, 1938; Mott, Albright, and Simmerling, 1933). However, political science did not exhibit much interest in this area until two decades later. In a five-year period from 1959 to 1963, judicial scholars published half a dozen papers that rekindled the interest in social background as an explanation for variation in the decisions of judges. That interest has continued to the present.

2. Jim Gibson has observed (in a letter to the author) that since the relationship between social background factors and justice behavior is mediated through judicial attitudes, the relationship between the two is always measured in attenuated form, and, because of indirect effects, may actually be stronger than the measurements indicate. Applying this thought to Tate's (1981) model underscores the remarkable results he achieved with seven predictors.

3. The choice of such a relatively long time span—66 years—was based on two considerations. First, the most successful attempt to explain Supreme Court decision making via the social attributes of the justices, that of Neal Tate, covered the most exten-

sive period to date—a span of 30 Court terms. Second, and more important, a period of 66 years enables us to investigate the potential of social attribute theory to account for behavioral variations over both short and long terms.

4. A mere glance at these tables makes it quite clear that support for government in each case varies dramatically across the justices taken in the aggregate. In Tables 1 and 2, the support percentages run from 18.6% (Goldberg) to 73.5% (Brandeis); 50.4% (Douglas) to 73.1% (White); 12.3% (Goldberg) to 92.9% (McKenna); and 26.7% (Murphy) to 75.6% (Minton).

5. Our four sets of cases are delineated in such a way as to control for certain possibly confounding factors. Set 1 consists of cases matching state or local governments against underdogs, while Set 2 makes the same match with the federal government as a litigant. Underdogs in these cases are litigants characterized as black, alien, criminal, subversive, or labor union. Set 3 matches federal government and non-underdog criminals—that is, criminals not characterized as black, alien, subversive, or labor union—while Set 4 makes the same match for state and local governments.

6. By using the ratio A/B as the criterion, we control to some extent for (1) external social and environmental influences playing upon the justices in a given era, (2) the legal strength of the case, and (3) the small-group influences emanating from the particular makeup of the Court at any particular time. Earlier studies have not employed such controls, thereby leaving unanswered questions about the impact of exogenous variables on levels of explained variance. The use of the ratio employed here should make it more difficult to obtain significant levels of explained variance; that is, to the extent that these control factors are related to the independent variables, the deck is somewhat stacked against affirming model predictions.

7. In his study of 25 justices over 32 terms in civil liberty cases, Neal Tate (1981) was able to account for 87% of the variance with seven variables. Our analysis of the Court's 1936–68 terms with only three predictors accounting for 72% of the variance is clearly in the same "ballpark." Given that our 33 terms overlapped 23 of the terms studied by Tate, and assuming that attitude toward civil liberty claims is the obverse of attitude toward governmental authority, it is possible that an application of our Model 3 to the 32 terms studied by Tate would produce results similar to those we have reported for the slightly different period. Such a finding would suggest that justices serving in this era were products of their social backgrounds, and that they were willing to allow the attitudes deriving from those backgrounds to affect their voting behavior significantly.

8. For a more extensive treatment of some of the matters discussed in this research note see Ulmer (1985).

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PARTISANSHIP AND GROUP SUPPORT OVER TIME: A MULTIVARIATE ANALYSIS

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We propose measuring group support for political parties by means of multivariate techniques that have become standard in other areas of political behavior. This approach yields improved insights into the marginal difference made by membership in each group and into the nature of a party's support coalition. As an example of this approach, we analyze the Democratic coalition since 1952. Our results differ from those of previous studies in a number of ways. Most significantly, differences with respect to the strength and timing of partisan changes lend support to Carmines and Stimson's conclusion that a realignment centering on race occurred in the mid-1960s. Our findings also indicate that the Democratic party is no longer so dependent on a few groups, as it was in the 1950s, but is now almost equally dependent on six groups.

Studies of changes in party coalitions (Axelrod, 1972, 1982; Petrocik, 1981) have focused either on overlapping groups, such as blacks and the working class, or on exclusive groups such as white, middle-class Protestants. Neither approach is adequate. Use of overlapping groups makes it impossible to tell which characteristics are the crucial determinants of party support. Reliance on exclusive groups largely predetermines the outcome, because the critical groups have to be defined a priori.

We propose a new and more revealing means of measuring group support. By analyzing the partisanship of individual

group members with multivariate methods similar to those applied elsewhere in the study of political behavior, we provide improved insights into the marginal difference made by membership in each group and into the makeup of a party's support coalition. As an example of our approach, we examine the Democratic coalition over the past three decades. Our results strengthen conventional wisdom about individual partisan changes by blacks and southern whites, but they differ significantly with respect to the strength and timing of changes by these and other groups. In addition, our findings lend support to Carmines and

Stimson's (1982, 1984) conclusion that a realignment centering on race occurred in the mid-1960s.

Party Identification: Measurements of Changes in Group Support

Multivariate analysis has become a common practice in studying most kinds of political behavior. The logic behind this development requires as well the adoption of a multivariate approach for the study of group support of the parties. Here we will rely on multivariate logit, because our dependent variable—claiming Democratic identification—is dichotomous, and because logit better reflects our theoretical expectations. With logit, the partisan impact of a group membership varies across individuals, depending upon the other group memberships of those individuals: a group membership has little impact on those who are otherwise almost certainly Democratic or extremely unlikely to be Democrats; it has its greatest effect on those in the middle.

In the present application we focus on partisanship over the period from 1952 to 1984, with special emphasis on the extent to which the New Deal coalition supporting the Democratic party has deteriorated. We concentrate on party identification as the more durable indicator of partisanship. Thus we estimated a total of nine equations—one for each of the nine presidential elections from 1952 to 1984.¹

In order to make the comparisons over time meaningful, the same exclusively group-based model is applied to each presidential election. The model includes the core elements of the New Deal coalition: native southern whites (11-state definition), blacks, Jews, the working class, union members (in the household), and Catholics. A variable for gender is also included. Other variables were considered in the initial analysis but were

dropped, because they had statistically insignificant relationships to Democratic partisanship.²

The Results

Overall and Incremental Effects of Group Memberships

Axelrod (1972, 1982) found that between 1952 and 1984 three groups had decreasing loyalty to the Democrats: Catholics, southerners, and individuals in households with union members. The poor, blacks, and those residing in the central cities of the nation's 12 largest metropolitan areas showed increasing Democratic loyalty. Petrocik (1981) reported that all groups except blacks shared a marked tendency toward independence and a diminished partisan preference; blacks became much more strongly Democratic.

Our results confirm the sharpest of these movements—especially of blacks and southern whites—but they differ in other respects. First of all, once other variables are taken into account, being poor and residing in metropolitan areas did not incline individuals toward Democratic identification (i.e., they did not achieve statistical significance) in the 1952-1984 period. The tendency of the poor and of metropolitan residents to support the Democrats can be accounted for by their other characteristics. Two indicators of status—education and income—were also not related to Democratic identification, perhaps because subjective identification with the working class better revealed the connections of status to partisanship.

Given the nonlinearity of logit, the original coefficients (not shown) are difficult to interpret. Transforming them into probability values, however, yields two summary values of special significance—the overall predicted mean probability of being a Democrat for each

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Table 1. Overall Probability of Democratic Identification for Group Members, 1952-1984

Group	Year								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
Black	.53	.50	.45	.79	.85	.68	.70	.70	.65
Catholic	.56	.51	.65	.61	.54	.50	.50	.42	.42
Jewish	.70	.60	.50	.52	.54	.52	.57	.69	.59
Female	.47	.43	.49	.59	.51	.43	.42	.42	.40
Native southern white	.75	.70	.72	.71	.52	.50	.52	.46	.42
Union household	.54	.50	.57	.67	.53	.45	.47	.44	.46
Working class	.54	.48	.51	.64	.55	.45	.46	.44	.42

Note: Cells are the mean of the predicted probabilities of Democratic identification for all group members in each year.

group, and the incremental probability difference each group membership makes.³

Table 1 presents the mean predicted probability that a group member would claim Democratic identification in each of the nine presidential election years—that is, the frequency of Democratic identification in each group before imposing any controls for other group memberships. Even these initial figures show the plight of the Democrats. Comparisons of the chronological endpoints, for example, show that only for blacks was the probability of Democratic identification significantly increased. White southerners registered the strongest decline, with most

of the decline coming in the 20-point drop between 1964 and 1968, and in the 10-point drop between 1976 and 1984. The probability of Democratic identification for Catholics, females, individuals in union member households, and self-identified members of the working class, although peaking in 1960 or 1964, later declined to levels below those of the 1950s.

The incremental impact of a particular group membership is isolated in Table 2. The strongest movements over the years are equally apparent in these figures. The black increase, the southern white decrease, and the Jewish decline and recovery are all evident in Table 2, and

Table 2. Incremental Probability of Democratic Identification for a Particular Group Membership, 1952-1984

Group	Year								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
Black	.17	.17	.09	.34	.47	.36	.40	.38	.35
Catholic	.20	.18	.29	.17	.17	.19	.20	.09	.12
Jewish	.38	.32	.18	.17	.23	.26	.34	.41	.34
Female	.00	-.03	.04	.04	.04	.06	.06	.08	.06
Native southern white	.43	.41	.41	.33	.17	.20	.24	.17	.13
Union household	.10	.11	.13	.14	.07	.07	.10	.08	.11
Working class	.12	.09	.09	.12	.09	.07	.08	.07	.06

Note: Cells are the average of the difference, for each group member, between the individual's predicted probability of Democratic identification and what the individual's probability would have been without the effect of the group membership.

are of about the same magnitude as in the overall results.

Significantly, however, the patterns are not identical to what has been observed using other approaches. Axelrod (1972), for example, found that in 1952 blacks were far more likely (by 38%) than the general population to vote for Democrats. That Democratic edge declined until 1964, when it rebounded to the same level as in 1952. Our results are not a direct contradiction, because they involve slightly different questions (i.e., the marginal impact and partisan identification), but they suggest that being black did not contribute strongly to being Democratic until 1964; before that, it had less marginal impact than, for example, being Catholic. In 1964 and later, however, the effect of being black contributed much more significantly to a Democratic identification than it had at any time in the 1950s.

Similarly, the impact of being Jewish declined between the 1950s and 1960s, but unlike Petrocik's (1981) trend, our results show a rebound, even by 1972, when his series ended. This trend continued through 1980, leaving the marginal impact very nearly what it was at the start.

Though smaller and therefore less noticeable, a contrast with earlier results is also apparent among Catholics. Axelrod (1972) and Petrocik (1981) found the contribution of Catholics declining by 1972 and 1968, respectively. Our data suggest that, except for the special circumstances of 1960, being Catholic stimulated a Democratic identification to about the same degree until 1980.⁴

The sharpest contrast suggested by our approach occurs for members of union households. Here both Axelrod's earlier results and our overall probabilities suggest a decline in Democratic proclivities of about five to eight percentage points. In contrast, the incremental push that comes from this characteristic appears to have changed very little over the last three decades. This difference may be iden-

tifiable because we are able to separate the effect of being working class from that of being in a union household. Since many union members consider themselves working class, and since there was a drop in the marginal impact of this variable, exclusion of the working class measure might erroneously indicate a decline in the effect of being in a union household.

Observe also the contrasting images created by the overall versus incremental probabilities for women. Females are less likely to identify themselves as Democrats in the 1980s than they were in the 1950s; at the same time, being female has given an increasingly larger boost to Democratic identification. Note, however, that this increment is not simply a Reagan phenomenon. Marginal female support for the Democrats goes all the way back to 1960 and has been slowly but steadily increasing since then. Interestingly, the only reversal in the pattern is in the latest Reagan election. It remains to be seen whether or not this counterintuitive change continues.

In addition to our ability to distinguish overall and marginal effects and to control more effectively for multiple characteristics, another way of using our multivariate approach is to ask whether changes in the mix of demographic characteristics have had a significant effect on the probability of claiming Democratic identification. It is possible, for example, that the decline in the number of females identifying with the working class (60% in 1952; 50% in 1984) and other such changes can largely account for the observed changes in tendencies towards Democratic partisanship. Reliance on either overlapping or exclusive groups cannot easily address such possibilities. However, there is a straightforward way to study this question. We can apply the coefficients from a later election year to an earlier distribution of individual group membership and contrast these hypothetical results with the actual ones for the

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later year. Such a comparison conveys the degree of partisan change arising from shifts in the coefficients (that is, the partisan meaning of group memberships). By similarly varying the distribution of group memberships, one can gauge the degree of partisan change arising from shifting mixes of individual group memberships.

The two sets of coefficients for the endpoints 1952 and 1984 capture the partisan meanings of group memberships for those two years. What happens if the patterns of meanings for 1984 are applied to the 1952 data? The overall probability of Democratic identification for the groups is not appreciably different, whether one applies the 1952 coefficients to 1952 or 1984 data. The changing composition of groups made for very little partisan change. However, major differences in probabilities result from applying the 1984 coefficients to the data from 1952. Ignoring the signs, the mean change in probabilities for varying the data distribution is less than two points, while for varying the coefficients it is 14 points. Thus the shifts in the meaning of group memberships far outweigh the partisan implications of demographic changes. Changes in the partisan meaning of group memberships, not declines in reinforcing group memberships, account for the decreasing probabilities of identifying with the Democrats.

Group Support and the Democratic Coalition

Axelrod's (1972) analysis reveals the extent to which the Democratic coalition is made up of blacks, metropolitan residents, and so on. If one wishes simply to know a group's share of the Democratic coalition, Axelrod's approach has much to recommend it. However, studying groups within party coalitions—just as studying individual voting behavior—does not require us to treat the groups as

if they were monolithic. Since most individuals have multiple group characteristics, removing a single characteristic from the mix need not mean that all members of that group desert the party. While Axelrod's findings reveal the coalition share each group composes, his figures cannot reveal, for example, how many southern whites would remain Democratic if being a southern white gave no nudge toward Democratic identification. Nor can they show what the size of the Democratic coalition would be if that Democratic propensity was lost. In contrast, our approach yields both of these results.

Such results are hypothetical, of course, but they are what the party strategist would most like to know. If the party stopped appealing to southern whites as such, what would that do to the party coalition? How many southern whites would remain Democratic despite the changing appeals, and how much of a loss would the party sustain?

Answers to these questions, along with comparison figures of the type that Axelrod generated, are given in Table 3. In the first two sections of the table we show the proportion of Democratic identifiers in the U.S. and, below that, the percentage of the Democratic coalition with a given group characteristic. This breakdown of the coalition is in terms of overlapping groups, making these percentages analogous to those presented for the Democratic vote by Axelrod. Not surprisingly (because of their size in the population), working class identifiers and females consistently make up the largest shares of Democratic identifiers. Blacks and Jews, despite their high Democratic loyalty, constituted the smallest shares of identifiers.

The results in the third section of the table show what would have happened to each group if the Democratic increment due to the group characteristic were removed. Females, for example, would

Table 3. Size and Composition of the Democratic Coalition, 1952-1984

	Year								
	1952	1956	1960	1964	1968	1972	1976	1980	1984
<i>Proportion of Democratic Identifiers in the U.S.</i>	47.3	43.7	47.3	52.9	45.5	40.3	39.5	39.3	38.0
<i>Percentage of Democratic Coalition with a Given Group Characteristic^a</i>									
Black	10.8	9.8	7.9	13.6	16.8	16.2	18.3	19.2	19.2
Catholic	26.1	24.8	27.3	25.2	26.3	29.7	31.5	25.0	31.0
Female	53.8	53.8	54.8	56.2	58.8	59.7	61.6	60.9	61.3
Jewish	4.8	4.3	3.4	2.8	3.3	2.9	3.5	5.8	4.1
Native southern white	25.6	27.4	27.3	20.3	18.7	20.7	20.1	19.4	20.8
Union household	31.5	31.7	33.1	29.6	28.6	28.9	27.6	29.3	25.7
Working class	70.5	69.0	72.0	65.8	62.9	61.1	61.5	58.5	57.0
<i>Percentage of Democratic Identifiers in Group Continuing to Claim Democratic Identification After Removing Democratic Tendency of Defining Group Characteristic^b</i>									
Black	68	67	81	56	44	46	43	46	46
Catholic	64	65	55	71	69	63	59	79	73
Female	100	106	92	94	93	87	85	82	86
Jewish	46	48	63	68	58	50	40	41	42
Native southern white	42	42	43	54	66	60	53	64	69
Union household	81	79	78	80	87	85	79	82	77
Working class	77	82	82	82	84	85	83	84	85
<i>Relative Size (%) of Democratic Coalition After Removing Group Characteristic</i>									
Black	96.6	96.8	98.5	94.1	90.5	91.3	89.4	89.6	89.7
Catholic	90.7	91.3	87.9	92.6	91.4	89.1	87.1	94.7	91.6
Female	100.0	103.4	94.8	96.0	95.2	92.3	90.6	88.8	91.3
Jewish	97.5	97.7	98.7	99.0	98.7	98.5	98.0	96.4	97.6
Native southern white	85.2	84.0	84.6	90.7	93.6	91.8	90.6	93.1	93.4
Union household	93.9	93.1	92.6	93.2	96.0	95.8	94.2	94.7	93.9
Working class	83.5	87.4	86.7	87.0	88.8	91.1	89.4	90.6	91.6

^aFigures derived from taking the mean predicted probability of Democratic identification for a group in a particular year multiplied by that group's number of respondents, and dividing this product by the number of Democratic identifiers.

^bFigures derived by recalculating the probabilities of Democratic identification without the effect of, say, working-class identification, then taking the mean of these probabilities for all respondents who claimed working-class status. The ratio of this revised mean probability to the mean probability that includes the effect of working class gives the ratio of the hypothetical size to the actual one.

have been the least affected. Despite the gender gap, Democratic appeals to women as such have been sufficiently few, and women's other characteristics sufficiently pro-Democratic, that even in 1984, 86% of the females who identified with the party would have continued to do so if there were no special push due to

their gender. In contrast, less than half of the blacks and Jews would have continued to identify with the party if it lost its special appeal to them as blacks and Jews.

What effect would such changes have had on the party coalition? The last section of Table 3 shows that the Democrats used to depend more variably and more

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heavily on appeals to certain characteristics. The loss of a given group increment would have reduced the coalition by as little as 2-3% or by as much as 16%. Recently, however, the overlap among group memberships and the Democratic tendency of each membership has been such that the loss of appeal to any of the characteristics would result in a rather similar 6-10% loss in identifiers. This change is significant, but it leaves the Democrats no less vulnerable than before. The party is no longer so dependent upon a couple of groups, as it was in the 1950s, but is now almost equally dependent upon six.

Conclusion

Previous studies of the group basis of partisan identification define groups in overlapping or exclusive terms, concealing rather than revealing the net partisan impacts of an individual's multiple group memberships. This study employed multivariate logit to assess partisan change, measuring the incremental impact of individual membership in selected groups on Democratic partisanship.

In part, the findings reported here strengthen current knowledge about shifting group loyalties. Being Jewish or being in a union household pushes an individual in the direction of being a Democrat as much as it did in the 1950s. Being black or being female gives a greater push now than either did three decades ago. Catholics, native southern whites, and working-class members receive a lessened stimulus toward Democratic identification from memberships in these groups.

The possibility that demographic change—reduced reinforcement of partisan tendencies from multiple, overlapping group memberships—figured prominently in the observed Democratic decline was tested and found wanting. Changes in the partisan meaning of group

memberships proved a far more potent link to the decline. We also noted that the contribution of the varying groups to the Democratic coalition was now highly similar, so much so that a loss of appeal to any one group would be a strong blow to an already weakened party.

In addition to strengthening current knowledge, the findings refine our understanding of the nature and timing of changes in Democratic support. The timing of changes by blacks and native southern whites—rapid shifts in the 1964-1968 period—suggest a racially-inspired shift in the group basis of the Democratic party consistent with Carmines and Stimson's (1982, 1984) thesis of a racial realignment (or at least a de-alignment) of the parties at that time.

Finally, in the light of the findings reported here, journalistic and party obituaries for the New Deal coalition appear harsher than the reality warrants. Native southern whites had a strong, prolonged decline in their incremental partisan impact, but the milder declines for most groups, coupled with the increases for females, Jews, and blacks, suggest the New Deal group basis of Democratic identification may not be dazzling, but is far from dead.

One reason for the resilience of the New Deal coalition may turn on the observation that the arrival of a new partisan order does not require the obliteration of the old ties. Sundquist (1983, p. 17) notes that even in a new realignment, the old realignment patterns endure—reduced, no doubt, but still detectable, with the realignment process resembling a "collage of successive overlays." The New Deal group cleavage may be of this sort; it has not disappeared, but it may have been superceded by other cleavages. Traditional group partisan differences still endure, but new ones are now as prominent, or more so. Groups such as feminists, evangelicals, environmentalists, residents of the Sunbelt and Frostbelt, "yuppies,"

Hispanics, and workers in high-tech or smokestack industries suggest clusters of voters whose loyalties might now be eagerly sought. As for the New Deal coalition itself, it is clearly smaller than in the past. However, if the New Deal coalition is not alive and well, at least its corpse has yet to disappear.

Notes

An earlier version of this paper was presented at the 1985 annual meeting of the American Political Science Association, New Orleans, LA. We would like to thank Thad Brown for helpful comments.

1. Altering the definition of Democratic partisanship to include leaners does not change any of our conclusions. When the same model is examined using Democratic presidential voting as the dependent variable, there are strong short-term fluctuations, but the trends essentially resemble those found for partisanship.

2. These were age, income, education, metropolitan residence, white migrants into the South, rural residence, blue-collar workers, white-collar workers, farmers, Protestant, Irish or Polish descent, and foreign-born parents. Multicollinearity had little effect on which variables remained in the equation. In fact, two of the most collinear variables, working class identification and union household, both remained. Missing data (primarily "not ascertained" cases) were excluded.

3. For the first value, we began by calculating a predicted probability for each individual. This required applying the parameter estimates for each variable to data values for each individual. The predicted probability for a group in a given year is

then the average of the predicted probabilities for all group members in that year. Similarly, the incremental difference is the average of the difference, for each group member, between the individual's predicted probability and what the individual's probability would have been without the effect of the group membership.

4. Logit also allows us to identify significant interaction effects among the independent variables. One such effect contrasts with Petrocik's (1981, pp. 84-85) results. Native southern whites of working-class status had a 34% decline in Democratic partisanship, surpassing the 30% decline by southern, non-working-class whites.

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REVIEW ESSAYS

A World of Secrets: The Uses and Limits of Intelligence. By Walter Laqueur. (New York: Basic Books, 1985. Pp. xii + 404. \$21.95.)

Regulating U.S. Intelligence Operations: A Study in Definition of the National Interest. By John M. Oseth. (Lexington: University Press of Kentucky, 1985. Pp. xvii + 236. \$24.00.)

A Season of Inquiry: The Senate Intelligence Investigation. By Loch K. Johnson. (Lexington: University Press of Kentucky, 1985. Pp. 317. \$31.00.)

Secrecy and Democracy: The CIA in Transition. By Stansfield Turner. (Boston: Houghton Mifflin, 1985. Pp. xii + 304. \$16.95.)

Intelligence Policy and Process. Edited by Alfred C. Maurer, Marion D. Tunstall, and James M. Keagle. (Boulder, CO: Westview Press, 1985. Pp. viii + 399. \$20.00.)

Knowing One's Enemies: Intelligence Assessments Before the Two World Wars. Edited by Ernest R. May. (Princeton: Princeton University Press, 1984. Pp. xiii + 561. \$29.50.)

The Missing Dimension: Governments and Intelligence Communities in the Twentieth Century. Edited by Christopher Andrew and David Dilks. (Champaign-Urbana: University of Illinois Press, 1984. Pp. vi + 300. \$27.95.)

Domestic Intelligence and Intelligence and Policy. Vols. 6 and 7 of *Intelligence Requirements for the 1980s*. Edited by Roy Godson. (Lexington, MA: Lexington Books, 1986. Vol. 6; Pp. xiv + 192. \$14.95. Vol. 7. Pp. xii + 290. \$14.95.)

No theoretical issue is more important and more difficult than the relationship of information and action: important, because knowledge is potential power and politics is about power, difficult, because analysis of the knowledge-action nexus requires understand-

ing of cognition, perception, and communications flow in political systems.

This generation of political scientists has seen, if darkly, the growth of a mammoth intelligence bureaucracy designed to supply policy makers with information and, ideally, foreknowledge about foreign affairs. Yet the relationship of knowledge to action has remained obscure. This is because of intelligence secrecy and the difficulty of measuring, in an elaborate multiorganizational process, the influence of the intelligence product on policy and action.

Recently there has been an outpouring of a variety of publications promising to improve our understanding of the role intelligence plays in foreign policy decisions. This essay focuses primarily on those works that attempt to analyze objectively problems of strategic intelligence policy, organization, influence, and democratic accountability.

Walter Laqueur's *A World of Secrets* is the most significant book since Sherman Kent's *Strategic Intelligence for American World Policy* (Princeton: Princeton University Press, 1949) and Roger Hilsman's *Strategic Intelligence and National Decisions* (Glencoe, IL: The Free Press, 1956). Kent's work was published at the dawn of the age of central intelligence, and is a descriptive and prescriptive analysis of intelligence doctrine that had great influence on the evolution of American intelligence professionalism. Hilsman relied heavily on interviews with producers and consumers, and analyzed the application of intelligence doctrine in the new, post-World War II intelligence system.

Laqueur's book represents a massive team assault on these enduring topics: intelligence doctrine, failure, influence on policy, process, secrecy, and democratic accountability. The book is the product of a research undertaking aided by the Twentieth Century Fund. This permitted the principal author, Laqueur, to organize a group of Washington-based

assistants who, one can infer, drafted a number of the chapters. In his preface, the author mentions six drafting coauthors by name. Interviews were held with unnamed Directors of Central Intelligence, Secretaries of State and Defense, and National Security Advisers. The total persons consulted numbered "more than two hundred people in half a dozen countries" (p. xii). Although many of these sources are not specified in the endnotes, the book contains 47 pages of footnote citations. No other work on the subject represents such wide coverage of sources. Here is a work of large-scale "academic intelligence" seeking out the bits and pieces of an ostensibly secret subject and attempting to fit them together in a meaningful mosaic.

After 40 years the central intelligence role has remained obscure. To some, the model is that "intelligence guides policy," in that policy makers precisely informed of external realities can maximize utility in rational choice among policy options. To others, the intelligence apparatus that has evolved since 1947 represents a politicized "invisible government" dominating policy. Laqueur seeks answers to such questions as, What role has intelligence played in foreign policy making? How accurate has the intelligence product been? How well has intelligence been communicated? What are the inherent limits of intelligence estimates? How can intelligence be improved? Are there special problems for secret intelligence agencies in a democratic framework?

In part 1 of the book, Laqueur analyzes the production of intelligence, giving particular attention to economic and scientific intelligence. In this section he describes the intelligence process, and it is replete with gratuitous comments about that process. For example, he writes that "intelligence production, quality and consumer demand varies [sic] enormously according to the political regime under which these take place" (p. 16)—a significant point, but inadequately developed.

Part 2 is the book's most important contribution, essaying an evaluation of intelligence performance since World War II. New insights are offered into the ways in which various presidents viewed and used intelligence, into relationships between presidents and CIA directors, and into the bureaucratic politics of intelligence. However, this part of the book has a gossipy quality and

contains some patently inaccurate remarks, such as the assertion that the American "left" (undefined) "opposed the very existence of intelligence" (p. 85). At another point the author says of Vice-President Mondale and David Aaron of the national security staff that their "enthusiasm for intelligence was limited" (pp. 85-86). At another point it is asserted that "intelligence was not considered important, but a source of potential trouble" in the first three years of the Carter Administration (p. 87). Such comments suggest either a bias or that Laqueur's sources were overweighted on the side of the professional intelligence lobby. Nonetheless, the author offers some insightful and useful comments on such important topics as information pathologies within bureaucracies, the compartmentalization of knowledge, the phenomenon of data overload, pressures for conformity, and other organizational factors that shape the quality of intelligence.

Laqueur then turns to concrete examples of intelligence performance. Although no definitive "case study" of any of the events described is presented, the quality of intelligence estimates is reviewed from the earliest days of United States central intelligence. Here Laqueur is able to utilize recently declassified estimates as well as interviews and secondary sources. Special and detailed attention is given to the "Missile Gap" controversy, the Cuban missile crisis, the Vietnam War, and estimates of Soviet missile strength. He uses these cases to demonstrate that intelligence forecasts were sometimes right, sometimes wrong. He also derives from this evidence support for the book's theme: that intelligence estimates "seldom had a decisive influence on the conduct of U.S. foreign policy" (p. 197). In this view, "intelligence can do no more than serve as a useful guide, a compass in a complicated world" (p. 198). Readers may experience some frustration with such ambiguous conclusions. The problem is that Laqueur, like others of us, has difficulty in measuring the influence of intelligence. This may be because many of the secrets are withheld from us long after events have passed.

Part 3 is a somewhat superficial survey of how other democratic nations, specifically Great Britain, West Germany, and Israel, have dealt with the dilemmas of secret intelligence

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services. The relevance to the book's theme is not clear, because the author concedes at the outset that the United States is different in important respects from the three nations compared. However, a chapter on the Soviet Union focusing on the KGB and the GRU (military intelligence) has substance. Here again, the author observes that the CIA and KGB are not comparable, either in function or size. After describing in outline the organization of Soviet intelligence, the author concludes—without supporting evidence—that “the overall record of Soviet intelligence achievements has been excellent” (p. 243). He suggests that any failures have not been due to ideological blindness, but to lack of knowledge of the outside world, inadequate understanding of foreign political systems, and belief in stereotypes. In this respect, it is not clear how the Soviets differ fundamentally from the West.

Part 4, ambitiously titled “Theories of Intelligence,” deals with the causes of intelligence failure and examines the question of whether intelligence is craft or science. He catalogues nicely the various “causes” of failure, such as surprise, bias, ignorance, and deception, but this section is disappointingly lacking in hypotheses, and concludes on the vague suggestion that even though “major mistakes should not often occur” (p. 291), intelligence is ultimately an art rather than a science.

The concluding section, part 5, addresses the future of intelligence. What do all of the organizational history, the bureaucratic gossip, the illustrative examples, and the comparative material add up to? At one point in the book, when he is evaluating intelligence performance in Vietnam, the author accuses the CIA of using “Delphic language” in intelligence estimates. Ironically, *A World of Secrets* has its own oracular quality that leaves the reader uncertain as to the answer to the book's central questions: the impact of intelligence on policy, the causes of intelligence failure, and prospects for intelligence improvements.

On the one hand, one reads that intelligence has “become an essential part of the process in making national security decisions,” and “once the policy process is under way, intelligence becomes essential to the government's pool of information,” and “no foreign policy official would want to be without intelligence report-

ing today” (p. 108). On the other hand, the author concludes that “intelligence was never as important in the conduct of policy as is commonly believed, nor is it ever likely to be” (p. 324). Furthermore, intelligence “is an important element in the decision-making process, but only one element” (p. 344), although at the outset of the book, the author states that “no one claims that intelligence has been of major importance in the conduct of the affairs of state” and that “intelligence is not held in very high esteem by those in high places” (p. 4).

Those of us who have studied these questions for many years can sympathize with the difficulty in measuring precisely the relationship of intelligence and policy. With his Delphic pronouncements, Laqueur leaves this fundamental question open. He observes that intelligence is a “prerequisite” for, but “can never be a substitute for policy or strategy, for political wisdom or military power” (p. 338). This begs the question, because we still need to understand how intelligence contributes to such “wisdom,” strategy, or the difficult choices regarding military force.

Laqueur's judgments on more specific issues are characterized by less ambiguity. He is skeptical about the value of basic intelligence organization reform, doubts that high technology offers solutions to many analytic challenges, and believes that better performance will result from better recruiting and the use of nongovernmental experts. He concedes a contradiction between free societies and secret intelligence, is wary of separating covert political intervention from the intelligence function, and is ambivalent about the effectiveness of the existing systems of congressional oversight.

One finds no sustained theme or consistent argument, no tested hypotheses, and essentially little encouragement for the development of intelligence theory. One does find an evaluative study of substance that covers most of the important questions that have been raised with regard to intelligence over the past four decades. While the book has its share of obtuse statements and errors, it deserves—primarily because of its impressive coverage of a variety of sources on the subject—to be ranked with the relatively small body of significant descriptive literature in a burgeoning field.

John M. Oseth's *Regulating U.S. Intelligence*

Operations addresses intelligence issues in the context of definitions of national interest. He stresses issues of policy, organization, and accountability, and analyzes the conflicting requirements of intelligence secrecy and public accountability. Unlike Laqueur, Oseth is more concerned with rules, organization, and accountability than with intelligence performance and impact on policy. He focuses on the turbulent period between 1974 and 1984, when the intelligence system was in a crisis of legitimacy and the issue of secrecy democracy was being sharply raised. He is concerned with the important question of how the intelligence function interacts with American political culture, and he explores the conflict between American ideals and the pursuit of external national interests, a subject too long neglected.

Oseth's method is primarily historical. He has combed the published material on the subject with the thoroughness characterizing a first-rate doctoral dissertation. His 39 pages of endnotes and a selected bibliography indicate an exhaustive survey of the relevant literature. He is searching for a better understanding of the substantive points at issue in the intelligence debate over the past decade, of the policy outcomes, and what the debate and outcomes tell us about national self-image and purpose.

This study reviews in detail the Ford, Carter, and Reagan administrations' handling of these issues. He summarizes the debates, among scholars, among intelligence professionals, in the media, and in Congress. The author offers no dogmatic position on the central issues: executive discretion in secret operations, congressional oversight, disclosure vs. secrecy, idealism vs. realism in foreign policy, the Bill of Rights threats to security. Oseth concludes that while intelligence law, organization, and procedures need refinement, new laws will matter less to the satisfactory conciliation of democratic values with security priorities than the "strength and character in our officials" (p. 187). This would seem to equivocate on some of the fundamental issues, but like many observers, he has little faith in new laws and reorganization as solutions.

In sum, Oseth has provided a fine catalogue of the issues surrounding a secret service in a democracy. His book is primarily a descriptive and evaluative account of the intelligence issue over the past 10 years. Anyone intending to

pursue a more empirical study of this subject, or wanting a competent synthesis of intelligence events and issues from 1974 to 1984, will find this study required reading.

Loch K. Johnson's *A Season of Inquiry*, is an analysis of the internal politics of the U.S. Senate Select Committee to Study Governmental Operations with Respect to Intelligence. This committee, led by Senator Frank Church of Idaho, was the Senate's response to reevaluations in 1974 that the CIA and FBI and other units of the "intelligence community" had been operating illegally and with little congressional monitoring.

Johnson, a former Congressional Fellow of the American Political Science Association, has written a unique study. His dual concern is intelligence policy and congressional behavior in a time of crisis that cast doubt on the legitimacy of the intelligence system, raised concern about the abuse of presidential power, and produced deep disagreements within Congress about its appropriate role and about procedures with regard to the government's secret intelligence operations.

The author writes from the vantage point of a scholar who has specialized in Congress, as a student of American politics, and as a major participant. He was a staff member of the Church Committee, serving also as the chairman's personal assistant during the sixteen-month Senate investigation. Later he was a staff director of the House Intelligence Committee, which gave him a broader perspective on the congressional role. As a political scientist retrospectively analyzing a process in which he was an actor, Johnson ran the risk of bias. He has overcome this problem, for his book is an objective account of the forces at work in a major Senate investigation of sensitive government policy and operations. Many persons regard the 1975 Senate probe of U.S. intelligence agencies as a circus at best, and a destructive witch hunt at worst. Former President Nixon refers to "the castration of the CIA" and, like others, appears to blame the congressional probes of 1974-1976 for emasculating the intelligence system. Johnson's book should help to dispel this mythology. It offers a balanced view of the "Year of Intelligence" in which both houses of Congress, responding to widespread popular concern, turned a bright and sometimes errant searchlight on intelligence.

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Most of the book is a personal memoir, written in the first person, of the stormy life of the Senate Committee. He describes its internal staff politics, its debates over roles and procedures, its conflicts with the CIA and the presidency, and its reflection of the pluralistic nature of American politics. This book brings a realism to a congressional probe rarely offered by authors of legislative case studies. This is the story of how the Senate eventually came to have, a permanent Select Committee on Intelligence, yet failed to agree on a "charter" for the intelligence agencies that spells out the "dos" and "don'ts" for the intelligence system. The book offers a rare insight into the many variables that contribute to congressional investigative behavior.

In *Secrecy and Democracy*, Stansfield Turner approaches the intelligence dilemma from the viewpoint of a professional naval officer, former Rhodes scholar, former head of the Naval War College, and former director of Central Intelligence (1977-1981). He came unexpectedly and reluctantly to head the CIA. He discovered immediately that the intelligence "community" was a highly charged political arena—not a community, but an assortment of competing entities. Indeed, he discovered that even the CIA itself lacked unity and coherence, and in fact was "three separate organizations operating almost with autonomy" (p. 185). He is referring to the three operating branches of the CIA: espionage (and covert political action), scientific and technical development, and research and analysis. In becoming the CIA's director, Turner discovered that it was out of control, at least by his standards of military or corporate command and control.

Turner quickly learned that (a) the espionage branch was the elite group with the greatest clout among the three CIA branches, and (b) each of the branches operated with a high degree of autonomy and was over-staffed and strongly resistant to centralized direction. The "rogue elephant" had three heads. Turner and his aides attempted, in his four years as director, "to bring the three-headed colossus of the CIA under centralized control" (p. 193). He restructured the organization, hoping to improve personnel, budgets, and plans; he placed his own appointees in key spots; and he strove to improve communications within the agency. Time ran out before he felt a sense of

accomplishment, but he reports that when his tenure as director ended in 1981, "I had no fear that the organization was operating behind my back or hiding things from me" (p. 194). He adds, however, that "being confident that the organization was not out of control was not the same as feeling that it was adequately under control" (p. 194). Turner left the directorship clearly unsatisfied with the agency's personnel procedures and with the outcome of his bureaucratic struggle to control the "three CIAs."

Only in America could, or would, a recent intelligence chief write such a startlingly candid book. Turner had difficulty getting CIA clearance for his manuscript. He was required by the agency to make more than 100 deletions on security grounds. Even so, Turner's book is the most authoritative and frank inside look at the CIA's bureaucratic culture, warts and all, ever published. Discounting his military command approach and the self-justifying aspects of any such memoir, Turner offers observers of intelligence exceptionally revealing testimony. There is insight here also for those interested in public policy, administrative management, and bureaucratic behavior. This candid (Turner admits some mistakes) and critical inside look at the heart of the nation's intelligence apparatus is not reassuring. The causes of many intelligence failures and inefficiencies become apparent. Regrettably, we cannot see the manuscript uncensored by the CIA.

Turner concludes with a catalogue of intelligence reforms that he feels are needed to equip intelligence better for its multiple functions. For example, he supports a revised comprehensive legislative charter for the intelligence system, favors effective congressional oversight, would have a Director of Central Intelligence separate from the CIA head, would combine the CIA's analytic and espionage branches, reduce the emphasis on covert political intervention, and depoliticize the director's role by appointment of a politically neutral person.

I have advocated most of these reforms for many years, and welcome the judgment of a former CIA director. Turner has faith that it is possible to reconcile "the necessary secrecy of intelligence to the democratic processes on which our government is founded" (p. 285). Whether such faith is justified remains a serious question.

Other works published recently add insight into issues of intelligence policy, organization, functions, process, performance, and accountability. *Intelligence Policy and Process*, edited by Alfred C. Maurer, Marion D. Tunstall, and James M. Keagle, is a collection of 20 essays by intelligence specialists, academic as well as practitioners. It probes the many aspects of the relationship of intelligence and policy. The book is the outgrowth of a conference held by the Political Science Department of the U.S. Air Force Academy in June 1984. The collection has been assembled with thoughtful attention to the problems of theory. Its design is coherent, even though its generally sophisticated essays lack thematic unity, as is common in such compendiums. An appended twenty-four-page bibliography and a glossary make the book a useful teaching instrument.

Two volumes of historical essays also deserve attention. Definitive studies of intelligence performance are most likely to be found in historical monographs. In *Knowing One's Enemies*, Ernest R. May has assembled an outstanding collection of case studies, divided into three sections: (1) World War I; (2) World War II in Western Europe; and (3) "Eastern Europe and Beyond" in the Second World War. These 16 monographs analyze what the various participating governments knew and when they knew it, as well as the policy consequences of the quality of the information. In a masterful summary essay entitled "Capabilities and Proclivities," May offers no simplified answers. In essence, the historical record is replete with tragic intelligence failures. What are the lessons implied? They include the following: Organizational change rarely brought intelligence success. Long-range intelligence projections were commonly inaccurate. Policies have failed when governments have attempted to see things from the standpoint of adversary governments. Governments have often identified the wrong enemy. Widely accepted presumptions were "often quite wrong" (p. 540), which is to say that strong biases dangerously pervade governmental intelligence and decision-making structures. More recent experience suggests that most of these lessons remain unlearned.

Andrew and Dilks have assembled, in *The Missing Dimension*, 11 essays that analyze a variety of intelligence-related subjects, with emphasis on the British experience. Topics

range from such subjects as "Japanese Intelligence and the Approach of the Russo-Japanese War" to "The Cambridge Comintern." This volume concerns itself with intelligence in foreign policy decisions and also with counterintelligence. This eclectic volume has no dominant theme. Together its essays demonstrate, in the words of the book's editors, "the gradual and erratic professionalization of intelligence communities in the West, and the equally gradual and erratic way in which governments have learned to cope with them" (p. 6). *The Missing Dimension* serves as a valuable supplement to *Knowing One's Enemies*.

Another valuable resource on this subject is the impressive, seven-volume series, *Intelligence Requirements for the 1980s*, edited by Roy Godson, Volumes 6 and 7 have recently been published. Volume 6 is entitled *Domestic Intelligence* and Volume 7, *Intelligence and Policy*. The format of these volumes is that of conference proceedings, with formal papers followed by brief summaries of discussions. The product is a provocative analysis of many aspects of the intelligence system. The primary weakness of these useful collections is that the views expressed are heavily weighted to the right of center on the ideological spectrum, which excludes many significant contrary views. The series is organized by the Washington-based Consortium for the Study of Intelligence.

To read Laqueur is to be better informed about the limits of intelligence and the variety of information pathologies that debilitate decision making. Oseth's work teaches about the American ambivalence regarding secret operations that has produced ambiguous intelligence policy. From Johnson we learn about the inner workings of a Senate committee staff and the prevalent confusion of Congress regarding its role in intelligence accountability. Turner offers us chapter and verse about how an entrenched secret bureaucracy can successfully resist responsible leadership. The many other writers and observers in the volumes cited bring revealing light to the dark corners of government and to intelligence history.

Patently an ample bibliography now can support widespread curricular and research attention to intelligence. Theory remains weak and tentative, and the secrecy of evidence remains a formidable problem if one wishes to go

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beyond historical and evaluative studies, but absence of information, ideas, and opinions is no longer an excuse to neglect intelligence as a subject for research and teaching.

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Equality and Education: Federal Civil Rights Enforcement in the New York City School System. By Michael A. Rebell and Arthur R. Block. (Princeton: Princeton University Press, 1985. Pp. x + 329. \$28.50.)

The Burden of Busing: The Politics of Desegregation in Nashville, Tennessee. By Richard A. Pride and J. David Woodward. (Knoxville: The University of Tennessee Press, 1985. Pp. xii + 286. \$24.95.)

A Semblance of Justice: St. Louis School Desegregation and Order in Urban America. By Daniel J. Monti. (Columbia: University of Missouri Press, 1985. Pp. xiv + 207. \$24.00.)

Metropolitan Desegregation. Edited by Robert L. Green. (New York: Plenum Press, 1985. Pp. xi + 227. \$34.50.)

The authors of these new books on school desegregation differ in their conceptions of equalitarianism. One theoretical approach emphasizes the degree of equality of opportunity that would be afforded if the courts limited their role in desegregation cases to ensuring the removal of all vestiges of segregation mandated by law. This definition implicitly posits that although all students, regardless of race or other characteristics, should have full opportunity to "swim or sink," there is no United States constitutional imperative for racial integration of pupils in classrooms. Also implicit is the need for incentives if this approach is to result in quality education for all.

The second theoretical approach to equality emphasizes structural changes to produce integrated classrooms with equal learning situations, which are considered essential to achieve equality of education. The public choice theory is rejected because freedom of choice is viewed as contributing to school segregation, which must be replaced by a mandatory diaspora of black students via busing, since blacks in black classrooms will not learn as much as they would if placed in integrated classrooms.

Standardized test scores generally support this premise, but one must question the validity of these test results because of the failure to factor into the scores the quality of teaching and facilities, pupil transiency resulting in absenteeism and a need to adjust to new surroundings, tardiness, and the degree of parental support or apathy.

It also is an unstated assumption of supporters of this second school of thought that blacks will receive equal opportunity only if they are placed in the same classrooms with whites, because the white ruling majority will not adequately fund black schools. Regardless of whether this assumption is valid, evidence is clear that "white flight," caused by mandatory integration and/or other factors, has resulted in dwindling white support for public schools, which are becoming increasingly black in large cities.

The books under consideration here directly or indirectly raise the question of the adequacy of the courts as Platonic guardians of public schools capable of Solomon-type decisions. *Brown v. Board of Education* reflected the first theoretical approach while hinting at the second, which the Supreme Court subsequently adopted in its decision in *Swann v. Charlotte-Mecklenberg Board of Education*, and then retreated from in *Milliken v. Bradley*.

Attorneys Rebell and Block have written the superior case study among these four books. *Equality and Education* is an insightful examination of the complex issue of school desegregation from the standpoint of political and organizational theory. This book, which focuses upon discrimination in teacher hiring and student services, should be read first, as it provides a comprehensive theoretical underpinning not only for their study, but also for other studies. They maintain that Congress by choice did not define the term *discrimination* in the Civil Rights Act of 1964, thereby ensuring conflict over the meaning of the term, which program administrators and courts would be called upon to define.

Equality and Education is unique among the four books in examining interest groups in school desegregation on other than a "protest" basis. Rebell and Block focus on the Federal Office for Civil Rights' scrutiny of the New York City school system for compliance with the Civil Rights Act of 1964. In the process, they also refer to similar studies done in

Chicago, Philadelphia, and Los Angeles, which they say were part of President Nixon's strategy to win the South and hurt the Democratic machines in these four large cities.

The political power of Albert Shanker, president of the United Federation of Teachers in New York City, is made apparent by the voluntary nature of the settlement between the city and the federal office, which contrasts with the mandatory provisions in the settlements in the other three cities. On the other hand, citizens have limited ability to understand the judicial labyrinth and the subtle issues involved in school desegregation cases, and to hold public officials—including judges—responsible. A major conclusion of the authors is that while administrative agencies have a greater fact-finding capacity than legislatures or the courts, the courts are more effective in monitoring compliance with agreements produced by administrative agencies.

It is somewhat surprising that Rebell and Block did not expand upon their reference to the neighborhood government and related black separation movements of the 1960s. The situation in New York City in the late 1960s was explosive, and the 1969 New York State law establishing community school districts with boards elected by proportional representation (PR) helped to defuse the situation. PR has provided representation in proportion to the turnout of voters by various groups, and has helped to satisfy black demands for control of schools in their neighborhoods.

In *The Burden of Busing*, Pride and Woodard attribute the decision to employ busing to the proposition that there is one dominant culture in the United States, and black children in segregated schools were being excluded from assimilation into this culture. They write: "it remained only for black children to be made into white children with dark skin" (pp. 279-80). In addition, they maintain that black pupils still are stigmatized by lower scores on national achievement tests, and that a number of black leaders are questioning the value of busing. They attribute white flight to a perceived loss of status by whites if their children are forced to attend school with black children.

According to *The Burden of Busing*, educational resources in Nashville could have been distributed equally among all schools without

busing, but the latter "was believed necessary for the reallocation of prestige, respect, and deference within the community" (p. 281). The authors attack busing because "it reinforces racial thinking. . . . Busing divided the preoccupied parents" (pp. 283-84).

Monti, who in *A Semblance of Justice* remains committed to the ideals of the integrationist movement of the 1960s, views the school desegregation movement in St. Louis as part of reform efforts to assimilate a diverse urban population and maintain order, a view somewhat similar to the one expressed by Pride and Woodard. The desegregation effort, in his opinion, is "a ritual that created only the illusion of change" (p. 182).

It is puzzling that a book could be written on the subject of St. Louis School desegregation without a complete examination of the briefs filed in the various court cases and a detailed examination of the court orders. Only one court decision is given a specific citation and the *St. Louis Post-Dispatch* is relied upon heavily for information. This sociological description and analysis of the school desegregation controversy will disappoint political scientists interested in public law and the political dynamics of such a situation.

Convinced that the desegregation campaign has benefited professional educators by increasing their power, Monti asks: "Why do we leave the administration of reform to the very people responsible for creating or perpetuating 'constitutional wrongs' in the first place?" (p. 185). His solution is radical: courts should dissolve boards of education. The result would be that "suddenly, school administrators would have nothing to shield them from an aroused public" (p. 185). Citizens, however, would be afforded less opportunity to make input into the educational system and to hold it responsible for its actions.

The most telling section of Monti's book is the foreword by Derrick A. Bell, a convert to an antipodal view on busing:

Even the most committed advocates of integration must now concede what was an all-out court campaign to desegregate the public schools has degenerated into a scatter-gun of still-litigated cases, none of which is likely to alter the status of most black children who continue to receive inferior and inadequate schooling in facilities

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generally as separate in fact as they were once segregated in law. (p. vii)

His solution is to bring "effective schooling to black children in black schools" (p. vii).

Metropolitan Desegregation is the product of a five-year study of desegregation in New Castle County, Delaware, by researchers from the Urban Affairs Programs of Michigan State University. Green sets the tone for the anthology by writing *ad judicium*, "the United States education system prepares many minority group Americans only for menial labor, the military, or unemployment" (p. 3). He is convinced that residential segregation in the northern states is the product of "willful acts of public officials and others operating under official sanction" (p. 9). Green apparently discounts evidence revealing that many minority group members desire to reside in areas where fellow members reside—be they Italian, Irish, Chinese, or black—for reasons of affinity, kinship, and/or support.

Green's bias shows in the adjectives he uses relative to the decisions in the Detroit school desegregation case, *Bradley v. Milliken*. Compare "the district court based its reasoning on sound legal principles" (p. 20) with "the Supreme Court rejected this line of reasoning, relying in particular on its belief" (p. 20). He urges school desegregation on a metropolitan area basis, supports his views on the degree of public opposition to desegregation by specifically citing public opinion polls conducted by a pro-desegregation citizens' commission, and holds the media chiefly responsible for the existing public opposition to desegregation efforts.

The majority of the essays follow Green's lead. Cassandra A. Simons concludes that the grouping of many minority children as "slow learners" "is evidence of the lingering effects of segregated schools' inferior education" (p. 73), but provides no empirical evidence. Meyer Weinberg comments, without supporting evidence, that "desegregation studies of achievement have been on a par with other customary inquiries" (p. 145). John H. Schweitzer, however, wisely writes that "the generalizability of the findings of this study is limited by the unique aspects of the school desegregation situation in New Castle County" (p. 184).

With respect to the role of the judiciary, the

conclusion is inescapable that the adversarial system of gathering information employed by the courts unfortunately does not produce accurate and complete information upon which to base judicial orders, since the evidence presented to the court often is incomplete and contradictory. The welfare of the students appears to be of little genuine concern to many plaintiffs. The courts embarked on remedying what they perceived to be wrongs in past segregation laws without a full understanding of the monumental social problems reflected in the low scores of many inner-city pupils on standardized tests. These problems cannot be solved by a judicial busing fiat.

The court-prescribed remedy—busing—to achieving "racial balance" in schools in large cities is impossible to accomplish, because of the sharply increasing black student population and the great mobility of poor families. Busing black pupils from a neighborhood school populated primarily by black students to another school where the black percentage is slightly lower in the name of racial desegregation is not sound public policy. The relatively large cost—money and time of pupils, administrators, and litigants—involved in attempting to achieve "racial balance" in schools through busing raises a question of the cost-effectiveness of this approach to guaranteeing quality education for all pupils. In general, these four books reveal that the problem-solving ability of courts is limited, and that judicial omnipotence in cases involving the Fourteenth Amendment does not ensure the superiority of the wisdom of a United States District Court to that of elected officials. A more flexible approach to improving the quality of education provided by big-city schools—the approach adopted in New York City is a possible example—is essential to ensuring all students an equal opportunity to gain a quality education.

Consolidation of school districts in the St. Louis area and New Castle County further impersonalized the system by increasing the distance between parents and the school board by "light years," probably contributed to "white flight," and resulted in less public support for the school system. In New York City, on the contrary, the unitary school system was opened up to wider citizen participation through the creation of 32 new community school districts with a total of 288 elected

school board members, which brought decision making on many educational issues down to the neighborhood level.

What has been the impact of mandatory two-way busing of pupils on race relations? Some evidence suggests that hostility between some whites and blacks has been increased by mandatory busing. This hostility can be manifested most effectively in cities where there is a legal requirement for a referendum for the levying of taxes or issuance of school bonds. New Castle County voters in October 1980, in the first tax referendum since the school consolidation, rejected a school tax proposal. On November 5, 1985, voters in the St. Louis City School District narrowly rejected, for the third consecutive year, a bond issue to renovate decrepit school buildings occupied predominantly by black students. Missouri law requires a two-thirds majority affirmative referendum vote for issuance of bonds. Observers attributed the defeat of the bond proposition to white voter discontent. The United States Supreme Court, in 1971 (*Lance v. Board of Education*, 91 S. Ct. 1889), refused to extend its "one-person, one-vote" dictum to extra-majority referendum requirements. Will the court reexamine its *Lance* decision in the light of the experience in St. Louis and similar experiences that may occur elsewhere?

The increasing disillusionment of a number of black civil rights leaders with school busing may herald the filing of fewer suits in the United States District Court that seek to promote equality of education through mandatory busing, given the reality of "white flight" from the public school system. In addition, the increasing black population of many central cities has resulted in political power passing from white control to black control. As blacks gain control of school systems, they will have less interest in court intervention to promote the ideal of equality, unless such intervention will result in increased financial support for the central city schools.

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Just Health Care. By Norman Daniels. (Cambridge: Cambridge University Press, 1985. Pp. xiii + 245. \$32.50, cloth; \$9.95, paper.)

With Dignity: The Search for Medicare and Medicaid. By Sheri I. David. (Westport, CT: Greenwood Press, 1985. Pp. xiv + 194. \$27.95.)

Political Analysis and American Medical Care: Essays. By Theodore R. Marmor. (Cambridge: Cambridge University Press, 1983. Pp. xix + 268. \$12.95, paper.)

These books suggest how the study of health politics may have to change as the problems in health policy change. The challenge today is not so much to realize policy ideals or enact new programs as it is to control an existing health establishment.

Norman Daniels' *Just Health Care* asks what principles should govern the distribution of health services. His standard, following John Rawls, is "fair equality of opportunity." The distribution should guarantee everyone an equal chance for "normal functioning," meaning a person's expected physical potential given his or her age and the conditions of a given society. Daniels' account displays mastery of the relevant literature and is closely reasoned. His deliberateness does make the book somewhat repetitive and technical; those not versed in Rawlsian analytic categories may find it difficult. Daniels applies his scheme to OSHA health regulation as well as to issues of health services.

His most startling conclusion is to question the heavy emphasis in current health policy on institutional care for the aged. Since many elderly would function better if they received care in their homes, he reasons, there should be more home health care and less resort to hospitals or nursing homes. At the same time, because he defines normal functioning relative to age, he thinks too much may be spent on the elderly. The health system gives them more help toward reaching their potential than younger cohorts. Accordingly, some expensive therapies, such as organ transplants, might justly be rationed by age and thus denied to many elderly, as is done in Britain. The argument is bracing but compelling. It amounts to saying that health care should do more to serve life and less simply to postpone death.

Otherwise, the implications of this scheme do not seem radical, at least for American

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health policy. Like Rawls, Daniels seems to accept the liberal welfare state in essence. He seeks changes mainly of degree. While he denies that the rights of health providers should impede movement toward a more egalitarian health system, he also rejects notions of a "right" to health that some theorists and politicians have used to seek massively greater government control and spending in health. Daniels' approach might justify quite a frugal health system in a poor society.

Nevertheless, he sometimes expresses the moral purism often found in theoretical treatments of policy issues. Political theorists tend by nature to emphasize what would be ideal, rather than political or economic limitations. Daniels says little about how a just health system could be realized politically, and in particular how excessive demands for care could be restrained. Like Rawls, he can in theory set limits to claims on government, but in practice he is loath to reject demands for health protections as long as basic social institutions are not fully just in Rawlsian terms—that is, as long as all inequalities do not serve the least advantaged. If setting limits must await such fundamental changes, political theory can be little help to health policy making today, where cost control is the major challenge.

The other two books say much more about actual politics. The analysis in Sheri I. David's *With Dignity* is largely legislative. The book tells the story of the enactment of Medicare and Medicaid. It carries the reader through every twist and turn of the battle in Congress, from 1953 through to victory in 1965. The book is clearly written and exhaustively researched. The sources include interviews and oral histories as well as public documents. Despite this effort, little about the findings is new. The story is much as in Theodore Marmor's *The Politics of Medicare* and other existing accounts.

David clearly sides with the backers of Medicare against those who opposed it or offered halfway measures. She believes that the popular forces demanding care from outside government held the key to good health policy. That view was most plausible before 1965, when government as yet did little to ensure health care to large groups of citizens. Afterwards, however, the very presence of Medicare and Medicaid helped set off the great

health inflation, damaging government budgets and access to care for everyone. Against these problems, the popular politics of health availed little. The coalition that enacted Medicare dissipated, and federal officials struggled with cost control almost alone. Expensive compromises with provider interests were inevitable. David does cover the implementation process, but because of her legislative focus she says little more about it than Marmor did in his book, published 12 years earlier.

The progressive view that the frontier in social policy lies where the people press demands on government has a long tradition in health studies. Ten years ago, Robert Alford contended in *Health Care Politics* that "structure interests" blocked the democratization of health. Not only doctors and other providers but health administrators and reformers, their apparent adversaries, had a vested interest in a pluralist health system in which consumer interests always came last. Another book with a similar viewpoint was Barbara and John Ehrenreich's *The American Health Empire*. More recently, Paul Starr's much-noted study of *The Social Transformation of American Medicine* traced the ascent of the medical profession from impotence in the nineteenth century to a position of cultural authority, wealth, and power over a vast health establishment. There have also been muck-raking attacks on specific health institutions, such as *Unloving Care*, Bruce C. Vladeck's critique of long-term care policy and the nursing home industry.

Such treatments understand the political problem in health as mobilizing a dispersed public against the concentrated interests that operate health services. They say this is tough to do in American politics, because of the pluralism of the system and the influence it accords to well-financed groups. Viewed blow-by-blow, achieving a government role in health was indeed difficult. The battle for Medicare took decades, in part because of provider opposition, and the nation still has no comprehensive health insurance system.

A longer view, however, suggests that government finally does move against overweening interests. Today, federal and state agencies are steadily trimming the prerogatives of doctors and hospitals, just as they previously curbed monopolies, Wall Street,

polluting industries, and other damaging expressions of unrestrained capitalism. In particular, the prepaid system of hospital reimbursement that Washington instituted in 1983, called Diagnostic Related Groups (DRG), has sharply checked the upward spiral of Medicare costs. The day may be coming when the providers become, in effect, public servants whose pay and conditions of work are closely regulated by government.

In the new environment, the main political task is no longer to mobilize popular pressures against government. It is to do two quite different things, both even more difficult for American politics: to restrain demands for benefits and to build an effective administrative system for health. Both tasks are necessary to control costs. The history is that middle-class benefit programs like Medicare may be controversial when enacted, but soon become sacrosanct. Politicians then face great pressure to maintain and expand the benefits, whatever the expense. The administrative problem arises because programs are enacted piecemeal, and their implementation is usually neglected. Administrative issues are seldom visible or partisan ones that easily mobilize a broad constituency. American politicians get elected mainly for initiating new programs, not for reforming or managing the old ones. They seldom have the patience for the years of bureaucratic statecraft needed to rationalize health or any other policy area.

Thus, the health analysis we need today is about health politics in the "small," not the "large." It should emphasize administration rather than legislation. It should mainly be about implementation problems in programs that already exist. It should suggest tactics for overcoming the political and administrative barriers to a successful health regime. It should help make American government do what it does not easily do—govern the health system well. We already know how Medicare passed. We need to learn why DRG succeeded. Somehow, the Reagan Administration sold Congress on a tough regime for hospital reimbursement when, only three years before, the hospitals interests defeated a Carter proposal with comparable aims. Was it that Reagan officials presented DRG as an exercise in "incentives" and even "competition," rather than as heavy-handed bureaucratic control? Was it that Republicans can regulate more easily than

Democrats because they cannot be seen as anti-business?

Political analysis like this is also an advance over merely economic treatments of health issues. Economists are very good at telling government what it ought to do. For example, Alain Enthoven in *Health Plan* urges that a health-care system based on competition among third-party payers could limit cost increases, while Henry J. Aaron and William B. Schwartz in *The Painful Prescription* show how British-style rationing of care could do so. Each case is compelling in itself, but neither shows convincingly how to institute the reform given the institutions we have. While the authors realize the political barriers, they think that making an impersonal case for reform is the only way to overcome them. That reflects the faith of economics that the core of policy making is optimal choice.

For political analysis, however, the central problem is assembling support for what is optimal. Successful tactics seldom follow directly from the economic case for a policy. That is because public choices are made, not by single officials, but by many, and the interests of each may diverge from that of the collectivity. Any recommendation must be squared with the electoral motivations of individual politicians, and individual officials must be able to carry out the policy within the bureaucracy. Macro policy analysis must be joined to micro analysis of how policy makers operate in existing institutions.

Theodore R. Marmor's *Political Analysis and American Medical Care* is, in large part, a manifesto for this new style of health analysis. The book is a collection of essays Marmor wrote in the 1970s, mostly with coauthors. He contends that political science can help identify what is special about health politics and also contribute to realistic policy options for health. His general discussion of how political science concepts might apply to health is dry, but the idea comes alive in many of the individual essays. The subjects include national health insurance, health planning, Medicaid and Medicare, and various strategies for controlling health costs.

Marmor and his associates examine what each program or policy has achieved or might achieve, given the political constraints and experience to date. They often draw illuminating comparisons between the United States and

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other countries, particularly Canada. If there is an overall message, it is that authoritative measures to limit health facilities and reimbursement are indispensable to curbing the expansion of the health system. Current American planning and funding policies lack the teeth simply to say *no* to excessive demands. The policy history here and abroad makes the authors doubt that incentives and other market devices will suffice, despite economists' claims, though they can make a contribution. Such "crude measures" as hospital bed limits will likely control spending better than "subtle management measures of theoretical force for which one cannot find large-scale instances of successful use and impact" (p. 173).

The essays suggest the great contribution the new style of health analysis might make. However, some are well-written, some not, and many are too short to analyze their subjects in depth. The discussion of the institutional setting is necessarily limited. They are also pessimistic, perhaps needlessly so. They tend to debunk nostrums in health policy and leave little in their place. Economics proudly calls itself the dismal science, a discipline for making tough choices. In Marmor's hands, political science sometimes seems too tough to offer any solutions at all. However realistic it is, the discipline must remain the platonic master science that expands collective possibilities. If nothing else, the shade of Burke rises up to demand that existing institutions somehow be turned to better account. A longer, more probing kind of study might suggest ways to improve health governance that would be both effective and politic.

Fortunately, a number of studies like this

have begun to emerge. Examples include Lawrence D. Brown's *Politics and Health Care Organization*, a study of federal HMO policy; Judith M. Feder's *Medicare*, an analysis of the early rule making in Medicare; and Victor G. Rodwin's *The Health Planning Predicament*, a comparison of health planning in the United States, Britain, France, and Quebec. Another example is Frank J. Thompson's *Health Policy and the Bureaucracy*, a useful synthesis of implementation findings about several major health programs. These studies are based on extended research done mostly inside the health bureaucracy. Each sensitively explains how political and bureaucratic forces have shaped the policies we have. Sometimes the authors make explicit policy suggestions, sometimes not; in any event, the reader or the policymaker learns the parameters of the possible.

The theme of such research, stated explicitly by Marmor and by Thompson, is that health policy making can no longer be as popular as it once was, in either sense of that word. The struggle over Medicare was the greatest progressive battle for health care. Not by accident, it was also the last. The struggles officials now face to limit demands and rationalize the system have only remote political payoffs, not immediate ones. They must operate in a system where heavy political commitments and existing agencies already encumber the ground. If political scientists can help them understand that environment, they have contributed all they can to the daunting tasks of health statesmanship.

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POLITICAL THEORY

The Liberal Future in America: Essays in Renewal. Edited by Philip Abbott and Michael B. Levy. (Westport, CT: Greenwood Press, 1985. Pp. vi + 210. \$29.95.)

During the 1960s and 1970s, the various political and philosophical shortcomings of

American liberalism were proclaimed in works such as *The Poverty of Liberalism*, *The End of Liberalism*, and *Beyond Liberalism*. In retrospect, the troubles of liberalism in those decades seem minor when compared to its difficulties in the era of Ronald Reagan. Philip Abbott and Michael Levy have compiled an in-

teresting collection of essays on the current dilemmas of American liberalism. The editors are especially concerned with "the difficulty of defining a liberal perspective in a complex world for which liberalism has neither lost its relevance nor proven its adequacy" (p. 11). The essays—all but one of which are published here for the first time—address a wide range of topics. Most of the contributors offer critiques from within the framework of the liberal tradition, while attempting to relate liberal theory to political practice.

The essays fall into two broad categories. Some focus on the broad philosophical premises of liberalism, particularly the relationship between the individual and society, while others emphasize issues of policy and politics. A recurring theme in the former group is the need for liberalism to complement its idea of individualism with a fuller understanding of the social and communal aspects of human experience. In the opening essay, Glenn Tinder examines the liberal conception of liberty. He maintains that the principle of liberty, properly understood, rests on the recognition of an incurable breach between the individual and a society that is always "morally degraded" to some degree. The ideal of liberty, based on this tragic vision, has been distorted by its transformation into the ideology of liberalism. Both *laissez-faire* and reformist liberals wrongly assume a basic harmony between liberty and other major political values. This argument strikes me as extremely problematic. Tinder seems to assume there is a single, essential ideal of liberty, rather than legitimate competing conceptions. Moreover, he criticizes ideological oversimplification in liberalism without examining the ideas of specific liberal thinkers.

The remaining essays in the first group, although not lacking in points of controversy, are rather more convincing. Philip Abbott observes that while liberalism has made major contributions to historic innovations in the political and economic realms (constitutional government and the market economy), there has been a scarcity of liberal "social inventions." In the contemporary world, where social institutions are increasingly penetrated by political and economic forces, the future agenda of liberalism must involve the creation of new communities with "purpose and commitment," as well as the development of a

philosophy that is "truly social." Robert Booth Fowler discusses a relatively neglected topic, the relationship between liberalism and religion. He contends that religion in America serves as a necessary "alternative" to liberalism, as a refuge from individualism and rational skepticism. Neither proponents nor critics of liberalism generally realize the extent to which liberalism needs religion; by diffusing the "costs" of liberalism, religion makes the liberal world more tolerable for many citizens. Jean Bethke Elshtain criticizes contemporary American feminist theory for being captive to the assumptions of liberal individualism. By giving primacy to individual rights and freedom of choice, feminism cannot account for central aspects of social experience. As an alternative to the framework of liberal individualism, Elshtain proposes a feminist theory that incorporates ideals of civic humanism, active citizenship, and participatory politics.

The best of the policy-oriented essays is Michael Levy's critical analysis of the doctrine of the "new property" as a legitimating basis for social welfare entitlements. He concludes that this conception of property rights ultimately undermines the fundamental liberal principle of the distinction between the state and society. As an alternative, he advocates a "neo-Lockean" approach to liberal egalitarianism, emphasizing social and economic policies designed to enhance meaningful opportunity for all individuals to develop their capacities. Alan Stone considers the neo-*laissez-faire* attack on government regulation. He contends that a key reason for the success of this attack is that liberal defenders of economic regulation have not developed a coherent framework for determining when regulation is appropriate and justified. Stone proposes a framework (essentially derived from mainstream welfare economics) that suggests three main justifications for regulatory intervention: efficiency, the problem of externalities, and considerations of equity. James C. Dick examines the liberal approach to the use of force in foreign policy. He criticizes liberals for focusing on the domestic politics of other nations, an error stemming from the application of liberal individualism to international politics. The liberal perspective tends to treat all individuals as citizens of the world, while it neglects intermediate institutions (i.e., nation-

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states) and the structure of the international system.

The volume concludes with an essay on liberalism and electoral politics. David B. Hill discusses the decline of the Democratic party and criticizes New Deal liberals for attempting to appeal to voters in ideological terms. He maintains that rebuilding a liberal constituency must begin with self-declared liberals in the electorate, while focusing on a narrow range of issues such as education, health, the environment, and civil liberties. Hill's strategy is based on a very dubious proposition; namely, that New Deal liberalism has been too "ideological." To the contrary, in the current era of conservative ascendancy, a conspicuous failure of American liberalism has been its inability to mount a visible ideological counteroffensive. Consider, for example, the conservative attack on the welfare state. While various constituencies have defended certain government programs, liberal Democrats have not offered any comprehensive defense of the welfare state. The political renewal of liberalism is not likely without some sort of ideological renewal. Ideological coherence need not entail adherence to rigid dogma. The reconsideration of liberal policies should not lose sight of the central importance of liberal principles.

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Psychological Perspective on Politics. By Carol Barner-Barry and Robert Rosenwein. (Englewood Cliffs, NJ: Prentice-Hall, Inc., 1985. Pp. iii + 342. \$18.95, paper.)

The authors undertake the task of describing "how . . . personal and environmental factors . . . influence political behavior" (p. 8). The task is a difficult one, seeming to require either a synthesis of diverse data and theoretic perspectives into one (or a few) explanatory paradigms, or the use of some other organizing tool to highlight similarities, discrepancies, and inconsistencies. The summary waivers between positivist and psychoanalytic perspectives. While this reflects the diversity of the field, it is discomforting to the reader and often creates difficulties (such as unwarranted causal attributions) for the authors.

The book lacks a central theme. It covers topics as diverse as the philosophy of science, involvement, socialization, leading and following, persuasion, bargaining and negotiation, conflict, decision making, and perceptions of justice. The book is an attempt to define political science and explain all aspects of politics. The authors try to do too much.

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Guilds and Civil Society in European Political Thought From the Twelfth Century to the Present. By Anthony Black (Ithaca, NY: Cornell University Press, 1984 Pp. xv + 280. \$32.50.)

This book is a study of the concept of political community as it appears in Western political thought. It is distinctive in that it focuses neither on the geographically local community nor on the idea of the polity itself as a communal entity, but on community in the workplace. As such, it stands as an important intellectual history of the guild.

To have provided such a history would itself be a significant contribution, but Black's achievement is rather more than this. His goal is not simply to understand and relate the history of the guild, but to demonstrate how the guild ideology influenced and contributed to the development of medieval and modern political thought. In this regard, the sweep of the book is quite extraordinary. Black treats the development of the concept of the guild from the Middle Ages to the twentieth century and, in the process, provides substantial treatments of such diverse thinkers as Marsiglio, Althusius, Bodin, Hegel, and Durkheim, among many others.

Throughout, the argument is structured by two sets of oppositions, one theoretical, the other formal. Theoretically, Black pursues the tension in political thought between the guild idea and the concept of civil society. The guild contributes to notions within political consciousness of fraternity and mutual aid, a sense of corporate identity, and the view that collective decisions should emanate from relations of friendship. The concept of civil society, on the other hand, emphasizes principles of personal security and freedom, legal rights, and—in

creasingly—a bourgeois view of the individual as having claims apart from his particular social connections. It is one of the distinct virtues of this study to have traced in substantial detail the interplay of these notions as they manifest themselves in political thought. The excellent treatment of Marsiglio in this respect (especially pp. 93–94) is merely an example of the way in which Black's conceptual apparatus elicits new and quite persuasive treatments of already well-worked material.

Formally, the book is structured by the tension and interaction between the ordinary consciousness of the guild experience, on the one hand, and the historically conditioned but nonetheless quite distinctive claims of philosophy, on the other. Thus, Black presents evidence not simply about the great thinkers, but about the mentality of medieval townsmen, the political ethos of German guilds during the Reformation, the consciousness of the modern trade unions, and the like. The scholarship here is exemplary, and the presentation is such as to require that philosophical formulations be considered in the light of historical practice and ethos, and vice versa.

In this connection, Black has appended to his study a useful and self-reflective "Note on Method" in which he acknowledges his debt to, yet carefully distances himself from, the work of Pocock and Skinner. He is wary of the notion of "paradigm" in intellectual history, worrying that it presupposes a kind of coherence where there is none. He recognizes the importance of seeing political thought in its political contexts, but wishes also to account for its socioeconomic contexts. Also, he refuses to prejudge the question of whether political thought merely reflects, or in fact has an autonomous impact upon, more "material" processes.

These various methodological principles are very well reflected in the study itself. Indeed, from the perspective of our own guild, the guild of political philosophers, this book is something of a model of good craftsmanship. It is, however, to be admired on other than purely aesthetic grounds, for I believe that Black has made here a substantial contribution for the history of ideas.

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Revolution: A History of the Idea. Edited by David Close and Carl Bridge. (Totowa, NJ: Barnes and Noble Books, 1985. Pp. 221. \$23.50.)

This book is at once a textbook and a work of scholarship. Produced by the disciplines of history and politics at Flinders University of South Australia, it brings together essential components of the study of revolution: the history and analysis of the concept, and several illuminating case studies. By the editors' own design, the book is not comprehensive. Its case studies are all derived from the twentieth century, and the subjects of the various essays were determined by the needs of the students and the interests of the authors.

The result of this design is a fine book that will serve both those who teach the concept of revolution and those who wish to learn something about it. Nearly two-thirds of the book consists of seven essays on the historical development and analysis of the meaning of revolution. The last third is occupied by five case studies: the Soviet Union, China, Nazi Germany, Guinea-Bissau, and France in May 1968. These serve as a valuable addition to a book seeking a student audience. The cases develop the notion of revolution as failed (USSR), as developmental (China), as unworthy (Nazi Germany), as successful (Guinea-Bissau), and as transformed (France, 1968). As such, this section takes the readers through the twentieth century concept of revolution, leads them beyond the European understanding of a revolution, and concludes with the question of whether revolution as a concept makes much sense. It is this section that will serve the broadest pedagogical needs.

The heart of the book, however, is its theoretical development of an idea. This of course is not something new for students of history or politics, but what recommends this particular treatment is its designed contentiousness. It is a wholly contentious book, and delightfully so. Not that the authors contend with one another in print, although the editors admit to a lack of agreement over the definition of the book's central idea. Rather, the book is contentious in the finest sense, because its central idea is contentious. Those who want to learn about revolution rather than imbibe aged academic ideologies of revolution will benefit from reading this book. Those who

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teach about revolution with some measure of openness will want to read this book also. It challenges as it informs.

Chapters 2, 3, and 4, all by Peter A. Howell, constitute the core of the book's analysis. They are also the most fun. From the ancient Greek polity through European liberalism of the nineteenth century, the common concept underlying revolution is that which underpins all politics until academic political science—the concept of social justice. Here is the effervescence that explodes, alters, and provides the medium for new orders. In his analysis of the history of the idea, Howell contends, paraphrasing Thucydides, that in its oldest form, revolution was thought to lead to a reversal of values:

Recklessness . . . mistaken for course; prudent delay and any attempt to understand a question from all sides . . . ridiculed as cowardice; moderation . . . regarded as the disguise of unmanly weakness. Fanaticism and violence . . . held to be the marks of a real man, and the seal of good faith . . . neither law nor justice but fellowship in crime. (p. 17)

Students of revolution who take the issues of social and individual justice to heart will recognize the sobering insight of Thucydides, and be perplexed when they recall that in spite of all we know about political life and political violence, there is still something about revolution that makes us sing. What is it that sobers as it makes us sing? Perhaps it is the *ideal* that human beings are capable of justice in the face of its paradoxes. Howell does an admirable job of tracing how the idea of revolution came to be part of the optimism of modernity.

Like all good books, this one should not have its story revealed in a review, but apropos the issue of contentiousness, those pursuing intellectual connections will want to consider the book's fine treatment of the medieval and modern ideas of politics and violence. Imagine St. Thomas Aquinas as a revolutionary and Marsilio of Padua as a reactionary! Readers of all persuasions will find something here. It is intellectual history at its best.

Underlying this history are the value-based issues which make the modern mind uneasy. Yet this is exactly what the study of revolution ought to do. If it is true that revolution is part of modern optimism, it is still true that Thucydides is right. At the root of Howell's

essays and of those on Burke, Marx and Lenin, and contemporary theorists of "modernization" is the implicit conception that without the idea that persons count for something and have rights and powers, the idea of revolution itself does not signify. Revolution is merely violent politics. Yet we know that this is not all; still we sing.

R. W. CARSTENS

Ohio Dominican College

Controlling Nuclear Weapons: Democracy Versus Guardianship. By Robert A. Dahl. (Syracuse, NY: Syracuse University Press, 1985, Pp. xii + 113. \$14.95, cloth; \$8.95, paper.)

Can democratic governments make wise and informed judgments about matters of exceptional complexity, such as the control of nuclear weapons? Yes, says Robert Dahl, but only if they undertake unusual steps—ones that have been made possible by recent advances in telecommunications—to increase the competence of citizens.

First delivered as the Frank W. Abrams lectures at Syracuse University in 1984, *Controlling Nuclear Weapons* is more accurately described by its subtitle, *Democracy Versus Guardianship*. In the United States today, Dahl argues, decisions about nuclear weapons are made by a technical elite to which we have not so much delegated authority as alienated it—in no meaningful sense do the citizenry exercise control over this elite. If these decisions require technical competence beyond the reach of ordinary citizens, perhaps we need a new governing ideal: guardianship, not democracy.

By *guardianship*, Dahl means a principled authoritarianism: rule by "a well-qualified minority," "governing in the interests of all, fully respecting the principle of equal consideration," and ruling with "the consent of all" (p. 31). Plato's *Republic* is his principal theoretical depiction, the Republic of Venice his closest long-standing historical approximation.

Competence to rule involves two components: technical or instrumental knowledge and moral competence. Moral competence, in turn, involves both an understanding of moral issues and a disposition to act on this

understanding (virtue). With issues like nuclear weapons, Dahl argues, technical and moral competence are inextricably tangled—instrumental judgments in these areas require assessments of uncertainty, choices of propensity toward risk, and tradeoffs among values. All of these have inescapably moral dimensions.

Recognition of the interdependence of moral and technical judgments, however, undermines the case for guardianship as well as democracy. Our *de facto* nuclear guardians may have technical but not moral competence. The historical record of authoritarian regimes shows that their rulers have difficulty in gathering undistorted information and in finding and training suitable successors. Their moral inadequacies are particularly conspicuous.

Who then shall rule? There is no solution to this dilemma—and thus no rescuing of democracy—unless the citizenry can become more technically competent. Rejecting increased citizen participation and better education (conventionally understood), Dahl puts forward a solution he admits is “quasi-utopian.” Using interactive telecommunications technology, information on major issues should be made readily available to all citizens. Self-perpetuating panels of experts (like the National Academy of Sciences or major professional associations) would be responsible for preparing the information in a fashion that was both accurate and “appropriate in level and form” (p. 79) to the diverse capabilities of citizens. Because no citizen could expect to become informed about all the difficult issues we face, a *minipopulus* of about 1000 should be chosen, by lot, to constitute a “highly informed body of public opinion that (except for being highly informed) is representative of the entire citizen body” (p. 76). Decisions of each *minipopulus* would not be binding. We would still have Congress, a president and the Court. Rather, the *minipopulus* would “stand for” the public, “reflecting public opinion at a higher level of competence” (p. 88).

These innovations are put forward only tentatively, and Dahl says little in response to several immediate objections that arise. Could the panels of experts escape politicization? Such associations “are probably about as independent of external controls as any organizations in our society” (p. 84), Dahl

asserts, but there has been little reason in the past to try to control or co-opt them. Could the information they provide be neutral? Dahl seems to think it could, but doesn’t this fly in the face of the interdependence of the instrumental and the moral? And what would become of representation by and the accountability of elected officials under such a scheme? How would we expect the citizenry as a whole to assess divergences between the judgments of a *minipopulus* and those of elected officials?

Dahl suggests that “small scale experiments should precede large scale adoption” (p. 89). His stress is not so much on the solutions as on the clarification of “a problem of considerable urgency” (p. 89). In recent years, Robert Dahl has challenged us to see what a commitment to democracy may require of us today, particularly an extension of popular control to the workplace and the economy. With *Controlling Nuclear Weapons* he addresses a different modern difficulty. This book cannot be said to add much that is new to democratic theory, but it does bring a vital issue into clearer focus. It is a readable reaffirmation of democracy in the face of the most troubling moral issue of our time.

DOUGLAS C. BENNETT

Temple University

Distributive Justice: A Social-Psychological Perspective. By Morton Deutsch. (New Haven: Yale University Press, 1985. Pp. vii + 313. \$27.50.)

With some willing suspension of disbelief I am prepared to accept that the social psychological laboratory models important aspects of the social world we all live in. Do university students engaged in card-sorting tasks share something with workers sorting things on an assembly line? When they play a rubbish removal game under circumstances of cooperation or competition do they reveal processes analogous to those of small business men whose trucks remove real rubbish on real streets? Certainly these tasks reveal as much about psychological processes as the eternal prisoner’s dilemma games—probably more. Since the findings of these laboratory studies

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are substantial improvements over the philosopher's intuition about human behavior and the economist's often naive reliance on rational maximization, the suspension of disbelief is willing, but temporary, for institutions are not laboratories and games are not the same as careers. It is the psychologists themselves, at last, who warn of the situational specificity of response patterns.

A little over ten years ago Morton Deutsch asked: "Equity, Equality, and Need: Which Value Will be Used as the Basis for Distributive Justice?" Here he reports studies that contribute answers to this question, and that correct his earlier answers—in an ingenious array of experiments, some of them reported here for the first time. Deutsch and his coworkers set subjects (mostly university students) to work decoding poetry, sorting cards, estimating jelly beans, improvising parts in plays, playing business games, and reporting throughout on their motives and feelings. Some tasks are cooperatively structured, some competitively structured, and some permit each subject to go his or her own way individualistically. The concern with justice is addressed by varying the reward system: to each according to his score or contribution, to each equally, according to need, or winner-take-all. The findings are fascinating.

Earlier Deutsch suggested that the justice of "equity" (the psychologists' term for rewards proportional to the contribution to some enterprise) would be favored where productivity was highly valued. In the experiments Deutsch finds that "there is no reliable or consistent evidence to indicate that people work more productively as individuals or as group members when they are expecting to be rewarded in proportion to their performances than when they are expecting to be rewarded equally or on the basis of need" (p. 198). Is the capitalist wage system (and, in practice, that of command economies too) based on an illusion? Deutsch notes that this finding applies only to tasks which have some intrinsic interest to subjects who are motivated to do well on other grounds; it would not, he says, apply to alienated labor. (He does not report that research on the effects of extrinsic rewards for tasks with intrinsic interest have usually found working-class subjects responding differently from middle-class subjects.)

Where the rewards are offered for per-

formance on group tasks requiring cooperation, competitive reward systems—such as those implied by scoring each individual for his contribution to the group score—impede group performance, for they inhibit sharing of information or exchanging cards or pieces of a puzzle. Because equal rewards to group members avoids this difficulty, equality under these circumstances is a form of justice with higher productivity. In line with many other studies, Deutsch finds that, in contrast to proportionality, the justice of equality promotes greater group harmony and more favorable attitudes toward fellow workers. This confirms his earlier hypothesis.

Before the experiment, most subjects preferred reward systems proportional to individual scores or to individual contributions. On the other hand, people tended to increase their preferences for whatever justice system they worked under. If proportionality is the justice of the market economy, it fits people's preferences—and reinforces them.

The findings in the four chapters dealing with these experiments are rich and often counter-intuitive. They deal with awakening the sense of injustice (social support greatly helps people's generally obtuse perceptions of unfairness to others—and to themselves!). People prefer rewards based on achievement instead of effort (effort-based rewards lead to work simulation and suggest shirking; they are disliked). There are some interesting personality correlates of justice preferences. The various distribution systems change attitudes in a particular way: people tend to accept and generalize the premises of the systems they work under, etc.

But the book is enlarged by essays on cooperation and conflict resolution, interdependence, worker self-management systems and preventing World War III—often with a genuflection in the direction of the altar of justice but without serious worship. As a consequence, the empirical justice studies—which carry, I think, the main freight of novel material—are condensed to the point where one must struggle to distinguish the variables and to extract their importance. The struggle is worth it. The ingenious and significant research reported here bears critically on the questions raised by philosophers like John Rawls and Ronald Dworkin, political philosophers like David Miller and William Galston,

and economists like Amartya Sen and Lester Thurow.

ROBERT E. LANE

Yale University

Nationalism, Ethnocentrism and Personality.

By H. D. Forbes. (Chicago: University of Chicago Press, 1985. Pp. x + 219. \$27.50.)

Since the writing of *The Authoritarian Personality* in 1950, a number of attempts have been made to extend the principal themes of that classic work in a way that advances political philosophy. Forbes's work is another example of such a piece. Though the recognition of the theoretical potential of *The Authoritarian Personality* (AP) is laudable, Forbes unfortunately fails to add much of substance to its theoretical thrust.

The reasons for Forbes's failure are predominantly two-fold. The first is that Forbes is wrestling with a massive collection of dissertation data (collected in 1968) that unfortunately does not tap any significant theoretical vein. The second is Forbes's own confusion concerning the theoretical state of the AP argument, as well as his confusion over why critical theory, his attempted avenue to theoretical advancement, is itself mired within the ideological debate over the role of psychology within political theory.

With regard to the empirical difficulties of the work, Forbes's two hypotheses are:

1. Extreme nationalists are recruited disproportionately from those with the authoritarian structure of personality; and
2. Different nationalists' attitudes are structured, or interrelated, in such a way as to justify using the term "ethnocentrism" to describe the attitudes of extreme nationalists. (p. 3)

His findings, the result of questionnaires administered to French-speaking and English-speaking Canadian high school students, is that there are different kinds of nationalism. Specifically, what Forbes finds is that ethnocentrism is only found to correlate with nationalism in those circumstances in which the outgroup is clearly different from the ingroup. At the least, such a finding is already

implicit within AP; at most, it still does little to advance political philosophy.

The attempt to advance political philosophy beyond the findings of Forbes's study is even more frustrating. Forbes, to his credit, does understand that AP is a clear outgrowth of the Frankfurt school of thought that we know as critical theory. He is aware that works such as AP are the result of a historically rooted admixture of Marx and Freud, and that the writings of Horkheimer, Fromm, Reich, and, of course, Adorno himself all attempted to deal with the psychological or, as it is frequently called, the "early" period of Marx. Yet if Forbes gives such theoretical significance to the critical perspective, one must ask why the almost paltry scope of the two hypotheses of the book is not supplanted by more than an occasional reference to critical theory. Within 193 pages of text, critical theory is abandoned after page 15 until it is resurrected on pages 145-148, and later on pages 189-193 (with the ironic disclaimer that "no one familiar with critical theory would lightheartedly undertake to plumb its depths and report the results in a page").

Even there, the "doctrine (or method) of liberation" that Forbes finds in critical theory is searching for a kind of "objectivity" that in turn is the result of internal contradictions between "objects" and "the claims made on their behalf" (p. 191). The difficulty with such a theory of liberation is that it deals with only the contradiction between reality and objective, between promise and performance. What AP offers, which Forbes seems to have little sense of but which others have dealt with quite precisely (see, Christian Bay, *The Structure of Freedom*, New York: Athenium, 1968) is that *perspectives* upon reality differ widely between the authoritarian and the anti-authoritarian personality. Stated another way, the latter-day questions surrounding AP have dealt with relative subjectivities, not objectivity.

There is now more than adequate evidence that there are significant and testable *distinctions* between psychologies that are highly relevant to differentiation between the political left and the political right. Such distinctions are available not only within the work of political scientists like Bay, but also within the work of cognitive psychologists like Herman Witkin, Joseph Royce, and Howard Gardner.

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To resurrect a classic work requires that one be aware of what has gone on since its demise. To attempt a theoretical advance upon a significant work without an exploration into the difficulties and biases of that work's theoretical framework, particularly within an area so ideological in its origins and implications as the authoritarian literature is, assures that the current work will be but a small contribution.

WILLIAM P. KREML

University of South Carolina

Georges Sorel: The Character and Development of His Thought. By J. R. Jennings. (New York: St. Martin's Press, 1985. Pp. xi + 209. \$25.00.)

J. R. Jennings's study manages the difficult task of combing intellectual biography and the analysis of ideas. It is especially good at making sense of the transitions that are notoriously a feature of Sorel's thinking. The book is compact and clear, the proportions of its discussion judicious, and its scholarship faultless. It is one of rather few works on Sorel that one could recommend to students without fear of their being confused or misled: At the same time, it is a sophisticated work (but unpretentiously so) that one ought carefully to consult in doing research on any of the diverse topics it discusses. It is one of several recent pieces of evidence that Sorel scholarship is at last being given the imaginative treatment it deserves.

One French commentator (quoted by Jennings) has said that Sorel's writing "excluded order, clarity, and, in general, all that would make the assimilation of his ideas easier." Jennings himself elaborates on the difficulty of reading Sorel's work, referring to its sheer extent and diversity, its frequent abstruseness, its heterogeneity of level, and the volatility of the hopes (and the despair) that impelled it. He adds that "pluralism" precluded Sorel from offering any unified or systematic exposition of his thinking (pp. 176-77). Pluralism is not inherently unsystematic. If Sorel had held that a single set of philosophical principles dictated identical conclusions in diverse fields—Jennings speaks of "methodological, scientific, epistemological and ethical pluralism" (pp. 12, 15)—then he would have been a systematizer indeed. However, Jennings does not take

Sorel's pluralism to be anything so elaborate or developed. It is something more like a disposition than a theory, and must be defined in a way supple enough to embrace several potentially separable things: conflicts between disciplines, conceptual diversity within disciplines, differences between types of explanation, incommensurability, as well as a sheer stubborn sense of the complexity of life and a hatred of the naive.

What is called pluralism here might possibly invite, the alternative label of *dualism*. Dualism may of course be merely an impoverished pluralism (or a monism *manqué*) but it need not be, and there seems to be a difference between positing an indefinite number of possible points of view and insisting on exhaustive choices. Very often, Sorel does the latter. He speaks of the artificial and the natural, the inside and the outside, the abstract and the concrete, the psychological and the scientific, and so on. Even his pluralistic doctrine of communities (*cités*) of enquirers tends, effectively, to collapse, into a "polar" opposition of science and religion (p. 11). It would be enormously interesting to examine the relation between Sorel's disposition to pluralize, or to open questions to an indefinite number of answers, and his disposition to dualize, or to insist that answers of only two kinds can count. It is among the many merits of Jennings's book that it poses such constructive and far-reaching questions.

RICHARD VERNON

University of Western Ontario

The Ivory Tower: Essays in Philosophy and Public Policy. By Anthony Kenny. (New York and Oxford: Basil Blackwell, 1985. Pp. 137. \$29.95, cloth.)

Kenny's collection of philosophical essays is intended to show that the concepts of intention, voluntariness, and purpose play a crucial role in legal and political issues of broad and general interest. The nine essays in this volume fall into two categories: philosophy and law, and philosophy and war. An epilogue consists of one essay on academic freedom.

Part 1, on the philosophy of law, contains four papers. The first two, "Direct and Oblique Intention and Malice Aforethought"

(1976) and "Intention and *Mens Rea* in Murder" (1977), seek a definition of murder which adequately takes into account the role of intention. Both essays present fundamentally the same arguments. Kenny uses the 1974 case of *R. v. Hyam* as a case study. The case concerned a woman who, intending merely to frighten away a rival for her lover's affections, set fire to the rival's home thereby resulting in the death of the rival's two children. The woman was charged with murder in the death of the children and subsequently convicted. While agreeing to dismiss the appeal, the Court of Appeals' majority judges in the case cited different views of intention required for murder. Can a single definition of murder be given? Kenny appeals to Bentham's distinction between direct and oblique intention and the position of Lord Hailsham, one of the majority judges. The result is that murder is "an act which causes death with the intent either to kill or to create a serious risk of death: the intent in each case is to be direct. The intent to kill should be taken to include the (direct) intent to bring about a state of affairs from which one knows death will certainly follow" (pp. 14, 29).

"Duress *per Minas* as a Defence to Crime" (1979) is Kenny's reply to Lord Kilbrandon's paper on duress delivered at a Royal Institute of Philosophy conference on the philosophy of law. The question is: "Should duress provide a defence or mitigation to a charge of murder?" (p. 32). The majority held, in the 1975 case of *Lynch*, a victim of IRA threats, that duress was indeed a legitimate defence. Kenny proceeds by laying out two main arguments permitting duress as a defence and one argument prohibiting such a defence. In the end, Kenny, invoking the opinion of Lord Kilbrandon (who was in the minority in *Lynch*) and the view of English legal historian J. F. Stephen that "criminal law is itself a system of compulsion" (p. 37), wishes to disallow duress as a defence for murder. "It is a very great misfortune to be placed in a situation where one must kill and suffer the consequences, or be killed oneself: any man must pray never to be thus placed between the devil and the deep blue sea. But if the law takes away the deep blue sea, a man will go wherever the devil drives" (p. 38).

"The Expert in Court" (1982), Kenny's Blackstone Lecture of 1982 and the longest of the papers in this volume, focuses on four weaknesses in the law regarding expert evi-

dence: (1) present law on expert evidence allows experts to adopt in various ways the functions of judge, jury, and legislature; (2) present law on expert evidence invites damage to the professions of the experts; (3) while the concept of genuine science is a necessary condition for the criterion for expert evidence, courtrooms are not appropriate places for distinguishing between genuine and pseudo-sciences—Kenny does provide a criterion for what constitutes a science; and (4) present legal procedure concerning expert evidence is flawed in two ways: an adversary legal system is often inhospitable to the proper use of experts and "there are no experts on morality" (p. 61). Kenny offers three broad corrective measures ranging from the establishment of registers of genuine scientific disciplines to empowering the court rather than the litigating parties to seek expert evidence.

Part 2 contains four similar essays concerning the morality of possessing nuclear weapons as a deterrent to war. In its broad outline, the composite argument of these four essays goes as follows:

(1) The destructive use of nuclear weapons in the conduct of war is immoral because it would unavoidably involve the intentional killing of innocents (this, for Roman Catholics would, apart from any utilitarian moral grounds, violate an absolute divine prohibition), and it would violate the Socratic moral dictum that "it is better to undergo wrong than to do wrong," and "the evil that we would do, if we used nuclear weapons in a major war, would be incomparably greater than the evil we would suffer if the worst came to worst after (unilateral) nuclear disarmament" (p. 116).

(2) The maintenance of nuclear weapons as a deterrent to war is immoral because it is extravagant, it is dangerous, and most important, it is murderous. It involves a willingness or intention to commit murder on a grand scale and "one may not intend even conditionally to do what is forbidden absolutely" (p. 72). (This argument appears first in "Counterforce and Countervalue" [1962] and has been seriously challenged [see Gregory S. Kavka's "Some Paradoxes of Deterrence," *Journal of Philosophy*, v. LXXV, no. 6, June 1978, and Kavka's "Nuclear Deterrence: Some Moral Perplexities" in Douglas MacLean, ed., *The Security Gamble: Deterrence Dilemmas in the*

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Nuclear Age, 1984]. One wonders whether Kenny's unelaborated remark that this essay "contains some Catholic assumptions which I no longer share" [p. 2], was affected by such challenges since his subsequent restatement of this key argument in "Better Dead Than Red" [1984] and "Logic and Ethics of Nuclear Deterrence" [1984] is somewhat weaker.)

(3) It therefore follows from (1) and (2) that the NATO nations should pursue unilateral nuclear disarmament on moral grounds. While unilateral disarmament by the West would leave it vulnerable to Soviet nuclear blackmail, the risk is preferable to that of nuclear war. "The prospect of standing defenceless before Communist Russia is indeed a sombre one. But that does not justify us in covenanting with the NATO powers to commit murder" (p. 74).

The final essay of the volume, "Enemies of Academic Freedom" (1984), takes a somewhat conservative approach to external threats to academic freedom (e.g., governments) and internal threats to academic freedom (e.g., from its abuse by professors and students).

Anthony Kenny's collection of essays is at once stimulating and redundant. "The Expert in Court" is imaginative and productive. Part 2 is unduly duplicative: "The Logic and Ethics of Nuclear Deterrence" would suffice. Four of the essays have been published previously.

B. G. HURDLE

Virginia State University

In the Name of Eugenics: Genetics and the Uses of Human Heredity. By Daniel J. Kevles. (New York: Alfred A. Knopf, 1985. Pp. x + 426. \$22.95.)

Writing with commendable ease and clarity of style, Daniel J. Kevles, professor of history at the California Institute of Technology, has crafted a painstaking investigation into the dynamics of eugenics ideology as it has evolved over the past century in the United States and Great Britain. Kevles's essential strategy is to present eugenics advocacy through the socialization, career patterns, and writings of its principal protagonists. We are introduced to and become intimately engaged in the lives of Francis Galton, Karl Pearson, Ronald Fisher, Lionel Penrose, and Julian Hux-

ley, among many others. Their private papers provide a fine repository of data, while accounts obtained from various close kin and professional associates enable the author to report further penetrating insights. The mix between "liberal" and "conservative" political strains of commitment to the cause emerges with incisiveness, and Kevles well describes the naivete and bias which often have colored both orientations.

A critical question is the extent of the relevance of this study for political science inquiry. That issue becomes prominent because the author, in his final 50 pages, makes bold to analyze the cultural implications of sociobiology, recombinant DNA, abortion, I.Q. testing, and human genetic engineering taken together as constituting a "new eugenics" capable of exerting powerful (and, at root, deleterious) influence upon contemporary and future public policy considerations. Kevles's treatment of these materials is not entirely satisfactory. For one thing, his skills as a historian are somewhat inappropriate to this portion of his task. Second, he has not given himself enough time to develop a balanced and in-depth perspective. Part of the problem here is that he has allocated five chapters (12-16) to the growth of human genetic science, a discussion that is not directly apposite either to the eugenics he addressed earlier or to the new eugenics he plans to address. Third, his modus operandi lacks methodological rigor. He conducts personal interviews with E. O. Wilson and Richard Lewontin, but not with Arthur Jensen and Jonathan Beckwith. We are never told the criteria by which respondents were selected and why. Fourth, and most important, there are serious gaps in the presentation. Wilson's *Sociobiology: The New Synthesis* is probed with objectivity and understanding, but nowhere can a mention of the more salient *Genes, Mind, and Culture* be found. Cloning is disparaged, but Jeremy Rifkin's machinations receive not a citation. Park Gerald is interviewed, but the fact that some of his colleagues succeeded in putting a damper on his XYY chromosome research fails to attain footnote status. The individual's freedom to learn about his or her own genetic makeup is considered, but the scientist's freedom to perform genetic experimentation within constitutionally acceptable parameters is never even hinted at. In general, this discussion is too anxious to link

the social implications of contemporary breakthroughs in genetics and evolutionary biology with the sophistries of main-line eugenics, thus bearing witness to the author's normative predilections.

All in all, Kevles has rendered a distinctively valuable contribution with his penetrating historical review. What we now need to complete the picture is a sequel entitled *In the Name of Euculture*, which would document with equal vigor and scholarship the careers and preachments of those who today and yesterday have argued that genetics counts for nothing in explaining hominid social and intellectual life, that our species therefore possesses cultural choices constrained only by environmental circumstance, and that contrary propositions should not be entertained even as hypotheses to be studied scientifically, else man will be tempted to deracinate his unique moral integrity.

IRA H. CARMEN

University of Illinois

Ideology and Soviet Industrialization. By Timothy W. Luke. (Westport, CT: Greenwood Press, 1984. Pp. xi + 283 \$35.00.)

Luke's goal in this broad-ranging examination of the modernization of Russia is to offer a "culturalist explanation of Soviet industrialization." Such analysis entails both an examination of the role of the intelligentsia as the carriers of the values of modernization and industrialization, and of the place of Bolshevism as a "civil religion" that informed and gave impetus to social and economic change in pre-revolutionary and Soviet Russia.

In his analysis of the revolutionary Russian intelligentsia, Luke argues that such activist social elements substituted themselves for the weak bourgeoisie that had failed to develop spontaneously within Tsarist society. Because of their liberating role in shattering the social and economic complacency of society and their déclassé identity as a moral community defined by a shared revolutionary consciousness and a vision of modern secular society, the intelligentsia performed the same function as the Calvinist thinkers during the Protestant Reformation in generating a "civil

religion" built upon a messianic vision of society and a mutually supporting array of moral and organizational imperatives that gave institutional form to a particular brand of political activism.

For the working class, in whose name the revolution was made, the new civil religion translated both into concrete manifestations of worker activism and discipline and into more general forms of expanded cultural awareness that linked literary and artistic movements with the regime's attempts to create a culture of modernization combining moral and aesthetic dimensions with the formation of a work ethic stressing discipline and order. The Stalinist revolution of the 1930s altered the gradualist approach to economic and cultural revolution based upon the themes of worker education and participation, and substituted what Luke terms the "statist" approach to industrialization based upon an intensification of the class struggle and the creation of a technologically sophisticated industrial infrastructure. What had been a broadly defined reliance on a cultural revolution stressing the interlocking cultural, social, and economic elements of modernization yielded a single-minded fixation with industrialization and the creation of an elite class of new Soviet workers concerned with the material incentives provided by a regime that fostered their emergence for political as well as economic reasons.

While Luke's study is a competent and readable account of the cultural aspects of modernization and industrialization, it does not break any new ground, in terms either of our understanding of the symbiotic relationship between economic and cultural affairs or of our comprehension of the political motives that led to the shift from the culture of modernization to the culture of industrialization. His descriptions of the disciplinary and incentive program by which the regime sought to reach its goals are perhaps the best contributions of this work, for they do place within a broader political and cultural framework programs that seem at face value to be motivated by short-term expediency.

Another major shortcoming of Luke's work is that he has failed to place the evolution of a workers' culture in a perspective of larger social and cultural developments. While it struggled with the problems of modernization

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and the development of a culture receptive to industrialization, the regime also had to forge a new comprehension of the role of the intelligentsia per se and of the new emerging middle class, both of which colored its treatment of the working class.

DONALD R. KELLEY

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At the Dawn of Tyranny: The Origins of Individualism, Political Oppression, and the State. By Eli Sagan. (New York: Alfred A. Knopf, 1985. Pp. xxiii + 420. \$22.95.)

Eli Sagan's most recent book explores the darker side of the human spirit and the darker side of politics through an examination of the origins of the state. The public sphere, Sagan notes, did not always exist. In primitive societies, the bonds of kinship determined all human relations. Concepts of law, justice, and politics were defined by highly structured kinship systems: "all politics was family politics" (p. 226). What would drive the people of a primitive society to leave behind the comfortable world of kinship relations, and to put in its place a world of strangers in which non-kinship relations took precedence over the ties of kin? And why, in taking this revolutionary step into the state, would they also invent political tyranny, human sacrifice, social stratification, and economic oppression?

Seeking clues to the destructive forces of modern politics, Sagan investigates the stage of human society that first invented the public sphere. He locates what he calls complex society in the ancient kingdoms of Buganda, Tahiti, and Hawaii, which were in a transitional stage of development when westerners first made contact with them in the eighteenth and nineteenth centuries. Lacking the literacy and complexity of archaic civilizations (such as ancient Mesopotamia, Egypt, and China), they nonetheless displayed far more political and cultural sophistication than the primitive tribe. The creative and energetic inhabitants of complex society gave the world monarchy, aristocracy, bureaucracy, and specialized labor—as well as epic poetry, theatre, and the fairy tale. Paradoxically, these same vibrant and inventive people showed the world a more terrifying

facet of the human character. Along with a novel emphasis on imagination and individuality came a new cruelty and aggressiveness: their kings regularly practiced a ritual homicide unknown to primitive society. The "magnificent heroes" who created the first states were at the same time "sadists and pirates and megalomaniacs" who destroyed whatever stood in the way of their quest for omnipotence (p. 318). Politics thus emerged from the kinship system in the garb of tyranny. To Sagan, it is that legacy with which we struggle today, and that legacy which we must understand if we are to limit the potential for atavistic violence in modern life.

At the Dawn of Tyranny gives a rich and detailed portrayal of complex societies and the people who lived in them. Sagan's primary purpose is theoretical, however. The last section of the book provides a psychoanalytic theory of social development that Sagan hopes will explain the relationship between politics and tyranny. In an analysis based largely on the work of Anna Freud and Margaret Mahler, he posits a "universal human impulse toward individuation" as the force that brought about an end to the kinship system and the birth of the state (p. 233). According to this psychoanalytic psychology, human beings experience a powerful inclination to separate themselves from their families (and particularly their mothers). The bonds of kinship, apparently, do not fully satisfy. Separation brings anxiety, though, and a subsequent ambivalence about individuation. Primitive society avoids separation anxiety by repressing individuation; complex society promotes individuation at the price of deep uneasiness. Hence it "preserves a tremendous fear of re-engulfment" (p. 363). Tyrannical kings alleviate that fear by fulfilling a fantasy of omnipotence. So, too, does ritual sacrifice, for the power over life and death is the ultimate power. The great rewards of individuation—the creativity and energy that flow from it—come at a terrible cost in aggression (p. 363). They will continue to do so, Sagan suggests, until the wounds of the separation process have fully healed. Tyranny will exist as long as people yearn for the lost bonds of kinship (p. 294).

Sagan's psychoanalytic perspective persuades but does not wholly convince. Its weakness lies, first, in its failure to explain the exceptional. Given that the Nuer, unlike the

Baganda, retained the kinship system, how do we know that the drive toward individuation is universal? Sagan concerns himself with the impact of the psyche on culture, and in doing so ignores the impact of culture—of political institutions, power relations, economic forces, and social convention—in shaping the psyche. "The psyche," he writes, "is not the given, with all social forms being variables" (p. 347). Sources of change can be found in society too—and especially in society's child-rearing practices. Still, "the energy that drives the whole history of the world is the force of the psyche struggling to fulfill its developmental destiny" (p. 383). In the end, Sagan claims too much, even while his analysis intrigues.

Loose organization and lack of focus also detract from Sagan's work. He introduces his theoretical framework belatedly, and thereby maroons the reader in an ocean of detail. Nevertheless, this is an important book. Too often, students of politics fail to take seriously the impact of kinship relations on the public sphere. Sagan reminds us to look carefully at the legacy of the family. "The state form of politics," he warns, "has never resolved the problem that it owes its origin to the breakdown and transformation of the kinship system" (p. 372). When the bonds of kinship stood in the way of the development of the human spirit, tyranny—and the fantasy of omnipotence it engendered—acted as a liberator. That fantasy now threatens us. Confusing individuality with power, we have promoted a self-seeking individualism at the expense of community. Freedom requires that we relinquish our longing for omnipotence, and that we seek new forms of community based on equality rather than oppression.

LAURA GREYSON

Rollins College

Women in the History of Political Thought: Ancient Greece to Machiavelli. By Arlene Saxonhouse. (New York: Praeger Publishers, 1985. Pp. xii + 210. \$39.95, cloth; \$12.95, paper.)

This important book raises questions that are central to the study of women and the understanding of political life. It should be

read by advanced scholars and students alike. It is both an examination of the position women occupy in the political thought of several classical theorists, and a critique of the failure of liberal political theory to take adequate account of women. Liberal theory, which ignores sexual differentiation in speaking of human equality, views women only as potentially equal members of the human community who have been denied rights given to men. By contrast, says Saxonhouse, preliberal theorists see the female as "a limit on the possibilities of politics," a member of the polity "whose difference from the male must be acknowledged and incorporated" (p. 9). Her project, then, is to recover that understanding of the social and political world which viewed women and the private world as "a part of, rather than just addenda to, political life" (p. vii).

This she does expertly, and in some instances brilliantly. A preliminary chapter contrasting liberal and preliberal views of political life is followed by chapters analyzing Homer and Greek playwrights, Plato, Aristotle, the Romans (Cicero, Seneca, Virgil), Christian thinkers (St. Paul, Augustine, Thomas Aquinas), and Machiavelli. Her interpretations of the *Oresteia*, *Antigone*, and *Trojan Women* are subtle and convincing explorations of the tragedians' understandings of the ineluctable claims of family as well as those of the city. Her reading of women's role in Plato's *Republic* as a foil for explicating the relationship of the philosopher to politics is controversial but deeply intelligent (for an alternative interpretation see Susan Okin, *Women in Western Political Thought*. Princeton: Princeton University Press, 1979). The chapter on Roman thought is a valuable exploration of texts rarely examined for their perspectives on women. Saxonhouse's reflections on early and medieval Christian thinkers reveals that their acknowledgement of women as men's spiritual equals is often accompanied by a denigration of women's traditional reproductive and family activities.

By rescuing the world of women from the oblivion into which it has fallen in most histories of political theory, Saxonhouse has performed a valuable service. Writing of Virgil's *Aeneid* she remarks that it is "a man's tale, a tale of war," but that throughout the poem "women remind us . . . that human life goes beyond the wars of men, the founding of

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cities, and the building of empires" (p. 119). Saxonhouse, too, holds fast to the totality of human life, believing with the theorists she studies that the "regime" is more than simply formal political structures, and that even when women do not hold public office or take up arms, they are still an integral part of the social and economic relationships that constitute the larger community.

Showing that women do play a role in preliberal political theories does not, however, place these theories as far beyond feminist criticism as Saxonhouse sometimes appears to believe. For example, Saxonhouse qualifies Aristotle's pronouncement that women are "misbegotten males" with inferior deliberative faculties by noting that this natural hierarchy is often subverted, and that some women are more rational than, and hence should rule over, some men. Here Saxonhouse's concern to do justice to Aristotle and women alike excuses too much. And despite Saxonhouse's contention that preliberal theorists value women's activities and roles and often depict the family as resting on "a non-explosive, communal relationship between the male and female" (p. 80) one must ask whether non-exploitative relations are really possible within either the family or the polity, given women's exclusion from, and men's empowerment by, participation in the public realm.

Saxonhouse has shown that however much we, like Machiavelli, may wish to criticize the "masculine" in terms of the "feminine"—and the "feminine" in terms of the "masculine"—with an eye to transforming the very concepts themselves, we will err if we reject the values and activities traditionally associated with the family and women. But the preliberal "solution" of isolating women from the arena of public debate and political action cannot be our own. Saxonhouse's recovery of the classical thinkers' appreciation of the world beyond politics sharpens, rather than resolves, questions about the costs of a gendered division of labor and about how men and women should function as political creatures. For that very reason Saxonhouse's book makes a significant contribution to both the history of political thought and contemporary feminist theory.

MARY LYNDON SHANLEY
AND AUDREY MCKINNEY

Vassar College

Majority Rule. By Elaine Spitz. (Chatham, NJ: Chatham House Publishers, Inc., 1984. Pp. xi + 238. \$12.95, paper.)

Elaine Spitz begins her study of the concept of majority rule by noting the surprising absence of literature directly concerned with that concept. Central to democratic theory and practice, majority rule is all too frequently assumed to be desirable per se, treated as mere procedure, or treated by its critics as a potentially tyrannical flaw in societies that deem themselves democratic. Spitz's intent is to provide a thoroughgoing analysis of the numerous constituent elements of the process of majority rule and to provide a justification for the centrality of that concept in any democratic society. She has succeeded admirably, producing a study that substantially fills the void she notes in the literature and that makes a genuine contribution to democratic theory.

One of Spitz's central themes is that standard discussions of majority rule tend to abstract the concept from the complex world of political decision making and to regard it as a device simply for recording and summarizing concrete preferences held by discrete individuals. In doing so, both critics and defenders of majority rule have been led to disregard the virtues of majority rule that arise from its characteristic practices of "inclusivity, discussion, periodic conflict resolution, and joint action" (p. 215). Hence, Spitz's study includes extensive and informed discussion of suffrage, citizenship, constituency, majority formation and duration, interest groups, and political parties as they combine to produce the milieu in which majority decision making operates. An entire section of the book is devoted to an analysis of the meaning of the word *rule*. Throughout, Spitz exhibits a wide-ranging knowledge of these often disparate subject matters and demonstrates ability to integrate them with her major concern.

Critics will disagree with some—perhaps many—points of her analysis in the first two sections of the work; for example, her disagreement with the Warren Court's apportionment decisions, or her belief that the problem of access to power for disadvantaged groups in a heterogeneous society poses no theoretical barrier to equal effective representation. Such disagreements would not disturb Spitz's major argument that majority rule must

be viewed as a multifaceted and ever-changing process that performs often unrecognized functions in democratic societies.

The final section of the book is devoted to theories, criticisms, and justifications of the desirability of majority rule. Spitz argues effectively that majorities in democratic societies must possess total sovereignty in principle, although in practice that power is subject to delegation or self-limitation (pp. 109-115). Hence, attempts by prior critics of majority rule (Madison, Dahl, Gans, etc.) to limit its power by institutions, logic, constitutions, or the like are misplaced. Similarly, prior efforts to justify majority rule (Locke, Jefferson, MacIver, Kendall, etc.) are flawed or incomplete, because they neglect the communitarian and integrating aspects of majoritarianism, viewing it largely as involving the numerical summation of individual preferences. Finally, Spitz simply dismisses, perhaps too casually, the more radical critics of majority rule, who would argue for a true democratic community based not upon a conflict model but upon shared values, claiming that their vision is based on a "leap of faith" (p. 193).

To persons committed to the principle of majority rule but concerned about its potential excesses, Spitz offers, I fear, a leap of faith similar to that of the radical critics: She recognizes the potential power of the majority to tyrannize, but believes the power will probably not be used. Her belief is grounded in the values that arise from the complex processes of majority rule itself.

Spitz ends the book with her justification for the desirability of majority rule. Properly understood, the concept presupposes, generates, and supports appropriate democratic values, and "the virtues embedded in the practice of majority rule foster both individuality (the creative potential of each) and community (the unified action of all) in order to build a social world in which transcendent universal values can operate" (p. 215).

Readers will without doubt disagree with points of Spitz's *Majority Rule*, but they ignore it at their peril, for it is a first rate contribution to a central concern of democratic theory and practice.

DAVID E. INGERSOLL

University of Delaware

From Colonies to Commonwealth: Familial Ideology and the Beginnings of American Republic. By Melvin Yazawa. (Baltimore: The Johns Hopkins University Press, 1985, Pp. 261. \$28.50.)

Over the past two decades, a renaissance in the study of the American founding era by historians has produced a secondary literature of high quality that must be read by anyone in political science serious about understanding American political thought. Historians like Bernard Bailyn, Gordon Wood, Jack Greene, Trevor Colbourn, Michael Kammen, J. R. Pole, and Jackson Turner Main, to name a few, have returned us to a careful and comprehensive examination of the pamphlets, tracts, letters, documents, and other public records which together provide the only valid basis upon which to base inferences and conclusions about early American political thought.

No longer is it intellectually tenable to do a close textual analysis of a single piece of writing from the era, even the United States Constitution, as if that single text were by itself sufficient for explaining its meaning. Far more so than is the case when analyzing major texts by European authors, texts in American political thought before 1800 are essentially "assembled texts," of which *The Federalist Papers* is but one example. The painstaking rediscovery of the broader political writing of the founding era, both in making these writings readily available and in providing coherent interpretations of their collective contents, is a process in which a good number of political scientists as well as historians have been engaged—witness the work of Herbert Storing, Charles Hyndman, and Martin Diamond, for example.

The work to date suggest that there is more coherence than was once considered the case at the same time that there is more diversity than was credited by earlier scholars. The relatively simple syntheses by Charles Beard, Carl Becker, Vernon Parrington, and Clinton Rossiter must be tested anew in light of what we now know about political thought in early America. One of the litmus tests for the adequacy of any such synthesis must now include an adequate accounting for the role of the concept of virtue. Badly misunderstood by many today because of its debasement by simplistic religious appropriation in recent years, the concept of virtue, we now know, was a com-

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plex, virile, and central component of early American political thought.

Despite the absence of the word in Yazawa's title, his book is in fact of the eighteenth century in America. The first debt political scientists owe Yazawa is his concise, comprehensive summary of what historians have to teach us on the matter. Skillfully working through the pamphlet literature and other sources, Yazawa provides a careful, balanced, and extremely readable summary of what might otherwise require a tedious reading of a voluminous literature, both primary and secondary, to attain.

Sometimes using concepts or forms of analysis borrowed from sociology and anthropology to complement his precise application of the standard techniques of the history of ideas, Yazawa blends it all into a fast-reading, jargon-free essay. The social science content is so unobtrusive that it must be looked for or missed entirely; but the net effect is to help produce an analysis that feels more realistic than is often the case with a pure history-of-ideas approach.

A second debt political theorists owe Yazawa flows in part from this quiet, effective blend of techniques. He is able to bring an understandable and credible coherence to what has increasingly become a source of confusion. How much weight should we give to the influence of the European Enlightenment, the Scottish Enlightenment, the English Whigs (Court or Country), classical sources, or Protestant theology when searching for the origins of American political thought? His answer, indirectly arrived at, is: it depends. It depends on the problem with which you are wrestling, the part of the founding era you are discussing, and what the thinkers of the day felt was relevant.

The crux of Yazawa's book lies in showing the evolution in American political thinking from its domination by the metaphor of the family to its domination by an image of individuals as "republican machines." Put in less abstract fashion, Yazawa demonstrates the continuing importance of virtue for American political thought; but also how the communitarian, hierarchical perspective in which mutual dependence and affection served to define political virtue during the colonial era was replaced by a more individualistic and egalitarian perspective in which citizens are consciously educated to republican virtue.

Throughout the eighteenth century, Yazawa argues, a virtuous population was the bedrock upon which the American commonwealth was seen to rest. The filial basis of virtue served to underwrite all levels of politics, whether local or imperial. Further, this benign, developmental view of virtue inclined the English colonists in North America toward what we now call republican government. Rooted in theology, common law, and a peculiarly English approach to life in general, the familial paradigm of politics—when possessed by people in colonies distant from the mother country—had a certain logic leading toward equal, paternal treatment from England. When this bond of "affection" was not honored by the king and parliament, the logic of the paradigm led toward separation.

Gordon Wood in his seminal *The Creation of the American Republic, 1776-1787* (Chapel Hill: University of North Carolina Press, 1969), argues that the revolution first took place in the minds of the colonists. Yazawa materially advances our understanding of the underlying logic to that mental revolution. Too often historians have used a legalistic reasoning found in some American pamphlets to explain the logic of the revolution. Legal arguments had their effect, but people do not by and large engage in momentous and arduous activity for legal reasons. Yazawa shows how seemingly simple acts or omissions by the crown and parliament set off deep-seated repercussions in the minds of large numbers of Americans habituated by culture and circumstance to view their relationship with England in a familial context. Once breached, however, the familial metaphor was weakened for use as the basis for political virtue at all levels of government.

Furthermore, the underlying commitment to republicanism toward which the familial basis of virtue inclined Americans tended to undermine the bonds of affection upon which the familial perspective rested. Rapid advances in the theory of republican government, accompanied by improvements in republican institutions in a newly-independent America, led to the appropriation of ideas from Europe that shifted the view of virtue. American understanding of virtue became less communal and more specifically political. While still viewing politics and morality as inseparable, virtue came to be defined in more individual terms—

although, despite what Yazawa seems to imply, the communitarian view of politics was far from dead at the beginning of the nineteenth century. Yazawa quotes Benjamin Rush to the point—"Republican citizens must be attached not to each other primarily, but to the republic itself."

The need to attach citizens to the republic itself requires an educational system with specific characteristics, and Yazawa has a brilliant chapter summarizing American literature from the era on the topic of education. Working from the premise that human nature is the same in all ages, and similar causes will produce similar effects (Hume), Americans set out to produce a body of citizens capable of and willing to support republican government, a body of "republican machines." This literature on education is indeed voluminous, and another litmus test of future syntheses must be the incorporation of this body of writing usually ignored by those writing in American political theory.

Yazawa traces the skeletal outlines of a process with enough fleshing out to be convincing, yet the study is not exhaustive. If he is correct, then a rethinking of the "politics of deference" that is often attributed to colonial politics is in order. So must the contents of the Declaration of Independence be reconsidered, without relying upon only European sources. With this new understanding it can now be argued that even in *The Federalist Papers* one can see the notion of a virtuous people lying at the base of Madison's extended republic. Yazawa's work stands as a potential fulcrum in the study of American political thought. To a certain extent it is a summary and synthesis of earlier work, its own contribution to our understanding leads to a rethinking of many of the texts political theorists in our discipline sometimes analyze in isolation. It will be a blessing if those building upon Yazawa's insights will have the same economy of style and superb gift for using the pamphlet literature. Although the work suffers a bit from the current tendency of historians to view all political ideas as simply ideology without concern for their truth value, this is still an important book, and should not be ignored by those outside the author's discipline.

DONALD S. LUTZ

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Locke: An Introduction. By John W. Yolton. (Oxford & New York: Basil Blackwell, 1985. Pp. xii + 162. \$29.95, cloth; \$9.95, paper.)

It is not an easy task to write a brief introduction to the thinking of a great philosopher, let alone to the thinking of one whose interests were as diverse and whose historical impact was as complex as those of John Locke. John Yolton's credentials for undertaking the task are as evident as they are solid. He has lived in some intimacy with Locke's *oeuvre* for well over 30 years. His writings on this *oeuvre* stretch from the admirable *John Locke and the Way of Ideas* (Oxford, Clarendon Press, 1956) to the two stimulating discussions of the subsequent fortunes of key portions of the *Essay* (*Perceptual Acquaintance from Descartes to Reid* and *Thinking Matter: Materialism in Eighteenth Century England*) that he published in 1984. Recently, too, he has taken over the mantle of Peter Nidditch as general editor of the *Clarendon Edition of the Works of John Locke*.

Any attempt to interpret thinking of real intellectual importance in the past is subject to two very different hazards: the risk of failing to capture the sense of its subject's understanding from his or her own point of view, and the risk of failing to convey just what that understanding, seen from the very different perspective of today, really signifies. No account of past thinking of any real power can reasonably hope to succeed perfectly in avoiding both hazards. Few, in fact, are conspicuously successful in avoiding either. The safest introduction to any thinker, as Yolton cannily notes and as Locke himself took the precaution of pointing out in the epitaph he composed for his own tombstone, is that thinker's own works. In the case of Locke these are voluminous in scale, and their purpose is not always very transparent to the modern eye. What Yolton sets himself to do—and on the whole does very well—is constrict the scale sharply and somewhat increase the transparency. His is a gracious and serviceable introduction but not, perhaps, a tremendously commanding one. He is better at performing the introduction itself than at conveying firmly to his readers just why they should require or welcome being introduced to Locke at all.

In practice today many different types of au-

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diences are apt to have occasion to encounter one aspect or another of Locke's thinking. These different audiences (students of several branches of history, of philosophy, political theory, education, divinity, psychology, linguistics) naturally require rather different sorts of introduction. An ideal introduction to Locke for a modern philosopher, for example, is unlikely to be perfectly suited to the needs of a student of the English ecclesiastical establishment and its seventeenth- and eighteenth-century vicissitudes. Yolton confronts the problem of intellectual design posed by this miscellany of audiences by resolutely ignoring it. He presents Locke to his readers not as a figure in one or another course of contemporary university instruction, still less as an insistent imaginative presence in modern thinking about any particular issue, but rather as an old and valued friend, seen sternly but not ungenerously with all his foibles and infirmities of character.

It is not that Locke's doctrines themselves are presented in a wholly uncritical manner. A few discreet doubts are raised about the adequacy of some aspects of his epistemology and cognitive psychology, and one rather severe judgment is offered in the book's concluding sentence about his limited capacity for theoretical or practical empathy. On the whole however, Locke's doctrines are left to speak quietly and plainly for themselves. The most interesting and effective element in their presentation is the stress laid upon the centrality of the concept of person in both his ethics and his metaphysics, and upon the role of

mental activity in determining the content of human belief.

The absence of any very strenuous consideration of the validity of most of Locke's philosophical doctrines is unlikely to commend itself to most contemporary philosophers, for whom the perfect introduction to Locke is apt to be a presentation of his doctrines designed to bring out their marked shortcomings when measured against the deliverances of a good modern philosophical education. At a more relaxed level, much the same is likely to be true for students of political theory, for whom the question of which, if any, elements of Locke's understanding of the nature of politics are still in fact essentially sound is also naturally of some interest. There are many sorts of introductory needs for which Yolton's book will be of little, if any, use. Especially, and gratuitously, unsatisfactory is the very perfunctory coverage of the range and quality of the modern secondary literature. Yet for the rather more advanced student (particularly the one caught up in the thick of modern disputes about intellectual issues on which Locke's thinking has left a lasting impress) this modest, direct, and scholarly account will be an admirable corrective to anachronism and a steadying influence upon the judgment. Locke himself would have had good reason to be grateful.

JOHN DUNN

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AMERICAN POLITICS

The Cross, the Flag, and the Bomb: American Catholics Debate War and Peace, 1960-1983. By William A. Au. (Westport, CT: Greenwood Press, 1985. Pp. xvii + \$29.95.)

William A. Au shows clearly that American Catholics are newcomers to the debate of war and peace issues in his excellent book, *The*

Cross, The Flag, and The Bomb. Prior to the Second Vatican Council's plea for a "new vision of peace," only two groups within American Catholicism were concerned with the peace issue: The Catholic Association for International Peace (CAIP), founded in 1927, working primarily from the just war tradition, and the Catholic Worker movement, which first proclaimed its pacifism during the Spanish

Civil War. Both groups, like the papacy, were more concerned about international issues rather than national ones.

The willingness of American Catholics to criticize their nation in the 1960s and 1970s was a significant departure from the American Catholic tradition of trying to be super-patriots. Dorothy Dohen has described this strain of Catholic patriotism in her book, *American Catholics and Nationalism*. The new attitude of Catholics resulted in a new ethical pluralism in the debate on war and peace issues. Au devises a conceptual framework in which he classifies this ethical pluralism into four conflicting schools of thought: Catholic realism, nuclear pacifism, pacifism, and resistance.

After reading the positions, the reader is amazed by the absence of theologians from the debate of war and peace issues. After the death of John Courtney Murray, S.J., individuals such as William V. O'Brien, Justice George Lawler, Gordon Zahn, and Philip and Daniel Berrigan were the leading spokesmen of the conflicting positions. None were theologians. Also noteworthy is the increasingly critical attitude of the American Catholic hierarchy toward the United States government's defense policies and the draft, the culmination of which was the 1983 pastoral letter, *The Challenge of Peace*.

Au explains how *The Challenge of Peace* avoided a direct confrontation between church and state on the nuclear defense issue. He also shows how the bishops, in striving to attain a moral consensus, ultimately avoided endorsing any one of the aforementioned four opposing schools of thought. Au attributes this accomplishment to the efforts of Reverend J. Bryan Hehir, principal architect of the pastoral letter, who, though himself a nuclear pacifist, endorsed the ethical pluralism of American Catholics.

Au, however, sees nothing to celebrate in the new ethical pluralism. He prefers to be the prophet of doom for whom the conflicting schools of thought are on a collision course that will alienate American Catholics from either their church or their nation. Au fails to stress the achievements of consensus reached by Catholics on issue of war and peace. One of the most important of these achievements was the right of individual conscience as it relates to conscientious objection and selective con-

scientious objection. This was an historic development that was achieved in the 1960s for the first time in the history of the Catholic church.

This is a fine analysis of the ethical issues of war and peace. My only quarrel with the author is his desire for an intellectual consensus on such a complex issue. In a world of theological and cultural pluralism, such unanimity of thought on ethical issues is highly unlikely.

PATRICIA McNEAL

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Discrimination, Jobs, and Politics: The Struggle for Equal Opportunity in the United States since the New Deal. By Paul Burstein. (Chicago: The University of Chicago Press, 1985. Pp. x + 247. \$30.00, cloth; \$12.95, paper.)

The legacy of social policies of the 1960s and 1970s concerned with discrimination and economic opportunity for those on the margin of social, political, and economic life is much discussed, but little understood. Opinions abound. Case studies proliferate. Techniques of data collection and manipulation seem barely altered over time. Comprehensive analysis is confounded by disciplinary barriers—yardsticks, frameworks, theories, and conventions—the language, logic, and liturgies of the not quite social sciences that tempt few to step beyond their specialized niches. Burstein is an exception. A sociologist by training with a track record in the sociological study of politics that is truly remarkable, he has brought together in one volume three fine elaborations of previously published studies concerning employment-discrimination legislation. The book enriches our understanding of the inter-relationship of complex factors, and will enliven future debate. Where he is informed by sound theory and exercises his high standards for exhaustive data manipulation, he is first rate. Where he flounders are in those areas where theory is highly contested, particularly economic models concerning women. There

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his scholarship becomes shaky, his conclusions forced.

Burstein's landscape is broad, and his approach to political sociology meticulous. He begins with the tension between the promise of fairness and the experience of discrimination that led to the organized struggle for equal employment opportunity legislation at the federal level. He self-consciously refuses to consider only periods of crisis surrounding the time that Title VII of the Civil Rights Act of 1964 and the Equal Employment Opportunity (EEO) Act of 1972 passed, and laboriously sets out to catalog another agenda: that this legislation is a direct legatee of the New Deal. He develops a coding system to analyze the hundreds of EEO bills introduced from 1942 on. His aim is to produce the "first attempt to use systematically gathered time-trend data to show how public opinion and the civil rights movement affected congressional action on EEO" (p. 72). He presents twelve or so arrays of data beyond this codification of EEO bills, including congressional sponsorship, attitude surveys regarding minorities and women, analysis of incidence of State EEO and Equal Pay laws, trends in judicial rulings of the Supreme Court, comparison of support for civil rights in principle and practice, incidence of riots, demonstrations, media coverage, and lobbying that may have affected the timing and passage of Title VII and the EEO Act of 1972.

He finds the EEO law essentially conservative. It was "respectful of property and of business, oriented toward conciliation, employing elaborate procedures, focusing on individual cases" (p. 186), hardly a radical trade-off for the cessation of social protest. Why had it passed? What effect had the civil rights movement and these other factors on congressional action? Burstein concludes that public opinion was the fundamental determinant of congressional action. The civil rights movement affected Congress in two ways: drawing attention to the issue of civil rights, and provoking a violent response that increased the salience of the issue for the public. Anti-rights activities had reduced congressional support, but they also increased the salience of civil rights for the public. The effect of these two was not large in Burstein's view, and he considers it impossible to say who gained the most. Essentially it was the power

of the first draft that effectively set the terms of future congressional action. It was essentially an idea whose time had come, that took the form of proposals that had been in existence for 30 years.

The second half of the book dealing with attempts to assess the impact of EEO legislation on the economic status of nonwhite males and females and of white women is limited in two respects. There is no attention given to the extraordinary reorganization of the U.S. Equal Employment Opportunity Commission (EEOC) structure and functions that occurred during the Carter and Reagan administrations, nor to subsequent Executive Orders, or EEOC directives concerning implementation. Burstein states that "even the most radical members of Congress show no sign of developing new legislative ideas to hasten the end of employment discrimination. And there is little pressure on them to do so" (p. 36)—a remarkable observation in the light of a decade of litigation and legislative activity on comparable worth. His construction of a measure of proportionate wealth called *group share* seems similarly misguided in overlooking current research dealing with the economic status of women. It remains an interesting and useful book, despite these reservations; ambitious and certain to stir further research.

ELAINE JOHANSEN

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Socialism and America. By Irving Howe. (New York: Harcourt Brace Jovanovich, 1985. Pp. 24. \$17.95.)

Socialists and the Ballot Box: A Historical Analysis. By Eric Thomas Chester. (New York: Praeger Publishers, 1985. Pp. 192. \$28.95.)

These are very different books. Howe's book tells us a great deal about socialism, liberalism, and America, while Chester offers a narrowly focused historical critique of the American Left's tactics and intends to speak only to a rather small audience. But although quite different, these books actually complement each other. This is because Chester seeks to explain, and to assert the limits of, Howe's politics. Chester assesses the tactics of the Democratic Socialists of America (DSA), a

somewhat influential outgrowth of what was left of the organized Left in the 1960s. Howe and his colleague Michael Harrington (one of the two men to whom Howe dedicates his book) are named on p. 10 of Chester's book as important targets of his critique, for both play important roles in the DSA. And in order to mount his critique Chester treats obscure but still quite interesting corners of the American Left's history. Chester thus sketches in the specific intellectual historical and organizational context of some of Howe's politics.

Still, Howe's analysis has an integrity all its own. The book is not a political tract at all—it is a cultural and social criticism. Howe breathes significant and vivid life into questions which can be and often are dismissed as settled, trivial, or wrongheaded: What explains why the United States does not how have a mass socialist party? What are the relationships between socialism as a theory and liberalism? What would liberal socialism or socialist liberalism be like in institutional practice?

Howe's discussion of the first question is a valuable cross between "national character" and historical organizational analysis. Lucidly written and psychologically perceptive, it is perhaps the best short introduction to the history of American socialism between 1900 and 1940, and to the existing scholarly perspectives on it.

Without ever calling it this, Howe offers, I think, a "track" view of American exceptionalism. Between 1912 and 1919, just as it became moderately successful, the Socialist Party faced a series of organizational challenges that it did not adequately meet. The responses to these challenges locked the party into an organizational limbo. Over time it became less movement-like and more sect-like. Even the Depression failed to revive it organizationally. It became only one among several left movements that were themselves small and organizationally fluid. In short, because the Socialist Party did not take hold during industrialization, the dice were loaded against it during the crisis of industrial capitalism. Ironically, the Communist Party made more of this crisis, but its subordination to the Comintern's policy twists made its success little more than a "brilliant masquerade," to use Howe's term for the Popular Front. The Popular Front was an ersatz version of mass influence that under-

scored the fundamental reality of the left's impotence; that is, its lack of an organic, historically created relationship to American politics. In all, Howe suggests, this is a story of non-development, of socialist politics being "tracked" away from the possibilities for political influence embedded in industrial society.

What, however, of the present? Even though Debsian Socialism never became a mass movement can the politics of democratic socialism say anything to contemporary liberal America? Howe spends the rest of his book on this issue, offering a subtly constructed argument that democratic socialism and liberalism actually sustain each other, in significant part due to the major innovation of the twentieth-century American political economy—the emergence of the welfare state.

The title of Howe's book thus has a different meaning in the second half. Howe daringly suggests that socialism and America have an as yet unknown and linked history. Starting with an analysis of a failed past, he ends with a plausible sketch of a possible future. Socialism may yet be in America's future. Democratic socialists may yet nudge liberal social democracy along, using argument and public debate, toward a polity in which hierarchies rooted in the market become less important than they are now.

This nudging type of politics, however, is precisely what Chester objects to. He subjects Debsian Socialism, the Popular Front, American Trotskyism, and the politics of the DSA to an historical and tactical analysis designed to show that militance rather than nudging has been consistently underestimated on the American left.

Chester's advocacy of militance is axiomatic. He assumes that social democracy and unbri-dled capitalism are equally repressive without ever quite saying why. Chester thus tends to picture nudging simply (and implausibly) as a failure of imagination and will.

Nevertheless, Chester has rather good historian's instincts. He picks up the story where Howe leaves off and in fact offers the first serious account we have of the origins of the contemporary American Left. These go back to the preparations that independent Trotskyists made during World War II for shaping postwar American social democracy. A group of highly anti-Communist American Trotskyists led by Max Schachtman gained practical

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experience in union organizing and in the institutional foundations of American social democracy. They did this by establishing a foothold in the United Automobile Workers during World War II and staying there, a feat made possible by long familiarity with unions and how they work. This experience set in motion events that culminated during the 1970s in the establishment of both the Social Democrats, USA (a group that has exercised considerable influence within the Reagan Administration) and of the Democratic Socialists of America. Chester thus offers very suggestive, useful glimpses into the institutional origins of some of the ideological impulses in our decade.

One book, then, should be in anyone's library, despite the disagreements one might have with it. Howe is a very stimulating writer. As for the other, probably only an institutional library should acquire it. There, it will be of help to certain scholars, some of whom, one hopes, will have just come from reading Howe.

RICHARD VALELLY

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Governing the Ungovernable City: Political Skill, Leadership, and the Modern Mayor.

By Barbara Ferman. (Philadelphia: Temple University Press, 1985. Pp. xii + 281. \$34.50.)

This book is, as the title of the second chapter would have it, a tale of two cities. It offers lengthy case studies of politics in Boston and San Francisco during the 1970's. Since these are—for good reasons—favorite cities among political scientists, and since the case studies are detailed, comprehensive, and very interesting, virtually all urbanists will enjoy reading this work.

The substantive focus is on mayoral leadership. As a result, what we really have are histories of, in Boston, the White administration and, in San Francisco, the Alioto, Moscone, and (more briefly) Feinstein administrations. The author seeks to explore the various ways in which these individuals sought to acquire, maintain, and enhance their own

political power. She finds that all of them relied, to a greater or lesser degree, on strategies that fall into five basic areas: the use of conflict, personnel policy, government reorganization, opportunities presented by federal grant programs, and the development of large-scale public building projects. The case studies are organized, and intertwined, in terms of these five categories. Thus, for example, chapter 4 on personnel strategies, looks at Kevin White's efforts to gain power first by recruiting progressive, good-government types (such as Barney Frank) and later by turning to the tactics of machine politics. The chapter then compares these with the appointment strategies of Alioto and Moscone, which were oriented more toward organized pressure groups. This mode of presentation is effective. It allows the author to contrast the two cases at every relevant point, and also to develop something of a theory of urban leadership. The theory itself strikes me as quite unremarkable. The various strategies, she says, are more or less effective, depending on the mayor's political skills, the city's political culture (public vs. private-regarding), and the political system (reformed vs. nonreformed). While not especially novel, such a view is however, particularly plausible when presented in the context of a typology of strategies and a wealth of case study material. In this sense, then, the book certainly is a significant addition to the analysis of leadership in urban politics.

It is with the case studies themselves that very serious questions arise. The literature of case studies in urban politics has, of course, been deeply concerned with issues of method and evidence. Such a concern is hardly in evidence here. The author explains that the study is based primarily on 123 interviews, roughly split between Boston and San Francisco. She tells us virtually nothing about who was interviewed, how they were selected, what kinds of questions were asked, or why the interviews would be (equally?) believable as evidence. This problem, troubling in itself, is particularly so in the light of the author's fundamental, though generally tacit premise; namely, that most of the things each of the mayors did can be fully explained as an effort to maximize personal power. Consider the following passage on Kevin White:

White's selection of the race issue as a major electoral appeal was strongly influenced by his per-

sonal ambitions for higher office. . . . When White ran for mayor in 1967, he hoped that the office would serve as a stepping stone to the gubernatorial race in 1970. . . . By adopting the race issue, White simultaneously appealed to the black community, which was not represented by any of the other candidates, and to liberal suburban voters whose influence in state politics increased as party lines became more tenuous. The pro-minority stance represented a sharp departure for a Boston city politician. This bold move enhanced White's appeal among his two target groups and helped him to sew up their support early on. (p. 55)

We have here a quite cynical picture of a man and his deep motivations, presented without a trace of evidence. I have my own views of Kevin White and, in fact, they do indeed comport with passages such as this. However, these are merely views, not empirical evidence, and I would be most hesitant to use them as a base for a social scientific theory of leadership in urban politics.

PETER J. STEINBERGER

Reed College

The Federal Government and Urban Housing.

By R. Allen Hays, (Albany, NY: State University of New York Press, 1985. Pp. xvi + 297. \$39.50, cloth; \$14.95, paper.)

This work provides a thorough analysis of federal intervention in the U.S. housing market from the 1930s to present. In doing so, Allen Hays attempts to demonstrate how the ideological orientation of each presidential administration influences both the design of federal housing policy as well as the level of federal commitment. In chapters 1 and 2, Hays develops a theoretical framework for analyzing the general role that political ideology plays in shaping public policy and how ideology specifically influences housing policy. Chapter 3 provides a comprehensive and well-balanced analysis of changes in the housing needs of U.S. families. The issues of affordability (measured as a percentage of family income paid for shelter), changes in housing quality, and the causes of housing inflation of the 1970s are carefully analyzed in this section.

Chapters 4-6 examine each major low-income housing subsidy program beginning with public housing through the current sec-

tion 8 housing assistance program. The author takes issue with those who argue that the homeowner and rental assistance programs established under the Johnson administration (sections 235 and 236) were failures. He provides evidence showing that even though the financial risk involved in these programs was greater than the risk found in more conventional housing assistance program (e.g. FHA mortgage insurance), the failures were blown out of proportion by critics of the programs.

Chapters 7 and 8 focus on federal involvement in urban redevelopment. Again using political ideology as the major determinant of policy formulation and change in this area, Hays begins with the Urban Renewal Program adopted in the Housing Act of 1949, and traces urban revitalization programs initiated by the federal government up to the 1974 Community Development Block Grant Program. Here Hays shows how urban renewal moved from an emphasis on neighborhood destruction in the 1950s and 1960s to a greater emphasis on neighborhood preservation in the 1974 Community Development Block Grant Program.

While Hays is likely to find general support for the argument that liberals and conservatives generally differ in their views about the role of government in the private market as well as about the role that the federal government should play in providing housing assistance for the poor, this glosses over "the coalition building process" that more accurately describes the "winners" and "losers" in the housing policy arena. For example, if U.S. housing policy were truly determined by the ideological orientation of each presidential administration, then one would expect that Mr. Reagan would have found more support in his own administration for his effort to do away with the mortgage interest tax deduction as well as with FHA. Despite the fact that these programs are ideologically inconsistent with the Reagan administration, they are favored by interest groups that are integral parts of the Reagan coalition.

As described in this book, "changes in the ideological orientation of policy-making elites brought on by changes in presidential administrations" serve as the major explanation of changes in U.S. housing policy. Even so, the author is compelled to acknowledge the influences of rising construction costs, changing housing needs, and program inequities that led

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many policy analysts to lose confidence in the low-income housing subsidy programs, developed during the Johnson administration, in favor of programs requiring less direct government intervention (e.g., housing vouchers).

Moreover, in his discussion of urban renewal, Hays shows that even ideologically conservative inner-city businessmen supported government intervention in urban renewal programs, in order to save their investments in the inner city. This discussion once again demonstrates the limitations in the use of the concept *Political ideology* to explain support for federal intervention in the private market.

Overall, this book succinctly and understandably summarizes U.S. housing and community development policy, and provides insight into the complexity involved in policy making in this domestic policy area. Even though the work relies heavily upon secondary sources and does not break much new ground in the housing and community development field, it does provide a timely analysis and critique of policy developments in this rapidly changing area.

BYRAN JACKSON

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The Election of 1984: Reports and Interpretations. Edited by Gerald M. Pomper and Marlene Michels Pomper. (Chatham, NJ: Chatham House Publishers, Inc., 1985. Pp. x + 197. \$20.00, cloth; \$8.95, paper.)

Visions of America: How We Saw the 1984 Election. By William A. Henry III. (Boston: The Atlantic Monthly Press, 1985. Pp. ix + 275. \$17.95.)

Presidential elections are the most thoroughly studied political events in modern times, and these books exemplify the "instant analysis" we have come to expect in the last two decades. The first represents analysis by political scientists and the second by journalists.

Gerald Pomper and associates at Rutgers University have previously reported the 1976 and 1980 elections; this book is the third in a series that is likely to continue. The election story is told, but is clearly subordinated to

analysis. The data on which the analysis rests are polls and election returns.

Of the nominating process in 1984, Pomper says that "President Reagan's renomination marked the transformation of the Republican party into a cohesive organization with a coherent ideology" (p. 1), and that in their Dallas convention the "Republicans were mired in agreement. . . . [They] stretched to fill the free media time" (p. 28). The Democrats once again had changed their rules (the Hunt Commission), this time in favor of an establishment candidate. They had their usual "carnival" nomination struggle, and Mondale "won the nomination in the end because he was the most consensual candidate available" (p. 23). The Democrats had what was for them a remarkably unified and successful convention. Jesse Jackson's candidacy for the nomination and Mondale's selection of Ferraro as his vice-presidential running mate were "firsts" in 1984, with enormous long-run potential importance.

Henry A. Plotkin discusses 1984 campaign issues as "the war of ideas" between the candidates, which rested most notably on their contrasting visions of America. Public confidence in economic recovery overshadowed Mondale's issues of "fairness" and "bad news" warnings about deficits. Considering a wide range of other issues, Plotkin concludes that though many of the voters disagreed with Reagan on particular issues, he "has succeeded in shifting the political agenda of the nation significantly to the right" (p. 56).

Pomper assesses several possible interpretations of the election, including that 1984 "may have moved the nation toward a new conservative and Republican alignment" (p. 60). Mondale, he points out, "won only among blacks, Hispanics, Jews, members of union households, residents of large cities, and persons either unemployed or with the lowest family incomes . . . added together, they constituted only a minority of the total population" (p. 66). The Democrats have good reason to worry about future prospects. First, the South, once solidly theirs, voted more heavily for Reagan than any other section. Second, the children of the "baby boom" did not take on their parents' party identification, and youth turned toward the Republicans in 1984. Finally, the Democrats seem not to have mastered the campaign technology to reverse

the secular decline of party loyalty nearly so well as the Republicans.

Scott Keeter presents public opinion data showing an "upbeat mood" among the people that would not be offset by any forecast of economic difficulties to come: (1) the people liked Reagan personally and perceived him to be a strong leader; (2) the economic indices from 1981 to 1984 almost perfectly fit Reagan's approval ratings; (3) Mondale's issues of "fairness" and "peace" simply could not make a dent in the "good news" campaign of the Republicans; and (4) the "gender gap," from which the Democrats expected a bonus in votes from women, especially considering the Ferraro candidacy, only slightly reduced the margin by which women voted for Reagan, although it may have been of sufficient help to decide some state and congressional contests for the Democrats. Ideologically, "one finds only a glacial shift to the right" (p. 108).

The incumbent advantage, and the fact that very few members of either the House or the Senate retired in 1984, explain, according to Charles E. Jacob, the near-status quo outcome of the congressional races. The very small freshman class in the Senate is more moderate or liberal than the group it replaces. The Republican portion of the equally small freshman class in the House is quite conservative, but the House is still in control of the Democrats.

The conventional wisdom, according to Ross K. Baker, is that "prospects for second-term Presidents are not very bright" (p. 133), and that Reagan seems bedeviled by a want of a clear mandate (because nothing much was promised), a host of economic problems, foreign policy dilemmas, the Democrats, and the potential for a split between the pragmatists and ideologues in his coalition. However, Reagan has the possibility of naming several members of the Supreme Court, which could satisfy the ideologues. He has the ability to focus on the part of government that fits his personal mission ("3-D government of defense, diplomacy, and deficit) and ignore the rest, and has begun a concerted effort to infiltrate the bureaucracy that has been so long dominated by liberals.

Writing on the meaning of the election, Wilson Carey McWilliams says of 1984 that "Americans wanted a moment of relaxation, and Ronald Reagan, campaigning as 'Dr.

Feelgood,' knew better than to prescribe any bitter medicine" (p. 158). Reagan, McWilliams says, ran as a moderate, ditched his party, and was reelected along with many Democrats in Congress because the people desired no great change. He argues that "Mondale is visibly a man of the old order" (p. 164), and that the old pluralistic politics of coalition building are played out, along with the New Deal strategy. McWilliams proposes a scheme for restoring key elements of the New Deal coalition to sufficient allegiance to win again nationally.

The weaknesses in this book, which are few, arise from it being a symposium and from the speed with which it was written and published. The chapters are a bit uneven, and there are occasional disagreements between authors. Despite these shortcomings, this is an excellent continuation of the series. It places the election in a theoretical perspective and it reached professionals and their students without the inordinate delays associated with most publishing schedules.

William A. Henry III is one of a current generation of journalists who have decided to follow in the footsteps of Theodore White and H. L. Mencken in reporting American presidential contests. *Visions of America* is his first effort, and a worthy one. In the preface, Henry says "the story ought to be a narrative" and "that the real campaign is the string of public events that voters observe, not the hidden web" (p. vii). Other, recent journalistic accounts have gotten inside the campaign, relying on both interviews with the strategists and their memoranda to such a degree that they have become quasi-participant-observer accounts. Such is not the case with this book. If you want to know in some detail how Reagan and Mondale prepared for their debates, you will not find it here. Nor will you find out how their staffs reacted backstage to the debaters' performances. There is little "name dropping" of advisers in the book, and almost no appreciation of behind-the-scenes theme-selection or strategy-building. In fact, on only a few occasions does the reader perceive that the author was present at important campaign events. Most of the information conveyed in the book could have been collected by watching television, reading the press, and checking a variety of secondary sources (including *Time* magazine, for which Henry is an Associate Editor). Like a good

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story, the book is well written. It has no footnotes, no bibliography, and no appendices.

The bottom line, as Henry reports it, was that all of the Reagan operatives believed the president was unbeatable, and although the Democrats thought there were five ways they might be victorious, none of their options was probable. Almost everything went right for the President. Henry excellently sums up Reagan and his effect on America:

Reagan apparently had the gift of making people feel good about the country—and the even greater gift of making people feel good about the rich. He dispelled class resentments; he eased the pangs of conscience among those who have toward those who have not; he reconciled his countrymen to inequality, and absolved the prosperous of any hint of shame at making gaudy display of their riches. (p. 209)

As for the Democratic team, once it had silenced most of the Ferraro tax return business, it was beset by the Catholic bishops on the issue of abortion and doomed to a never-ending search for an issue or a theme that could make the Democratic challenge competitive. The first debate (presidential) became the "most influential moment of the campaign" (p. 244), because Reagan did not live up to media expectations, and his bumbling performance permitted journalists to discuss openly the issue of age and competence. The second (vice-presidential) and third (presidential) debates were judged on the basis of different media expectations. In the rematch with Mondale, Reagan won the "sound bite," the best one-liner of the evening, and with it, reelection by a landslide. The people preferred Reagan's "vision of America."

Although *Visions of America* is atheoretical, it goes well beyond the standard "who, what, when, and how" of traditional journalism to treat "why" the 1984 election went the way it did. Accordingly, it is a welcome addition to the literature on one of the most complex political events we political scientists have the responsibility to explain.

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Without Justice for All: The Constitutional Rights of Aliens. By Elizabeth Hull. (Westport, CT: Greenwood Press, 1985. Pp. xi + 244. \$29.95, cloth.)

Citizenship Without Consent: Illegal Aliens in the American Polity. By Peter H. Schuck and Rogers M. Smith. (New Haven: Yale University Press, 1985. Pp. viii + 173. \$22.50, cloth; \$6.95, paper.)

Moments of political crisis tend to provoke statesmen and theorists to reexamine the fundamental principles of a political community. So it appears that the recent "crisis" surrounding the influx of illegal aliens and refugees into this country, and the apparent loss of control over our borders, has stimulated a political response in the introduction of the Simpson-Rodino bill in Congress and an academic response as evidenced by these two brief books. These studies of the rights of aliens and the underlying premises of our immigration policy raise the broader question regarding the nature of citizenship itself. Indeed, as a nation with a long tradition of unrestricted immigration, America has seldom been forced to formulate any specific notion of citizenship other than at the time of its founding and during the recurrent struggle over the citizenship of its black population. With the imposition of the first immigration laws, the polity was forced to articulate a basis for inclusion into the political community for outsiders. While our native literature has told us what it means to be an "American," our political theorists including the Founders have left us uncertain about what it means to be a citizen. Recent confrontations with the problem of illegal aliens and refugee groups suggest that we have yet to articulate a coherent theory of citizenship.

In *Without Justice for All*, Elizabeth Hull focuses her attention on the constitutional rights of aliens. This is a highly readable and informative account of U.S. immigration law and policy, with the central concern the political status afforded aliens—both resident aliens and those who have entered the country illegally. That political status and the protection of constitutional rights is considerably less for illegal aliens. Also legal resident aliens who have entered the country under the terms of the Immigration and Naturalization Act do not enjoy the same constitutional rights as citizens.

Even while the Constitution itself makes certain guarantees to all "persons," the question is whether Fourth and Fifth Amendment rights, due process, and equal protection attach to citizenship only, or are granted to any person legally under the jurisdiction of the U.S. government. The Supreme Court has consistently recognized some restrictions upon the rights of aliens (beyond the requirement of citizenship for holding high office specifically mentioned in the constitutional text). However, in *Graham v. Richardson* (1971) the Court recognized alienage as a "suspect" category, at least in terms of receiving state and social welfare benefits. The tone of Professor Hull's book suggests that she favors a broad movement in this policy direction. Indeed, one gets the impression that she opposes any attempt to distinguish between aliens—whether legal or illegal—and citizens, or to make any discriminations regarding who may enter and join the political community. The implication of such a view is much the same in the Court's holding in *Graham*—it "marks an important milestone in the devaluation of citizenship," as Schuck and Smith suggest (p. 107). Hull's book is a useful guide to understanding U.S. policy toward aliens; however, one may reject the author's attempt to sever the notion of constitutional rights from citizenship. In looking to evaluate the rights of aliens to those of citizens, it is to the Court that Hull inevitably turns: "This country's alien population would benefit significantly if the Supreme Court intervened more actively in its behalf" (p. 151). There is no doubt of such a benefit; the question is whether it is appropriate.

Schuck and Smith recognize the legitimacy and right of a polity to determine the terms of inclusion into its membership. The authors fruitfully spend much time explicating the notion of citizenship in liberal political thought and American legal theory. This effort generates a formulation of two distinct models of citizenship—one ascriptive and the other consensual. While the latter is viewed as more appropriate to a liberal polity, the authors suggest the historical prevalence of the ascriptive model in which the circumstances of one's birth determine citizenship. They outline an alternative approach incorporating a Lockean notion of "tacit consent" thereby emphasizing consensual elements even while recognizing birth as initially determinative.

This theoretical study of the historical origins of these different notions of citizenship and the extrapolation of a concept of citizenship from the U.S. Constitution, the 14th Amendment, and Court decisions is excellent. While the book as a whole is presented as a timely discussion of recent debate over immigration law and policy, its greater value lies in its contribution to understanding the theoretical underpinnings of citizenship and the basis for inclusion into the polity. The publisher offers this work as part of a paperback series designed to "bring subjects of keen current interest to readers as quickly as possible." This is appreciated; however, a topic as important as this deserves an even fuller treatment. One hopes the authors will provide it in the future.

SHELDON D. POLLACK

University of Pennsylvania

To Serve the People: Congress and Constituency Service. John R. Johannes. (Lincoln, NE: University of Nebraska Press, 1984. Pp. xv + 294. \$21.95, cloth; \$10.95, paper.)

Appropriately enough, the very appearance of this review of *To Serve the People* results from "casework," the subject Johannes examines in this book. The publisher discovered that an original review copy had not reached the *Review*. Another was sent, and the regular process went forward—one small instance of rectifying an error or solving a problem, which is what most casework is all about.

Congressional casework is inescapably mundane, and perhaps the greatest virtue of *To Serve the People* is that Johannes never forgets this central truth. At the same time, casework is neither simple nor unimportant. Rather, it meshes, often seamlessly, with the remainder of congressional life—legislation, reelection, oversight, relations with the executive, and so on. In addition, casework bridges the district-Capitol Hill gulf. In that context, this book provides a useful supplement to Fenno's *Home Style* (Boston: Little, Brown, 1978) by demonstrating how members and staff divide their responsibilities and deal with their Hill and home constituencies.

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The book's straightforward organization moves from a relatively limiting definition of casework through discussions of the extent of caseloads and how cases are typically resolved. Subsequently, with a heavy (and appropriate) emphasis on both congressional and executive actors, Johannes examines the structure and process of casework. Many of the book's most valuable insights come in his reporting of how agency and administration personnel react to various forms of congressional entreaties.

The volume continues with three chapters that analyze the effects of casework upon (1) the equitable resolution of constituents' issues, (2) policymaking and oversight, and (3) incumbents' reelection success. Johannes concludes his study with a rather choppy chapter that attempts to assess casework within a broader, ombudsman framework. This approach does not so much fail as appear a bit out of place; by the end of the book the reader is scarcely looking for universal conclusions.

Although Johannes has been involved in some heated exchanges over the impact of casework on election outcomes, this subject receives only modest attention in the book. Of greater importance is the way in which the author combines statistical analyses with a rich collection of interview and questionnaire data. *To Serve the People* stands as an excellent example of how to weave different kinds of data into a coherent whole. It appears that Johannes made a virtue of necessity here. The statistical findings, mostly from probit analyses, are far from overwhelming. Rather than overemphasize some modest results, Johannes chooses to underscore the complexities inherent in the casework process. For example, both congressional staff and executive-branch actors observe that obtaining favorable results for one individual may mean that others in the constituency will come out losers. Casework may not quite represent the simple "pure profit" that Fiorina has claimed.

Upon completing this book, I felt I knew a good deal more about casework than I had beforehand. More importantly, I can think more coherently about casework than I could previously. From this perspective, *To Serve the People* will serve its constituents—students of the Congress—well for a long time. Johannes had the good fortune to begin his research in the wake of the major changes in staffing levels. Although automation will change the

nature of casework, the basic outlines of the process were firmly in place by the late 1970s and early 1980s. For example, most casework had been moved to the district, at least by younger members, and Johannes is able to take this into account.

Congressional scholars could do a lot worse than to emulate this book's approach to "the New Congress." The institution we once understood reasonably well underwent a major transformation in the 1970s. Johannes has successfully examined a small part, as have scholars such as Sinclair (majority leadership) and Kingdon (voting decisions). There remains a good deal to be done.

BURDETT A. LOOMIS

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Presidential Management of Science and Technology: The Johnson Presidency. By W. Henry Lambright. (Austin: University of Texas Press, 1985. Pp. xii + 224. \$25.00.)

With the United States poised on the verge of making a multi-year, multi-billion dollar commitment to the Strategic Defense Initiative, this book on technoscience policy could not have arrived at a more propitious time. What this book has to tell us, however, is not particularly surprising. More than anything else, it is apparent that the scientific community (or "estate" as the author prefers to call it) acts much as any other interest group. Except, and this is a big exception, the scientific community has an institutionalized representative in the White House. One would think, given this sort of access, that the scientific community enjoys a unique advantage in Washington policy debates. This seems to be the case, within limits. Those limits come in the form of problems of implementation, particularly through the bureaucracy. Therefore, this book has both a new and old story to tell. We are reminded of the limitations inherent in the political process no matter how well connected the interest. At the same time, we have the opportunity to observe the institutionalization

of policy making within a new and developing realm of governmental responsibility.

There are, of course, limits to the flexibility of case studies. But, in truth, this book is really not a case study. Students of the Johnson presidency will be disappointed to find that this is a book about technoscience policy that makes reference to the Johnson administration rather than the other way around. This, despite the fact, that the book is third in a series commissioned by the University of Texas Press to chronicle the administration of Lyndon Johnson.

For the specialist or the general reader interested in technoscience policy, however, this book provides a compelling if not conceptually complete framework for analysis. For one thing, it is not entirely clear what are the bounds of technoscience policy. Included in the 24 cases the author has chosen to examine, are issues as diverse as the Mohole Project (intended to get a core sample of the Earth's mantle) and the ABM. Obviously it would be difficult to suggest that many of the same principles apply to the consideration and implementation of these very different projects. Herein lies both the strength and weakness of this book.

To the extent that most technoscience issues fall in the range of low to mid-level policy making, this book gives us a unique perspective on the presidential administrative process. Presidential scholars have a tendency to choose for analysis cases that by virtue of being "major" policy issues are rather exceptional. Major policy issues are often kicked upstairs thereby circumventing the presidential bureaucracy and demanding relatively undivided presidential attention. These sorts of decisions, however, are the exception rather than the rule. The great strength of this book and what will be of most interest to presidential scholars is its focus on lower-level presidential decision making.

The great weakness of this book is that the author may not recognize the potential benefit for presidential studies of focusing on this lower-level process. So much has been written about the ABM and NASA that when the author delves into these programs little is provided that is new or interesting. After all, these are precisely the sort of issues that have been afforded book-length examination. It is when the author discusses the Mohole project or

NIH funding or programs designed to predict earthquakes that we see a side of the presidency which is rarely exposed.

Given that there are characteristics special to the Johnson Administration, such as the penchant for a lack of follow through or the Vietnam War, there is still much to be learned from most of the cases outlined by the author. One only wishes that more attention had been paid to analysis of lower-level decision making. After all, once major issues are labeled national security concerns or special presidential projects, the scientific estate becomes just another lobby, and a minor one at that.

DANIEL PAUL FRANKLIN

Colgate University

Abortion and the Politics of Motherhood.

By Kristen Luker. (Berkeley: University of California Press, 1984. Pp. viii + 324. \$14.95, cloth; \$7.95, paper.)

Since the abortion debate so often supersedes the other concerns of policymakers, candidates, and single-issue voters, one would expect to find many analyses of the politics surrounding this important issue. Unfortunately, however, most of the writing about abortion policies is intended to advance a particular point of view. Kristen Luker's book is a notable exception to that generalization. Luker says her purpose is the discovery of "how people come to differ in their feelings about the rightness or wrongness of abortion" (p. 3), and she presents her findings in a sympathetic but dispassionate manner.

Luker's book provides a history of the evolution of U.S. abortion policies, followed by a case study of the controversy in California over the last two decades. Her information was gathered from interest group literature and records, from newspaper accounts, and, especially, from lengthy interviews with more than 200 pro-choice and pro-life activists. Many quotations from the interviews are included in the book, thereby adding texture and feeling to the narrative.

Because Luker herself is a sociologist, it is not surprising that the strongest part of her book concerns the social background char-

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acteristics and "world views" of the activists. Although most activists are women, Luker describes them as "two very different constituencies of women, two groups that have different experiences in the world and different resources with which to confront it" (p. 8). By comparison with the pro-choice activists, the pro-life women are not only much more religious, but also have more children, are less likely to work outside the home, are less educated, and have lower income levels.

Given the major differences in their social circumstances, it follows that the world view of the pro-life activists should differ greatly from that of their pro-choice opponents. For both groups, the debate over abortion is "merely the tip of the iceberg" of diametrically opposite beliefs about women's roles, sexual behavior, and the nature of marriage and the family. Luker's comprehensive description of the two world views helps to explain why the abortion issue has generated so much emotionalism. She asserts that the debate is really "a referendum on the place and meaning of motherhood" (p. 193) and on "the meanings of women's lives" (p. 194). The activists on both sides have a personal stake in winning the abortion controversy, because a loss would represent "the very real devaluation of their lives and life resources" (p. 215).

Much of the information in this book is of special interest to political scientists. Luker describes the organizational frameworks and resources of pro-life and pro-choice groups, as well as their recruitment patterns, cohesion, and membership involvement. The tactics of grassroots mobilization and legislative lobbying are also explained, along with a critical evaluation of the strengths and weaknesses of the contending groups. Especially insightful is Luker's explanation of the "technology of telecommunications" (p. 219), which has enabled the pro-life women to be politically involved without leaving their homes.

Because the pro-life groups are more active, they receive more attention in Luker's analysis. She also chooses to confine her discussion to legislative lobbying, rather than deal, in addition, with interest group pressures on the bureaucracy and the courts. Furthermore, Luker's study concerns only state and local activists, so similar research still remains to be done at the national level.

Nonetheless, it is likely that Luker's book

will become the best-known study of the abortion controversy, not only because of the dearth of direct competition, but also because of the intriguing insights it offers.

CONSTANCE EWING COOK

Albion College

Black American Politics: From the Washington Marches to Jesse Jackson. By Manning Marable. (London: Verso, 1985. Pp. ix + \$27.50, cloth; \$8.95 paper.)

This book examines the "theory and the historical practice of Black politics: the racial politics of class struggle, the pursuit of power in monopoly capitalist and colonial capitalist societies" (pp. 1-2). Although the author considers racism a manifestation of "false consciousness," he acknowledges that it is a powerful social force—a force that is logically and functionally related to class inequalities and oppression in the bourgeois democratic order.

Marable offers a provocative theory of black leadership. Black electoral politics is best understood as the "history of the class-conscious Black petty bourgeoisie seeking to influence the bourgeois democratic or colonial-capitalist state for its own purposes. Such political intervention may or may not serve the social class interest of the Black majority" (p. 174). Among the presumed reasons for this dereliction of leadership is the petty bourgeoisie's unwillingness to commit "class suicide," its propensity to be self-serving and self-interested, and its failure to understand that racial equality cannot be achieved within the liberal democratic state. The most predictable characteristic of this class is its "political and ideological inconsistency" (p. 157).

This theory of black politics is applied to the analysis of the 1941, 1963, and 1983 marches on Washington, D.C.; the electoral politics of Chicago, including the 1983 election of Harold Washington as mayor; and the presidential campaign of Jesse Jackson. In each case, Marable is critical of the black middle-class leadership—especially the National Association for the Advancement of Colored People—for its role in dampening the militancy of these movements. That the Washington and Jackson campaigns produced advanced agen-

das is attributable in part to the initial reluctance of many main-stream black leaders to be identified with the candidates, thus leaving a vacuum into which left-progressive leadership moved. Jackson himself "may best be understood as an ideological weather vane, an ideological and changeable indicator of the mind and mood of Black America" (p. 266).

In Marable's view, the next stage in the black struggle should be a revolt against the Democratic party in order to force the petty bourgeois black leaders to "either commit class suicide or openly defend the status quo" (p. 189). This move would pave the way for "militant, uncompromising leaders [to emerge] from the oppressed social classes" (p. 189).

Given the legitimacy of liberal democracy in the United States, the low level of class consciousness, the extent and character of white racism, and the deficiencies of black leadership that Marable asserts, this reviewer cannot see as realistic the scenario for the socialist revolution he describes. Neo-Marxists, however, tend to see history as more teleological than the reviewer. To them, social movements are "pre-revolutionary modes of social class struggle which set the historical stage" for the social revolution (p. 78). Accordingly, the revolution will simply be the culmination of the many struggles—for example, the marches on Washington, the Jackson campaign—that precede it. Whether this logic salvages the argument remains to be seen.

It is difficult to do justice to this complex book in a brief review. Partly, this is because of the ambitious scope of the work; partly, it is because of the way it is organized and written. An example of the latter difficulty is that the chapters are too long (the first is 74 pages), and are broken internally only by Roman numerals. Moreover, the prose tends to be cumbersome and obscure. Marable uses statistics frequently, but includes no tables; all the numbers are spelled out in the text.

Still, because of its historical reach, I would recommend the book to anyone interested in black politics. My reservations about its theoretical underpinnings notwithstanding, I would especially recommend it to students of black political leadership.

TOBE JOHNSON

Morehouse College

Beyond Entitlement: The Social Obligations of Citizenship. By Lawrence M. Mead. (New York: The Free Press, 1986. Pp. xi + 318. \$19.95.)

Early in this thoughtful, thoroughly researched, and fervently argued book, Lawrence Mead makes his argument clear:

The thinking behind [welfare] policy in Washington has been too narrowly economic. The programs claim to serve equality, but essentially all they do is transfer benefits to recipients. As long as equality in America requires competence as well as income, programs must set standards as well as support people. The most vulnerable Americans need obligations, as much as rights, if they are to move as equals on the stage of American life. (p. 17)

Near the end of the book, Mead lists what "seem to be the main social obligations expected of adults in the United States today." His list begins with work in available jobs for heads of families, unless aged or disabled, and for other adult members of families that are needy. It goes on to include contributing all that one can to the support of one's family; fluency and literacy in English, whatever one's native tongue; learning enough in school to be employable; and law-abidingness, meaning both obedience to law and a more generalized respect for the rights of others.

Why is it that these main social obligations expected of the general run of adults are not, in Mead's view at least, expected of welfare clients? In a word, over-permissiveness. In a paragraph:

New Deal policy had confined government's responsibility to providing certain new economic protections and opportunities; it was the responsibility of ordinary people to make good use of them. In the Great Society conception the public responsibility was potentially unlimited. Government could never say it had done enough for the poor, because the burden of change was not shared with them. That feeling helps to explain the restless innovation of Great Society policy. Planners had to keep coming up with new programs until they found something that "worked," that is, some benefit to which poverty responded without obligating the poor themselves. (p. 59)

Because the Great Society emphasized society's responsibility to the client rather than the client's equal responsibility to society, Mead

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tags the programs of the Great Society as the cause of today's welfare woes. He notes the sharp increase—from 63% to 91%—between 1967 and 1970 in the share of eligible female-headed families actually on welfare, and correctly concludes that "something dramatic happened to break down the stigma surrounding welfare." That "something" was a change in governmental attitude at federal and state levels from a mean-spirited reluctance to honor their own welfare statutes to an overdue willingness to abide by the spirit of the thirty-year-old law. Just so, yet the change is not something of which Mead approves, because "it can demoralize recipients" (p. 64).

Neither Mead's careful scholarship nor his deft presentation of the results of the negative income tax experiments, however, preclude arching an eyebrow over the apparent implication here that public benefit programs should be showpieces, that their merits are greater when they are passed over by the legally-eligible population. Can it be, for example, that civic obligation would best be served if an ever-larger number of taxpayers simply passed over the personal exemption that is explicitly and unequivocally provided for in the Internal Revenue Code, and by so doing significantly cut away at government tax expenditures? Should there be a massive campaign to stigmatize high-income persons eligible for social security who do not follow President Reagan's example and decline to file for benefits? The point need not be dwelt on. A government of laws demeans itself if it writes laws—whether restraints or benefits—and allows those who simply follow the law to be marked with a scarlet letter.

Welfare law, to be sure, becomes Mead's ultimate target; sensible restructuring of laws and regulations to obligate beneficiaries to take work more seriously emerges as the ultimate message. The book seemingly espouses welfare paternalism of a type practiced in Switzerland, most recently described by Ralph Segalman in the Winter 1986 issue of *The Public Interest*. If it begs the question of how government's willingness to take on the posture of enforcer of civic obligations is affected in the United States by race, class, and regional complexities, the sure-footed tone would encourage a belief that Mead does not necessarily shy away from that thicket. One hopes he may take it on as a later stage in the development of the argument.

Whatever one's reaction to the policy position, Mead's effort stands as a classy model of public policy analysis. *Beyond Entitlement* provides as good an overview of the literature on the politics of welfare as has appeared during the last 25 years. The author argues his position with passion and decency. In the process of so doing, he also explains a lot about the development of social policy.

GILBERT Y. STEINER

The Brookings Institution

Unnatural Monopolies: The Case for Deregulating Public Utilities. Edited by Robert W. Poole, Jr. (Lexington, MA: D.C. Heath & Company, 1985. Pp. xiv + 224. \$25.00, cloth.)

This book is not—and does not purport to be—a well-balanced analysis of arguments for deregulating public utilities. The editor and the authors all agree that deregulation is desirable. From their perspective, the question is not whether to deregulate but how and when. In seven articles that focus primarily on telephones and cable television, they attempt to make a case for radical deregulation. Unfortunately, they do not succeed.

A key problem is that the authors produce little evidence to support the propositions that deregulation will ensure competition and that competition will promote efficiency. Instead, they rely heavily on theoretical arguments that will please those already enlisted in the cause but will not persuade more skeptical readers. For example, Cornell and Webbink denounce rate-of-return regulation as "an unacceptable fraud on the public." Utility regulators cannot hope to make sound judgments, they contend, because they depend exclusively on regulated firms for data. This assertion exaggerates the importance of raw data and ignores the growing capacity of public utility regulators for independent data analysis. Public utility commissions now have large professional staffs to analyze data and audit utility company books. Moreover, one-third of all public utility commission staff members have had some experience working with regulated firms, so that

they are not easily hoodwinked. Ironically, the "revolving door" helps regulators to challenge utility company interpretations of raw data. In addition, regulators often benefit from submissions by consumer advocacy groups, some of whom present their own econometric models, forecasts, and rate schedules as alternatives to those advanced by utilities. These observations apply more to state and federal regulators than their local counterparts. Regrettably, the differing abilities of different levels of government to regulate in the public interest are never discussed.

Two of the authors do make a systematic effort to examine the consequences of direct competition as an alternative to monopoly. In an interesting piece on cable television, Hazlett cites data showing that cable television prices are no higher in cities where cable television companies compete directly for customers than in other cities. Without data on profits, it is difficult to know how long this will last. Also, without controls for population density, penetration rates, and other variables, it is difficult to draw inferences from these findings. Still, the article is worth reading. In another interesting chapter, Primeaux concludes that direct competition between electricity distribution firms results in lower average costs, at least in smaller cities. For reasons he explains, Primeaux limits his analysis to municipally-owned firms, although they account for a very small fraction of the total electricity output in the U.S. Unfortunately, municipally-owned utilities are not easily compared, because tax payments and tax-equivalent payments are handled differently in different cities. Moreover, Primeaux's data, from the 1960s, antedate a whole series of important changes in the economics and politics of public utilities. In particular, regulators in the 1970s took a variety of steps to improve the performance of utility monopolies (management audits, incentives to lower costs, etc.). Primeaux's data, nearly 20 years old, tell us nothing about whether these efforts have succeeded. Nevertheless, this article will be of interest to specialists in the field.

Readers who study public utilities will learn something from this book. There is useful information on historical trends and developments (Hazlett, Samuel, Mellor and Allen) and on new technologies and their implications (Samuel, Jackson). Several of the articles,

though flawed, are provocative (Webbink and Cornell, Primeaux, Hazlett). Also, the book's single-minded enthusiasm for deregulation is tempered somewhat by brief critical comments following six of the seven chapters. Although the commentators seem sympathetic to deregulation, they do not hesitate to point out weaknesses in the authors' arguments. Moreover, their criticisms are typically concise and incisive.

Yet, these brief comments by discussants cannot compensate for other problems. The authors are not sufficiently sensitive to the complexities of regulation today—the multiple purposes of regulation, the different capacities of different levels of government, the potential for innovation within a regulatory framework, and the extent to which interest groups other than utilities make valuable contributions to the regulatory process. The recommendations are stronger than the evidence on which they are based, as several of the discussants note. Finally, this is a book that could have benefited from stronger editing. The editor's introduction is brief, and the reader must wade through some pretty difficult material with little help. There is no concluding chapter in a book that desperately needs one. In addition, one wonders why so much attention was paid to telephones and cable television (the focus of four of the seven articles), while other utility monopolies were ignored (natural gas companies, water and sewer utilities, refuse collection, the U.S. Postal Service). Is the case for deregulation weaker in these domains or was it difficult to find contributors on these topics? Without more information from the editor, it is difficult to know for sure.

In conclusion, this book is somewhat disappointing. It is narrow in its perspective and its scope. While specialists in the field may wish to read two or three of the articles, there is little to commend the book to a wider audience.

WILLIAM T. GORMLEY, JR.

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American Domestic Priorities: An Economic Appraisal Edited by John M. Quigley and Daniel L. Rubinfeld (Berkeley: University of California Press, 1985. Pp. xv + 398. \$32.50. cloth; \$9.95 paper.

The Reagan Administration's "New Federalism" proposals represent a major departure from the trend since the New Deal of active federal involvement in state and local policies. This collection of essays by 12 distinguished economists poses two interrelated questions about the New Federalism: What would the costs and benefits of transferring public functions from the federal government to the states? and, Is it possible to reduce the federal deficit through cutbacks in federal domestic programs?

Perhaps the most interesting feature of these essays is the general consensus that reducing federal control over domestic programs (with the exception of poverty and welfare) and increasing the control by states and localities could in many cases result in a more efficient allocation of resources. This conclusion rests on the argument, presented by Edward Gramlich, the "Public goods [are] to be provided by the jurisdiction covering the smallest area over which benefits are distributed—that way the public-goods efficiencies are maximized and the effect of taste differences is minimized." This conclusion is surprising since it comes from a group of economists who, in the past, often advocated a strong federal role in domestic policy. This endorsement of a more decentralized domestic policy approach is a measure of the New Federalism's success in transforming the terms of the policy debate.

Despite endorsing more decentralization, John Quigley, Daniel Rubinfeld, and the other contributors to this volume do not endorse the Reagan Administration's New Federalism proposals. Indeed, they persuasively argue that the New Federalism proposals were primarily designed to reduce federal spending by transferring costs of public functions from federal to state and local authorities, *not* by a desire to transfer power to the states. As Henry Aaron points out, the New Federalism was a "raw deal for the states." In the long run, they would lose a lot of federal money and take on new responsibilities. If the major purpose of the New Federalism was to transfer power to the states, the Reagan proposals could have sweetened the pot financially for the states.

In their concluding essay, Quigley and Rubinfeld assert that neither cutting federal domestic programs (other than Social Security) nor transferring control of these programs to the states offers much promise of substantially reducing the federal deficit. They advocate instead increasing federal tax revenues by disallowing federal deductions for mortgage interest payments and for state and local taxes, and by removing the federal tax exemption for obligations issued by state and local governments. They argue that this set of federal tax breaks disproportionately aids higher income households because it benefits homeowners and taxpayers who deduct. They also argue that these tax breaks will distort state and local spending decisions since some of the costs of these decisions will not be borne by local taxpayers.

Despite a general agreement that domestic policy making might benefit from a more decentralized control, the economists in this volume disagree considerably about particular fiscal issues or policy strategies. These disagreements will be informative and provocative for students of economic policy. Gramlich, for instance, questions economists' conventional wisdom that economic stabilization policies are best conducted at the federal level, and presents an ingenious case for subnational fiscal policy. Quigley and Rubinfeld disagree with Gramlich. The difference between these two positions rests on different assessments about the mobility of labor and goods across regions.

There is also disagreement over the proposal to eliminate deductions for state and local taxes. Gramlich, Aaron, Quigley, and Rubinfeld recommend eliminating these deductions for the equity and efficiency reasons discussed earlier. George Break, on the other hand, argues for federal deductibility on the grounds that people should be taxed on their discretionary income; that is, payment of state taxes reduces the income available to pay federal taxes. Break recommends changing the federal deduction for state taxes into an exemption so as to eliminate differences in tax treatment between those who deduct and those who do not deduct.

These essays are quite ambitious. The authors provide an overview of major policy areas (e.g., housing, environment, transportation) in which they review policies in the areas, assess the research evidence about the useful-

ness of various policy strategies, predict the results of funding cuts, and propose alternative policy strategies. A good example is Jose Gomez-Ibanez review of the federal role in transportation. Gomez presents a compelling case that the availability of discretionary mass-transit grants has resulted in the building of many new and probably unnecessary rail systems.

John Kain's chapter on black suburbanization in the eighties is excellent. Kain shows that there is still considerable segregation in both cities and suburbs and that this segregation is not primarily due to the low incomes of blacks. He notes a very hopeful new phenomenon. In an analysis of residence patterns in four metropolitan areas, he finds an important qualitative change between 1970 and 1980 in blacks' living arrangements. In 1970, many suburbs had less than five black households; by 1980, most suburban communities had more than five black households. Kain argues that federal support of civil rights and fair housing likely contributed to this trend and strongly endorses continued federal support for civil rights and fair housing.

In sum, this book on *American Domestic Priorities* provides a lively, informed, and solid discussion of whether the federal fiscal system ought to be restructured, and of a wide range of domestic policy issues. The essays in this book should prove a valued resource for teachers, policy researchers, and policy makers.

MARY CORCORAN

The University of Michigan

Neighborhood Organizations: Seeds of a New Urban Life. By Michael R. Williams. (Westport, CT: Greenwood Press, 1985. Pp. xiii + 278. \$35.00.)

How realistic is it to expect that disadvantaged citizens can influence urban policy decision making at the local level? This is the central question that Michael Williams poses in *Neighborhood Organizations: Seeds of a New Urban Life*. Drawing upon the organizational behavior and citizen participation literature and his own experiences in neighborhood ac-

tivism since the late 1960s, Williams offers an analysis of how neighborhood organizations can mediate the concerns of the disadvantaged vis-a-vis corporate and governmental influences in America's cities.

The notion of neighborhood organizations as mediating institutions was originally developed by Peter L. Berger and Richard J. Neuhaus in *To Empower People: The Role of Mediating Structures in Public Policy*. (Washington, DC: American Enterprise Institute, 1977). Williams extends their arguments by suggesting that advocacy-oriented neighborhood organizations can redefine the problems of the have-nots who reside in urban America. Yet Williams recognizes that neighborhood organizations need the financial support of a federal government committed to reinvestment if the disparities between the "haves and have-nots" are to be addressed.

The book is organized into four parts. Part 1 examines the setting for neighborhood organizations and competing conceptions of neighborhoods, while part 2 studies the internal dynamics of neighborhood organizations. Drawing upon concrete examples of neighborhood activism in the areas of energy, crime, and education policy, Williams argues in part 3 that the central task of neighborhood organizations "is the development of an advanced level of knowledge, participation skills, and organizational abilities within the community" (p. 147). It is in part 4 that Williams outlines his plan regarding how citizens can play the key role in urban service delivery. A useful concluding bibliographic essay summarizes the neighborhood organization literature discussed throughout the book.

While Williams offers a spirited argument on behalf of neighborhood organizations, he fails to address several key issues. Williams ignores the likelihood that neighborhood organizations increasingly will be competing against one another in a zero sum environment of fiscal retrenchment at the local level. Little is said regarding the impact of the Reagan administration's cutbacks of programs such as the Urban Development Action Grants (UDAG) and the Community Development Block Grants (CDBG) upon urban service delivery.

Williams does identify the key conflict between citizen participation and technical ex-

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expertise that confronts community organizers at the local level, but he fails to offer persuasive suggestions regarding how citizens might be provided with the technical expertise needed to understand the intricacies of urban policy initiatives. It may be too much to expect that neighborhood leaders will be able both to mobilize and to educate the citizenry. Perhaps urban planners can play a valuable role here.

A central theme of the book is the importance of conflict between community group members and decision makers if the concerns of the "have-nots" are to be protected. Yet Williams does not adequately specify the kind of conflict that will rekindle "the fragile and abused ideal of democracy" (p. 25). He might also have outlined more clearly how this conflict will hold corporate elites and bureaucrats accountable to the citizenry.

The book does not clearly identify how citizens will be mobilized to participate in neighborhood organizations. For example, Williams ignores the fact that many urban neighborhoods are artificial boundaries created by urban planners. Citizens living in these artificially created neighborhoods may not have the sense of community that the author feels is necessary for active participation in neighborhood organizations. To suggest that local activists will mobilize large numbers of the disadvantaged residing in urban areas may be overly optimistic. Problems of time, lack of resources, and an absence of program information are barriers that must be overcome.

Despite these shortcomings, *Neighborhood Organizations: Seeds of a New Urban Life* should be required reading for students of urban policy, citizen participation, and organizational behavior. Williams has offered a useful contribution to the literature regarding the role of neighborhood organizations in a viable democracy.

CRAIG A. RIMMERMAN

Hobart and William Smith Colleges

The Rise of Modern Judicial Review: From Constitutional Interpretation to Judge-Made Law. By Christopher Wolfe. (New York: Basic Books, Inc., 1986. Pp. xii + 392. \$24.95.)

When policemen broke into his bedroom, one Rochin hastily swallowed a pill that lay on the bedside table. The cops dragged him out of bed, hustled him to the station, and forced a stomach pump into him. The disgorged pellet was the decisive evidence that led to his conviction on a narcotics charge. But in the end he was freed. Justice Frankfurter, for the U.S. Supreme Court, said that he had been deprived of his liberty without due process of law, because the conduct of the police "shocked the conscience." Frankfurter's frequent antagonist, Justice Black, agreed that Rochin had been unconstitutionally convicted, but for a different reason: Rochin, Black said, had been compelled to testify against himself, and such compulsion violated the Constitution. Black was scornful of Frankfurter's position. If the test of constitutionality, he said, was "the conscience," then the Court would decide cases on the basis not of the Constitution, but of the judges' own sensitivities, preferences, "consciences." The Court should not do this. In recent decades, the justices had struck down laws regulating business, ostensibly because such laws violated the due process clause, but really because they disapproved of such regulation. They had improperly exercised a legislative judgment. So Black argued—but was he not himself legislating, when he said that being subjected to a stomach pump was the same as being compelled to testify against oneself?

If Christopher Wolfe had dealt with the *Rochin* case, he would have argued that yes, both Frankfurter and Black were turning judicial review away from its proper function of interpreting and applying the Constitution. Instead of just stating and enforcing the fundamental law as propounded by the framers two centuries ago, the judges were making law in accordance with their own ideas of what the law and the Constitution should be. The author, rather disarmingly, suggests that his own preference, like that of Attorney General Meese, is for judicial review based on the intentions of the people who drafted and ratified the document. Stomach pumps were unknown

in 1791, when the due process clause and the prohibition of compelled self-incrimination became constitutional clauses; ergo, neither clause could help Rochin in 1952.

However, Professor Wolfe does a remarkable job of muting his own predilection and presenting a clear, readable, and fair account of the development of judicial review. He divides the history of it into three periods: first, from *Marbury v. Madison* to 1890, nearly nine decades when, on the whole, the Supreme Court stuck to traditional concepts of interpretation (a glaring exception was the *Dred Scott* case); second, 1890–1937, the period when the justices (Holmes dissenting) substituted their own preference for laissez-faire economics for that of Congress and state legislatures, and so themselves became, in effect, legislators; and third, the modern era of “judicial activism,” when the Court, while no longer finding that the regulation of business is made impossible by the due process clause, has elevated the First Amendment to a preferred place in the constitutional constellation, and in so doing has departed from the intentions of the framers. All of these chapters, plus critiques of the relevant ideas of, among others, Woodrow Wilson and Ronald Dworkin, are worth reading, for legal scholars and laymen alike. They well serve the author’s stated purpose: “the education of the American people to the true character of modern judicial review.”

At the end, Wolfe speaks yearningly of the Constitution not as a “living Constitution” that is wax in the hands of those who apply it, but the Constitution fairly interpreted to faithfully express the meaning it was given by its authors and understood by those who gave it authority by ratifying it.” We may agree with him that the federal judiciary is not a democratic institution, and still ask if democracy would really fare better if the courts went back to “traditional interpretation.” For example, the Sixth Amendment gave defendants in criminal cases the right to have “the Assistance of Counsel.” The authors of that amendment meant only that if a defendant wanted a lawyer at his trial, he could retain one. The idea that the State should appoint counsel for an indigent defendant never occurred to them. Yet in 1963, in *Gideon v. Wainwright*, the Supreme Court held that the Constitution gave the penniless Gideon the right to have the State appoint a lawyer to represent him. Should this decision now be overruled, because the Court read into the Sixth Amendment something that had not been in its authors’ minds? That is the kind of question that Professor Wolfe, quite properly, asks us to debate.

THOMAS H. ELIOT

Cambridge, Massachusetts

COMPARATIVE POLITICS

Redesigning the State: The Politics of Constitutional Change in Industrial Nations. Edited by Keith G. Banting and Richard Simeon. (Toronto and Buffalo: University of Toronto Press, 1984. Pp. xii + 257. \$30.00, cloth; \$14.50, paper.)

This volume is an outgrowth of papers presented at a conference held at the Institute of Intergovernmental Relations of Queen’s University, Kingston, Ontario, in April 1982. The conference took place precisely when the final accord putting an end to some 50 years of constitutional stalemate in Canada was

reached. Against this backdrop, the conference must have been an extraordinary intellectual experience. The goal of the conference was “to bring together the experience of constitutional change from different settings and to trace the complexities of constitution making in particular cases” (p. 3). The pursuit of this goal consists of nine readable and interesting essays on European and North American case studies and general interpretations.

In the introductory essay, Keith G. Banting and Richard Simeon concern themselves not with comparing different constitutional designs and the prospects for reiterating con-

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stitutional choice, but with processes of attempted or actual constitutional change. They discuss several distinctions: the meaning and significance of constitutions and constitutional alterations; the sources of demands for constitutional change; the issues and interests at stake; the mechanisms and processes of constitutional debate; and, finally, the outcomes and their consequences. These distinctions do not appear to be derived from any theoretical construct. Instead, they appear to be distillations of positions that emerge from the case studies.

The case studies cover different types of political systems. Gerhard Lehmruch sketches the recent evolution of West German federalism. Richard Gunther examines the constituent process that took place in Spain following the death of Franco, and suggests reasons why the institutional reforms that resulted may be viewed as among the most far-reaching ever attempted in a modern industrialized society. Maureen Covell details revisions of the Belgian unitary state, including the 1970 agreement to create regional institutions, as a response to a long-standing conflict between that country's two language groups. Alan C. Cairns surveys the Canadian constitutional development that led to the proclamation of the Canada Act in April 1982. James D. Kellas explores the referendums in Britain over issues such as devolution, the European Economic Community (EEC), and local government finance in order to place in sharp relief the challenge of constitutional change in a country with no fixed procedures for constitutional amendment. Stephen Schechter traces and explains the politics of recent constitutional amendments in the United States as part of the mobilization of successive generations. Finally, Teresa Rakowska-Harmstone assesses the prospects for fundamental changes in Eastern European nations, where the monopoly of power exercised by the Communist party is not subject to constitutional constraints. The major generalization that emerges appears to contradict somewhat the very title of the book: there are little or no prospects for redesigning the state. As Tocqueville noted so well in his work on France, serious limitations exist in the politics of constitutional change. Once a particular organizational arrangement is in place, it is exceedingly difficult to undertake reforms that will lead to the transformation of the

system into a different system based upon a radically different design. The essays by Richard Gunther, James D. Kellas, and Teresa Rakowska-Harmstone are especially insightful in this regard.

It must be added, however, that the collection of essays introduces few issues, arguments, interpretations and methods of analysis that are not generally known to individuals working in the particular areas. I say this as a description, not as a criticism. In the main, the collection makes a convincing case for viewing constitutional decisions, as Daniel Elazar points out in the concluding essay, as the preeminent political issues a country faces—a truism that needs restating “after a generation of withdrawal on the part of many political scientists from consideration of all that is labelled ‘constitutional’ in the world of government and politics, on the grounds that such matters are merely formal and hence not ‘real’ ” (p. 232). The concluding essay is, in fact, vintage Elazar, original and suggestive in the way it tries to pull together some of the interpretive threads presented in this volume. Unfortunately, it is not, perhaps because of limitations of space, a sustained discussion of constitutional models and paradigms, but, in Elazar's own words, “a modest first step in that direction” (p. 247).

The volume succeeds in overcoming the parochialism of much constitutional debate in Canada and elsewhere. It deserves attention from Canadianists and comparativists alike.

FILIPPO SABETTI

McGill University

West German Politics. By Lewis J. Edinger. (NY: Columbia University Press, 1986. Pp. 330. \$30.00, cloth; \$10.00, paper.)

Policy and Politics in the Federal Republic of Germany. Edited by Klaus von Beyme and Manfred G. Schmidt. (NY: St. Martin's Press, 1985. Pp. 239. \$39.95.)

These two books are notable contributions to our understanding of contemporary West German politics. Edinger has written a general and largely introductory account of politics and public policy in the Federal Republic,

while von Beyme and Schmidt have brought together a number of original essays on policy making by German political scientists. A common feature of both books is their emphasis on the legal, political, cultural, and socioeconomic determinants of German public policy. They focus on the present, but are mindful of the historical and international context in which German politics is played out. While both rely heavily on empirical findings, they nevertheless hazard fresh interpretations of old and new data.

Edinger's is the more comprehensive—and better written—of the two books. A long-time student of German politics, he presents his "fourth major effort in about three decades to interpret ongoing political developments in the German Federal Republic" (p. xiii). His purpose is to map the changes that have taken place in West German politics, particularly in the 1980s, and to extrapolate from these changes possible scenarios of the future. His net judgment on West Germany is not set forth in any one place, but it can be summarized as follows: The Basic Law and the institutions created by it, including the federal structure and the formal framework of party government, have matured beyond the "possibility of significant alterations" (p. 319); not only is there "broad elite support for [present] political arrangements" (p. 132), but also, "most West Germans accept the legitimacy of the prevailing patterns of political authority" (p. 94); nevertheless, the body politic is ill, and the illness, while not serious, needs to be monitored.

Edinger's diagnosis focuses partly on the state of the German public mind and partly on the pattern of German politics. We find, for example, that popular participation in politics is insubstantial; that Germans are afflicted with a "low sense of political involvement and efficacy" (pp. 97 and 148); that they lack cohesive community sentiment (pp. 100–103); that the "average person has little or no control over public policy" (p. 104); that pessimism about the future pervades the young, especially the better educated (p. 105); that political leaders are coopted from the top rather than produced from below; that "the stability of the West German party system is . . . open to question" (pp. 178–179); that there are "severe restrictions on the formation and survival of new parties" (p. 144); that policy making

results largely from group negotiations or a kind of "functional representation" (p. 188) "marked by the corporatist vestiges of a preindustrial society" (p. 219); and that "West Germans still look beyond these groups to the authority of the state as the supreme guardian of law and order and ultimate arbiter of socioeconomic conflicts" (p. 92).

When the reader confronts such statements about Germany's contemporary condition, he is not always sure whether they should be taken as criticism, although in most instances Edinger appears to be underscoring defects in the body politic. Because this book is written at such a high level of generalization, the reader coming to German politics for the first time is unaware of certain historical details and institutional considerations that would help to explain broad generalizations about politics in Germany. When, for example, the author tells us that the "Basic Law does not explicitly recognize interest associations, but . . . grants all citizens the right to form . . . groups provided they are not 'directed against the concept of international understanding'" (p. 182), the reader cries out for further explanation. The author deliberately sacrifices concrete details about political and legal institutions in quest of more universal propositions about the "policy environment," "political orientations," "Politicization and participation," and "party politics," all for the purpose of setting the stage for the discussion of policy making and public policy in the book's last three chapters.

These concluding chapters, which make up almost one-third of the book, are an excellent discussion of the problems and consequences of German public policy. Richer in detail than the previous chapters, they not only identify the forces and elites responsible for certain policy decisions, but also furnish the reader with information on the basis of which interesting contrasts with the United States can be drawn in such subject areas as abortion, education, social welfare, internal security, foreign policy, labor-management relations, and antitrust policy.

Policy and Politics in the Federal Republic nicely complements Edinger's overview of German policy making. In addition to von Beyme's opening chapter—a "systematic introduction" to the comparative study of policy making—it includes insightful essays on budgetary, social welfare, health, labor, hous-

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ing, environmental, energy, internal security, and military policy issues. These studies, like Edinger's work, avoid rigid monistic explanations of public policy, and with their emphasis upon policy formation and implementation, they turn away from the radical criticism and ideological passion that have marked policy analysis in Germany. Edinger's work, however, tends to picture German policy-making generally as a process of negotiation and accommodation (often "behind the scenes") among various elites. These studies, on the other hand, detect a variety of policy-making styles. "Broad generalizations about a national style of policy-making," writes von Beyme, "tend to obscure historical changes over time, variations between fields of policy and the differences between the roles of various parties" (p. 18). A major conclusion of several of the chapters is that political parties do matter and that policy-making styles change with the changing complexion of coalition governments. They also find that policy-making styles range from the highly bureaucratic approach to a "participatory policy style," depending on the nature of the particular policy area under study.

One might conclude by suggesting that these two books would serve nicely in any class in German politics oriented to the policy-making process. The American student would find the von Beyme-Schmidt volume the rougher going. The writing style is often convoluted (vintage academic German), typographical errors abound, some passages are inelegantly translated, and, as with any multi-authored volume, the various chapters are mixed in quality. In a few cases the reader is aggravated by political judgments having no basis in the evidence presented. On the whole, however, the essays are worthy of study. Equally valuable is the extensive bibliography on policy and policy analysis attached to each chapter, although most of the citations are to German language materials.

DONALD P. KOMMERS

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Political Culture and Communist Studies.

Edited by Archie Brown. (Armonk, NY: M. E. Sharpe, Inc., 1985. Pp. xii + 211. \$14.95, paper.)

The essays included in this collection discuss the various strengths and weaknesses of using the concept of political culture in studies of comparative communism. The unifying core of the book emerged from a panel organized by Archie Brown on political culture and communist studies for the Second World Congress for Soviet and East European Studies held in West Germany during October 1980. The chapters contributed by Mary McAuley, Gordon Skilling, and Stephen White began as papers presented at this meeting. Yet, the volume clearly is much more than "conference proceedings," inasmuch as it features new additional essays by Archie Brown, John Miller, and David W. Paul.

Archie Brown's introduction highlights the conceptual evolution of political culture in comparative communist studies and the diverse interpretations given to political culture in the field. In turn, Mary McAuley's essay, "Political Culture and Communist Politics: One Step Forward, Two Steps Back," serves as the touchstone for the other contributions, framing the debate over the application of political culture concepts in comparative communist studies. Specifically, she divides the work of "political culturalists" in the field into two camps; "subjectivists," like Stephen White, who study individual-level subjective perceptions of history, politics, attitudes, and values to examine how political knowledge affects behavior in specific historical groups and nations; and, "non-subjectivists," like Robert Tucker, who examine the political system as a complex of ideal and real cultural patterns and political behaviors within existing political structures.

McAuley is more critical of the subjectivist school of interpretation, using examples drawn from several analyses of comparative communism, and particularly Archie Brown's and Jack Grey's 1977 work, *Political Culture and Communist Systems*. McAuley expresses her doubts about how culture is defined, what data are used to compare cultures, why dubious inferences are drawn from past beliefs to present behavior, and how traditional attitudes are seen as steering present-day activity. Since

political culturalists want to argue "that past culture is a crucial factor in forming today's culture" (p. 23), she believes that models of contemporary culture must be based on explanations that firmly define the pattern of traditional values, test whether or not present-day behavior can be explained in terms of contemporary interests, and, if it cannot, then turn to how traditional values, when transmitted, must account for actions today. McAuley concludes that political culturalists continue to define their concepts poorly and often confuse their terminologies, while failing to apply their frameworks with care and dedication.

John Miller's essay continues McAuley's theoretical questioning of the political culture approach. Despite the obvious utility of some of its constructs, he challenges the uncritical acceptance by many scholars of Marxism-Leninism's content and Communist socialization at face value, as well as the tendency to assume a "one-to-one correlation" between what the regime teaches and popular beliefs. Instead, he stresses the importance of looking for the "hidden curriculum" in communist ideology, or the ironic discontinuities in political acculturation and individual socialization that actually contradict official Marxism-Leninism. To reinforce his point, he argues that it is not the content of political beliefs, but their form, defined in terms of cohesiveness and adaptive survival, that political culturalists should examine more closely. By comparing relative success of party recruitment in Estonia, Latvia, Uzbekistan, and Kazakhstan, Miller suggests the relatively low party saturation levels in Estonia and Latvia compared with the relatively high levels in Kazakhstan are largely a function of Stalin's destruction of the Kazakh cultural elite in the 1930s. Whereas Estonian and Latvian political culture was transmitted through a broader base of more independent and literate households, the nomadic Kazakh culture rested on a much smaller base of cultural elites. Consequently, for the Kazakhs, "to destroy their elite was literally to decapitate society" (p. 57). After the collectivization campaigns, then, Kazakh political culture lacked cohesiveness and adaptability, making many of its members more susceptible to appeals from the party.

In addition to these more theoretical discussions by Brown, McAuley, and Miller of the concept of political culture, some contributors

to the volume also try to vindicate the political culture approach as an analytical tool to account for political change and continuity in communist states. Addressing this theme in the USSR, Stephen White's essay compares pre-revolutionary Russia's political culture with other European regimes, both authoritarian-traditional and liberal-democratic. He suggests that much of the Tsarist autocratic tradition has continued into the Communist era. Even so, the Soviet leadership's intention to construct socialism has transformed many political values, social rituals, and cultural attitudes within that autocratic tradition. "Soviet Political Culture Through Soviet Eyes," also looks at the role of political culture in the USSR, but does so by focusing on how Soviet politicians and social scientists use the concept in political discourse. Since Lenin initially used the term "political culture" in the revolutionary era, the notion frequently has circulated in declarations by modern Soviet leaders. It also has directed the inquiry of Soviet social scientists investigating political socialization and education in the USSR. Thus, Brown sees the concept of political culture assuming greater significance in the USSR in both official propaganda and scholarship, which also conveniently provides a common vocabulary and intellectual interest for Western Sovietologists concerned with political culture.

The theme of continuity and discontinuity between pre- and post-revolutionary political culture likewise underpins H. Gordon Skilling's and David W. Paul's essays on contemporary Czechoslovakia. Skilling's essay highlights the importance of political pluralism in Czech political culture under Hapsburg rule, the First Republic, and the Communist regime. While German, Slovak, and Czech nationalism frequently undermined pluralism before 1939, pluralist practices seem to have remained a permanent feature of Czech political life, and to a lesser extent, as Paul notes, of Slovak life, despite the antipluralist fascist and communist regimes of 1939-1945 and 1948 to the present. The limited restoration of pluralism in 1945-1948 and 1968-1969 illustrates how resilient Czechoslovakia's underlying pluralist political culture remained, at least up until the Husak regime. Still, Paul and Skilling conclude that outside forces and Czechoslovakia's international situation have been the basic con-

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straint on Czech pluralism as well as the ultimate source of changes and continuities in Czech political culture.

Archie Brown's conclusion accents the important contributions that anthropological and social psychological research on culture can make to the study of political culture and communist studies. In particular, he suppresses the importance of distinguishing between the "dominant" political culture, or the fundamental political beliefs and values of the majority of the people, and the "official" culture of the party leadership and its norms for behavior within the state and society. Defining and explaining the cross-national and longitudinal variations in these two contending political cultures among communist regimes, Brown suggests, is one of the most promising avenues for moving forward the study of political culture in communist systems. All of the essays in the collection are well worth reading. Yet, McAuley's vigorous critique of political culturalist analysis and Brown's equally spirited concluding defense of this approach are the main attractions, as they expertly elaborate the promises and pitfalls of using political culture concepts in comparative communist studies.

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Party Strategies in Britain: A Study of the 1984 European Elections. By David Butler and Paul Jowett. (New York: St. Martin's Press, 1985. Pp. 172. \$25.00.)

British Democracy at the Crossroads: Voting and Party Competition in the 1980s. By Patrick Dunleavy and Christopher T. Husbands. (Winchester, MA: Allen and Unwin, Inc., 1985. Pp. 320. \$25.00, cloth; \$13.50, paper.)

Britain at the Polls, 1983: A Study of the General Election. Edited by Austin Ranney. (Durham, NC: Duke University Press/American Enterprise Institute, 1985. Pp. 227. \$27.50, cloth; \$13.75, paper.)

"Any national election," write Butler and Jowett, "faces party leaders and party organi-

sations with a challenge." The same might be said for political scientists. Elections are supposed to be central to the operation of a liberal democracy, and political scientists feel compelled to study each one as it occurs. But this latest crop of books on the 1983 British general election and the 1984 election of the British members of the European Parliament raises questions about how political scientists should tackle this task.

There are two principal actors in an election—the electorate and those hoping to be elected. To the study of the behavior and attitudes of the former political scientists can bring one potentially powerful tool: the sample survey. But the electorate plays only a minor role in this collection. Ivor Crewe contributes a useful chapter to the Ranney collection in which he attempts to account for the substantial net shift of support from laborer to the SDP/Liberal Alliance between 1979 and 1983. Crewe's work is based upon a large quota survey of the electorate (4,141 respondents) conducted on polling day and the day before by the Gallup organization for the BBC. Dunleavy and Husbands were able to fund a smaller and rather limited quota survey (1,023 respondents), which was conducted within three weeks of polling day, but only three of their nine chapters are based on its findings.

It is the politicians who consume the most space in this collection. From the pages of Dunleavy and Husbands, and of Ranney, one can acquire a detailed knowledge of their plans, hopes, and activities before and during the 1983 general election, while Butler and Jowett perform a similar function for the 1984 European election. Yet rarely do any of them do much more than describe the events they are concerned with. While we may gain knowledge, we do not necessarily gain understanding.

One's sense of disappointment is greatest with the Dunleavy and Husbands volume. Their introduction suggests that they have two distinctive aims. The first is "to break out of the sterile compartmentalisation of professional political science" by bringing together the study of electoral behavior and political party behavior. The second is their presentation of their own "radical" model of electoral behavior and party competition, whose distinctive position is an emphasis upon the creation of new cleavages in the electorate through

the expansion of the economic role of the state. The aims of the authors are at least clear.

But the execution is disappointing. For example, their two chapters on the events between 1979 and polling day 1983 are little more than informed descriptive accounts of the period. Only a small proportion of the book helps to advance the case for the radical model. Often, as in their discussion of the influence of the media upon electoral choice, their limited cross-sectional survey design does not give them the data they need to pursue their arguments adequately. The discussion of party competition and of electoral behavior is not brought together in a way that reveals original insights. Only in their discussion of the possible implications of future developments in the role of the state in the economy's consumption side does their approach enable them to make a distinctive contribution to our understanding.

In contrast, the other two volumes eschew any explicit or implicit theoretical approach to the study of party strategy. Both concentrate on the presentation of information. In this limited role both are reasonably successful. Butler and Jowett adopt a characteristically clear and readable style, although at times one feels this is achieved at the expense of providing the reader with further (useful?) information. Austin Ranney unfortunately begins his preface with a misstatement of the level of turnout (it was 72.7% rather than 76.3%), while a slightly more active editorship would have reduced the amount of repetition of central campaign events in more than one chapter. Anthony King's summary of the events and style of the first Thatcher administration, however, combines elegance and comprehensiveness, while Michael Pinto-Duschinsky's chapter on the Conservative campaign and Peter Kellner's on Labour's have clearly benefitted from their having been involved in their subject's central campaign.

Yet one is left wondering how these books contribute to the study of elections. Few generalizations are offered or tested that extend our understanding of the British political system or its political parties. While commentary is offered on the wisdom, rationality, or efficiency of a party's actions, the criteria by which these judgements are made are rarely made clear. As a consequence, their value is confined to professional Britain watchers who

already have an understanding to test against the information they are offered. Their value to students must be limited.

The failure of any of these books successfully to apply a theoretical approach to the study of party strategy suggests that political scientists as yet lack sufficiently useful tools to make an original contribution to the analysis of the "high" politics of a modern day election. The challenge in writing books about particular elections is that they are data-driven rather than theory-driven. If they are to contribute to our theoretical and empirical understanding, they need not only to present the new data, but also to test it against our existing understanding. They should question and challenge as well as inform. Political scientists need a strategy as well as the politicians.

JOHN CURTICE

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African Independence: The First Twenty-Five Years. Edited by Gwendolen M. Carter and Patrick O'Meara. (Bloomington: Indiana University Press, 1985. Pp. xiii + 364, \$29.95.)

This collection of essays by 13 prominent scholars of African politics is the product of a series of seminars and a conference organized by the editors in 1983. The stated purpose of these gatherings was to take stock of the first 25 years of African independence. Unfortunately, little attention was paid to the question of what significance the first quarter-century of independent rule is likely to have for the second. As a result, one has a sense of *dejà vu* as one ploughs through this volume. The basic problems of the continent and their causes were identified long ago, and have been discussed extensively in the literature: the marginality of African states and the weakness of attendant political institutions; economic stagnation, especially in agriculture; the dependency of African states on the international capitalist system; the appeal of Afro-socialist and Afro-Marxist ideologies. All have been given deeper treatment elsewhere, in many instances by the contributors to this volume.

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At a time when the political economies of most African states are in an advanced state of disrepair, it is disappointing that this book offers little in the way of a systematic treatment of the policies, processes, and alternative scenarios through which African states might have some prospect of climbing out of their present morass. The problem for this reader is, at base, the failure of social science, as practiced by leading Africanists, to come to grips with Africa's future in light of its past. Are we only to debate the future prescribed by the World Bank in the Berg Report and its sequels? One should hope for more.

African Independence also suffers from a malady of many edited collections—the lack of a common theme, and what appears to be a complete lack of any theoretical framework or set of basic questions around which the contributors were instructed to develop their presentations. Other than the focus supplied by the topic, the volume lacks coherence and structure. Particularly disappointing in this regard is the absence of an introductory essay by one or both of the editors. Given that this volume and the seminars and conference on which it is based were to honor the culmination of Gwendolen Carter's 10 years of contribution to the African Studies Program at Indiana, it is especially unfortunate that this volume is not graced by an exposition of her insights on the continent.

Notwithstanding the above, *African Independence* contains several well-written and interesting essays and one superb contribution that can, and do, stand on their own. In their essay on the marginality of African states, Robert Jackson and Carl Rosberg present a nice discussion of the forgotten distinction between juridical independence and sovereignty as conferred by other states of the international system versus "empirical independence" and sovereignty based on conditions present within the territories over which African states claim jurisdiction. Donald Rothchild's essay on state-ethnic relations contains a useful review and updating of the nature, significance, and future of clientelist politics on the continent. In attempting to set forth a model of hegemonial exchange and bargaining between central and local political forces, Rothchild's contribution is the only one explicitly concerned with the important issue of the politics of state-peasant relations. One

wishes there had been more. A third useful contribution is the excellent review of Africa's agrarian malaise by Michael Lofchie. While largely derivative of earlier work, Lofchie's chapter is a must for any student in need of a one-shot briefing on the subject.

Perhaps the best contribution is John Saul's essay on ideology, particularly the long section on the rise and evolution of Afro-Marxism that includes a mini-case study of Mozambique. For those who have found the recent outpouring of Marxist writings on Africa to be frequently preoccupied with theoretical nit-picking, and silent on the question of how a transition to socialism might be accomplished, Saul's essay is a refreshing departure that should be read by both those who work within the Marxist paradigm and those who do not. Though Saul's presentation is devoid of any data on the significance of Marxist formulations of class analysis (especially class consciousness) on the thinking of Mozambican peasants, his discussion of the evolution of ideology by the top leadership of the Mozambican regime is first-rate.

African Independence is thus a diffuse work with a few bright spots that merit the attention of generalists who desire a review and update on the region, or by those who are particularly interested in the topics of the essays singled out for distinction above. Specialists on Africa, and would-be specialists, however, would do better to reread and reflect upon the major research monographs that made many of the contributors famous.

JOEL D. BARKAN

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Development and Decline: The Evolution of Sociopolitical Organization. Edited by Henri J. M. Claessen, Pieter de Velde, and M. Estellie Smith. (South Hadley, MA: Bergin and Garvey Publishers, Inc., 1985. Pp. xii + 369. \$34.95.)

The first sentence of the preface states that "this volume grew out of a three-day conference on the evolution of sociopolitical organi-

zation, held during the First Intercongress of the International Union of Anthropological and Ethnological Sciences in Amsterdam, April 1981." Indeed. The book consists of 21 chapters, organized in three parts, contributed by 21 authors from nine countries. The bibliography contains about 800 items, from Engels and Harry Eckstein through Marx and Morgan to Orenstein and Organski, but with a heavy preponderance of a variety of anthropologists. The range of substantive material, adduced mainly for illustrative purposes, is enormous. Opening the book at random I find references to information theory, the Aztecs, Guinea Papuans, Nazi Germany, the Aleutian Islands, Samori's state and Kenedugu in West Africa, Clovis and Charlemagne, bi-facial hand-axes in the Middle Pleistocene, Hans Kelsen and the Homeric trial, and, as the Germans say, "und und..."

The preface states that the aim of the volume is:

to explain the evolution of sociopolitical forms. . . . The evolution of sociopolitical forms is a domain within the wider area of social evolution.

Basic to this subfield is the concept of politics. Many researchers have tried to define it. . . . (p. 77)

Here I was tempted to close the book at random, but I was rewarded for not giving in to the temptation.

Part 1 is an "Evaluation of Evolutionism." Christian Guksch, in chapter 2, "The Conceptual Approach or Modeling Evolution," writes:

I would now like to claim that evolutionary culturology is a subset of synergetics (adhering to synthesis as methodological thema) that tries to identify order parameters that cannot meaningfully be reduced, such as ideology, bureaucracy, interests, or even science as a conceptual behavior. (p. 21)

The reward is not yet.

In chapter 5, "Generalized Coercion and Inequality: The Basis of State Power in the Early Civilizations," Bruce G. Trigger informs us, surprisingly, that information theory suggests "the origin of the state can be linked more directly to political than economic factors" (p. 48).

In chapter 7, "Rank Society or Rank Societies: Processes, Stages, and Types of Evolution," Anatolii M. Khazanov conveys the following insights:

The principle of limitation of leading positions is inseparable from the very essence of leadership and its functions. This rule is also operating in those societies in which different spheres of leadership and corresponding positions are separated. Not only in the United States or France can just one of the presidential aspirants be elected. . . . (p. 83)

The curious and unique peculiarity of the human mind is to conceive of the past in terms of the present. (p. 94)

Not yet.

Part 2 deals with "The Evolution of Sociopolitical Organization," and Part 3 is entitled "Toward a Model of Social Evolution." There are some nuggets to be found here for our profession. Ronald Cohen writes in chapter 18, "Warfare and State Formation," in a section on "The Origins of the Military":

Warfare conditioned the rise of states where none previously existed. Fine, but what about its consequences as an aspect of state policy? Political scientists are deeply involved in the causes of war; anthropologists mirror this interest, although they also look at effects—up to a point. (p. 282)

The model of social evolution toward which the co-authors are moving is not clear to me. My difficulties arise both from their practice of writing highly condensed papers for learned journals, evidenced by the bibliography, and from my impatience with efforts to rediscover the (re)invention of the wheel as both artifact and metaphor. No, I don't think Aristotle, who is not cited, said it all. And yes, I have gone through the (re)inventing phase myself (see "Comparative Politics: A Comprehensive Approach," this *Review*, 56(1962): 577-95). However, editors should be able to put the contributions of so many bright and innovative scholars into a clearer, more pointed framework without being Procrustean about it.

I have written this review on the assumption, for which I lack empirical evidence, that anthropologists have a sense of humor that includes, where appropriate, *Galgenhumor*. I came away from reading the book confirmed in my old convictions about the primacy of politics and its study. If there are indeed "wider areas," "domains," and "subfields," and if there is really something called "cultur-

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ology," I know which is the wisest. I trust my readers do, too.

HERBERT J. SPIRO

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Conflict and Violence in Singapore and Malaysia, 1945-1983. By Richard Clutterbuck. (Boulder, CO: Westview Press, 1985. Pp. 398. \$24.00.)

Richard Clutterbuck's (Major General retired, British Army) most recent book updates his 1973 study, *Riot and Revolution In Singapore and Malaya, 1945-1963* (London: Faber and Faber Ltd.). The six volumes he has written in the interim on political violence worldwide have in no way altered his interpretation of the Singapore/Malay(sia) case. That is, parts 1 and 2, "Urban Revolution—Singapore" and "Rural Guerrilla Revolution—Malaya," are identical, down to the pagination, with the original study. Part 3 is new and covers the 1963-1983 period in the same chronological and descriptive style. The book recounts the history of modern Singapore and Malaysia in terms of their successful countering of threats to the emergence of quasi-democratic, pro-Western regimes, namely, labor and Chinese-school unrest in colonial Singapore; the 1948-1960 "emergency" against Malayan Communist Party (MCP) guerrillas; Indonesian confrontation of Malaysia in the early 1960s; and recurrent communal violence culminating in the 1969 Malay-Chinese clashes and the temporary suspension of parliamentary government in Malaysia.

The distinctive flavor of Clutterbuck's account reflects his experiences as a British military officer and Malaysian civil servant fighting Communist insurgency and helping to manage the transition to independence. His detailing of antiguerrilla campaigns, the recruitment of undercover agents, and sketches of key personalities provide information not readily available elsewhere. By the same token, the book has a pro-government slant (for a recent marxist analysis see Hua Wu Yin, *Class and Communalism in Malaysia: Politics in a Dependent Capitalist State*, Lon-

don: Miriam Books, 1983), is of little analytic interest (compare, for example, Lucian W. Pye's *Guerrilla Communism in Malaya*, Princeton: Princeton University Press, 1956), and does not attain the imaginative syntheses of primary sources characterized by the best work in the field (for example, William R. Roff's *The Origins of Malay Nationalism* New Haven: Yale University Press, 1967, or Michael R. Stenson's *Industrial Conflict in Malaya*, London: Oxford University Press, 1970.).

Clutterbuck's basic conclusion is that constituted authority—particularly as embodied in the legal system, the police, and the military—must remain constantly alert to the dangers of overreaction to provocation. The rule of law, freedom of the press, and individual liberties should be maintained to the greatest degree consistent with meeting the challenge of those who do not accept the democratic rules of the game. Within this strategic framework, Clutterbuck focuses on such topics as the gathering of intelligence, the use of the military for development projects, and measures to assure the safety of government officials. These and other counterinsurgency principles derive from conflicts rooted in the decolonization process under the influence of what Clutterbuck calls Leninist (in urban Singapore) and Maoist (in rural Malaysia) models. Their broader relevance is problematic, as suggested by the historically overlapping, somewhat similar Vietnam experience.

In *The Long Long War: Counterinsurgency in Malaya and Vietnam* (New York: Praeger, 1966), Clutterbuck argued that Vietnam could survive as a non-Communist nation if the lessons of Malaya were properly understood and applied. Yet the Malayan Communist Party never managed to make inroads into the conservative Malay community. In Vietnam, no equivalent barrier existed. The critical importance of context is vividly seen in the effort to reproduce Malaya's New Village program in Vietnam's strategic hamlet program.

In Malaya about half a million Chinese, forced by war and economic collapse into squatting on the fringes of the jungle and comprising the Maoist sea in which the guerrillas swam, were resettled into villages where they could be protected and controlled. Clutterbuck adduces a number of cogent tactical considera-

tions that explain why Malaya's success in this endeavor could not be replicated in Vietnam, but surely the basic reason, not subject to human will or judgment, was that the uprooted were not landless immigrants but peasants linked to their land through age-old, sacred bonds.

Part 3 of the book further underscores my claim. Over the last two decades Singapore and Malaysia have become world-class successes in terms of economic development and political stability. Lee Kuan Yew, whose political savvy and toughness was instrumental in the defeat of communism, continues as Singapore's prime minister more than 20 years after independence. In Malaysia, the same communal coalition that gained independence remains at the core of the ruling National Front. The current prime minister, like all of his predecessors, rose through the ranks of the dominant Malay party. Concurrently—and it is difficult not to presume a causal link—the MCP has been reduced to an isolated band on the Thai-Malaysian border, history appears to have bypassed Singapore leftists, and no serious communal clashes have occurred since 1969.

Clutterbuck concludes his book with an assessment of the prospects of Singapore and Malaysia circa the early 1980s. Here, as throughout, his discussion is a balanced presentation worth reading, albeit couched in an "establishment" framework and devoid of analytical innovation, provocative insight, or novel interpretation.

JERRY BASS

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Concorde and Dissent: Explaining High Technology Project Failures in Britain and France. By Elliot J. Feldman. (New York: Cambridge University Press, 1985. Pp. xvii + 201. \$34.50.)

There is no dearth of comparative studies of European public policy making, but *Concorde and Dissent* is an interesting and welcome

addition to the literature. Elliot Feldman has selected two policy initiatives in order to analyze the differences and similarities in French and British experiences with civil aviation. The author has a very specific goal in mind: to dispel stereotyped notions that French planning is superior to British pluralist decision making. He challenges the frequently voiced opinion that the cultural backgrounds of French and British decision-making elites differ. The first case study compares the outcome of the debates about the need for a "third international airport" in London and Paris; the second part is a detailed case study of the Concorde project. These empirical illustrations show that neither British ineptness nor French rational planning account for the construction of Concorde, and that decision makers held similar values and priorities during the debates on a third airport.

Paris, however, obtained its famous Charles de Gaulle facility, whereas the British could not decide upon the location and size of their planned third international airport. Superficially, therefore, the French were more successful in realizing their stated objectives than the British. Yet a closer look reveals a somewhat different picture. Even by the standards of the French planning agency, de Gaulle airport was an ill-conceived, seriously flawed white elephant. It exacted enormous social and economic costs, and the airport "is in the 1980s a monument to technical hubris and uncoordinated central planning" (p. 19). Ironically, the failure of the British to proceed with the building of a third international airport spared "the public most social cost and considerable capital investment" (p. 147). The British simply expanded the already existing infrastructure, which was more than capable of meeting the increase in air traffic.

Cultural similarities among British and French officials and politicians are revealed in the Concorde case study. Concorde was a truly joint Anglo-French project and a spectacular commercial failure. Beset by financial and technological problems, the French and British cooperated until the bitter end to complete the enterprise. As Feldman argues, Concorde cannot be understood by pointing to the history of French planning and British stagnation. In fact, both countries succumbed to very similar pressures. Labor unions strenuously objected to the discontinuation of Concorde,

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on the grounds that it provided much-needed jobs.

Feldman does not deny that British and French policy processes and organizational structures differ. In France, the technocracy has far greater autonomy than in Britain. The different outcomes in the airport debates reflect this divergence. As parliamentary democracies, both countries coped with political instability and frequent rotation of appointed officials. In Britain especially, government has changed color numerous times over the last two decades. However, the frequent changes in political leadership had a different impact upon the policy process in the two countries. Since bureaucracies have far greater independence in France, the involved state agencies could proceed with the plans regardless of the political change. By comparison, the formation of a new British government was accompanied by institutional reorganization. The British difficulty in accomplishing long-term goals derived from "inadequate continuity in political construction and oversight" (p. 156).

This is a well-written and carefully crafted argument. If fault must be found, it is the absence of an analytical or schematic presentation of the differences and similarities in the two systems. In a methodological appendix, Feldman briefly touches upon the structural similarities in different political systems; but he never delineates analytically the ways in which the political systems converge more than diverge, which is, after all, the crux of his thesis. Nevertheless, this informative study is a good starting point for further comparative investigations, and it is just plain fun to read.

PAULETTE KURZER

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The State of the Police. By Phil Scraton. (Dover, NH: Pluto Press, 1985. Pp. viii + 171. \$7.50, paper.)

Policing Industrial Disputes: 1893 to 1985. By Roger Geary. (New York: Cambridge University Press, 1985. Pp. vii + 171. \$34.50.)

The Politics of the Police. By Robert Reiner. (New York: St. Martin's Press, 1985. Pp. xiii + 258. \$29.95.)

In democratic societies the public delegates to the police the legitimate use of force or the threat of force to assure compliance and to accomplish their mandate of law enforcement and maintenance of order. Because of the delicate nature of this task, controls are imposed upon the police in order to assure that they do not transgress the boundaries of their mandate and in turn violate principles that bind a democratic society together. The controls include legal constraints, administrative constraints, and political constraints. The British police have long been supposed to have subscribed to all three constraints. In the course of the last decade, however, that position has come under the careful scrutiny of several scholars and has been found wanting. What prompted this interest has been the role the police have played recently in the quelling of labor unrest and urban disorder. The three books cited above represent some of the most recent critical analysis from this revisionist school.

In *The State of the Police*, Phil Scraton offers a left-of-center sociopolitical explanation for the growth of police autonomy. He develops two theses. One is designed to call into question the myth that police serve as neutral arbiters in social conflicts. This view is frequently expressed in the popular literature on the British police, and is often articulated by senior police officials. Scraton argues that the police have not been neutral in either their order maintenance or law enforcement responsibilities. He contends that they have always represented middle- and upper-class interests, and that they have never gained the support or possessed the consent of the working class. The other thesis, which is the principal focus of the book, presents a persuasive argument that the police have become an organization that is beyond the normal constraints of democratic society. Scraton offered several case studies to

illustrate law enforcement's disregard for the law. In such instances, neither judicial nor organizational constraints served to check such behavior; in fact, it was frequently condoned. In those cases where some blame was forthcoming, it was directed at individual officers rather than at the attitudes and policies ingrained in the organization as a whole.

What proves most distressing to Scraton is the total lack of political constraint on the police. The British have long prided themselves on having local police committees that are supposed to consult with the chief constable. Scraton, points out, however, that there have been several instances in which these committees have either deferred all policy decisions to the chiefs or have been thwarted by the chiefs in their attempts to demand greater accountability. The result of all of this has been a growth in the centralization of the police system that has all the characteristics of a national police force. Scraton does not offer much hope for containing this growth in independence, which has been frequently justified in the name of administrative efficiency. The only course of action presently available to the citizen is the careful monitoring of police activities. This tactic may pressure the police into recognizing the legitimate role that political constraints have within the democratic process.

While information about industrial conflicts and the police response is not the principal source of interest for Scraton, it becomes the focus of Roger Geary's efforts in *Policing Industrial Disputes: 1893 to 1985*. This is a detailed and judicious historical assessment of the changing nature of the relationship between strikers and the police. Geary suggests this relationship has been marked by several distinct phases. Three critical variables—the tactics of the strikers, the policy of the police, and the role of the central government—are essential to understanding this transformation in attitudes and behavior.

Before the turn of the century, strikers often escalated their protest by destroying their employers' property and by violently attacking those who were sent to restore the peace. Throughout the twentieth century the tactics of the strikers changed to obstruction of the employer's operation. This led to a considerable reduction in violence that Geary characterizes as pushing and shoving between

the strikers and police. Not only have the police tactics changed, but the nature of the police force has also been altered considerably. In the nineteenth century the army often assumed a major role in maintaining order at industrial disputes, because the manpower of the local police forces was inadequate. The arrival of the army usually prompted the strikers to respond with violence, which led at times to shots being fired by the military. In the twentieth century industrial disputes became the sole responsibility of the police. In light of the change in the strikers' behavior, the police responded in kind, developing several tactics designed to discourage violent clashes, such as serving as a liaison between the employer and the union, limiting the number of police at picket lines, explaining to the pickets what they could and could not do, and establishing a friendly rapport with the strikers. Finally, the central government significantly changed its role in industrial disputes. In the nineteenth century its role was negligible. Local authorities determined the need for military assistance and directed their mobilization and deployment. In the twentieth century the central government assumed the initiative by centralizing the method of gathering information about strikes and overseeing the police response to them.

Like Scraton, Geary ends his study on a somber note, with a consideration of the level of violence that occurred during the miners' strike of 1984. He attributes this in part to a change in the behavior of the strikers. The miners were not united on the decision to strike, and the militant majority on the miners' executive committee was no longer committed to seeking peaceful reform. Geary is particularly concerned with police tactics and their apparent lack of restraint in dealing with the situation. After the 1981 urban riots the police established a tougher policy and more forceful tactics in handling public order disputes. When the miners struck, the police employed those hardline tactics and violence escalated. They did not differentiate between an industrial dispute and urban disorder. Geary concludes by calling upon the police to make that distinction, and to establish tactics that employ greater restraint.

Because of the nature of the police response to the 1984 miners' strike and of the tactics employed during the urban riots of 1981, the

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police and their policies have been the subject of a good deal of partisan political debate. On the one hand, the right has blindly defended the police response as an expression of their political "law and order" platform. The left, on the other hand, has perceived the tactics as an illustration of a coercive strategy designed to mitigate any attempt to secure democratic control. In *The Politics of The Police*, Robert Reiner offers an insightful analysis of the flaws found in both positions. Following an extensive review of the historical and social science literature, he contends that the police are at a critical juncture in their evolution. What makes this period so significant is the fact that the police and their mission are on the verge of becoming thoroughly politicized in a partisan sense.

Reiner points out that the police are partially to blame for this development. Speeches by some Chief Constables and the Police Federation's "law and order" campaign that was designed to influence the 1979 election are cited as marked departures from law enforcement's noninvolvement in politics. Reiner's most telling arguments are directed at the myths espoused by the political right and left. The right has portrayed the police as the panacea for effective crime control. Reiner employs several British and American studies to prove that most police work does not involve law enforcement. Social service, and to a greater extent, order maintenance are the distinctive characteristics of what police do. The left, for its part, has characterized the police as an oppressive political force. Reiner admits that any relationship that includes a power dimension is by its very nature political, but he contends that the real debate among scholars has centered on the degree to which the police have been involved in partisan politics. The police have been adamant in denying the existence of partisan politics within the forces, and Reiner offers a qualified endorsement of that position. He suggests that the history of the police from 1829 through the 1960s is in part an illustration of law enforcement's efforts to depoliticize its role in society. He also points out that a distinction must be drawn between partisanship of intent and partisanship of impact. In a society that is unequal because of class, law enforcement may well be partisan in its impact, but, he argues, that does not mean it is partisan in its intent.

Reiner contends that both ends of the political spectrum must approach police reform with a proper understanding of police culture and practices. Presently, both the left and the right maintain perspectives that are flawed in historical fact and sociological insight, and this has led both to offer simplistic views on the nature of reform. He supports much of the reform agenda spelled out in Lord Scarman's report, because it is essentially designed to depoliticize the police and their policies.

Those who are interested in comparative police systems will do well to read these books. They should also be required reading for students of British politics. Much has been written about the socioeconomic travails of Britain, and these works offer another insight into the nature of that dilemma from the perspective of the need to maintain order within both society and the police system.

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A Village Without Solidarity: Polish Peasants in Years of Crisis. By C. M. Hann. (New Haven: Yale University Press, 1985. Pp. ix + 208. \$15.95.)

Poland After Solidarity: Social Movements Versus the State. Edited by Bronislaw Misztal. (New Brunswick, NJ: Transaction Books, 1985. Pp. xi + 167. \$19.95.)

Authors writing in recent years on Poland have found it virtually obligatory to include the noun or name "Solidarity" in the title of books or articles. It is assumed—probably by publishers more than authors—that this constitutes a good marketing technique and will encourage greater readership. The books by Hann and Misztal are the most recent illustrations of this phenomenon. What is really ironic, in their cases, is that Solidarity figures very little in their analyses. This may represent both a strength and a weakness.

Around 1979, before the onset of the "Polish August," Hann first took up residence in the remote Polish village of Wislok, located in the desolate Bieszczady hills of southeast Poland.

He had earlier completed an ethnographic work on an "atypical" Hungarian village, and he sought to examine further the impact of the socialist system on rural life in general, and on the evolution of a new community in particular. Wislok had been left destitute by the Polish militia in 1947 during "Operation Vistula"—the campaign to eradicate Ukrainian "brigandry" and produce an increasingly socially homogeneous postwar Poland. The Poles subsequently transplanted to the area were faced with basic community-building tasks. Further, as Hann incisively argues, colonists from the Central European plain were singularly ill-suited to conduct agriculture in a mountainous area. The study provides an interesting account of the economic progress made by the postwar Polish settlers, and their often tense relations with the small group of Ukrainians and *Rusnaks* (Ruthenians) who had managed to survive in these lands.

A central thesis of the author is that the system of private agriculture has brought ruin to Poland. The peasantry, whose income is derived exclusively from the land, is inevitably being replaced by commercial farmers or by worker-peasants. But in insisting, 40 years ago, on the maintenance of individual farms, peasants essentially traded off indispensable state assistance (machinery, fertilizer, investment) for largely irrelevant legal deeds. It is to Hann's credit that he does not belabor this argument, for in the course of his analysis little evidence is marshalled to support the view that collectivization is a preferable form of agriculture system. If state intervention in agriculture is, on balance, helpful to the intermediate-sized family farm and to the specialist farmer, the graphic presentation of forms of "doing business" in the public and cooperative sectors only sheds further light on the emergence of "Individual Farmers' Solidarity" and the determination of all farmers to evade state takeover of land (now improbable following the constitutional enshrinement of the principle of private agriculture.)

For the student of Poland who is shell shocked by the spate and unevenness of recent literature on the country, *Village Without Solidarity* is a soothing experience. In an unpretentious yet forceful way we learn of the family system, community bonds, patterns of religious worship, and subjects of gossip in Wislok—the "atypical" Polish village. The

book is full of data on the more obscure and the more trivial aspects of Polish society—for example, the survival of a Greek Catholic (Uniate) church in the area (the religion was officially banned in 1946), or the fact that the number of children living in the countryside remains 700,000 more than the number in urban centers (n. 1, p. 199). Such crucial information is regularly overlooked by sociologists and political scientists. If Solidarity is only of marginal concern to the author, and if his conclusion seems aimed more at the handful of Polish social scientists working in this field than at us, these tendencies only add to the charm and authenticity of the author's monograph.

It is embarrassing for political scientists when an anthropologist reveals greater theoretical and empirical aptitude in examining a highly politicized society, such as Poland has been in the 1980s. Misztal and his contributors are not all political scientists to be sure. They are, by and large, sociologists concerned with political phenomena; put another way, there are steeped in the specifically socialist East European tradition of plucking just the sociological feathers from the polity. Oftentimes this proves to be an irritating approach, for it absolves political leaders and political institutions of their obvious faults. The reason we learn nothing about Solidarity from this book is its abstruse and overindulgent theorization. Thus we find two different periodizations of the Solidarity movement in the very first two chapters. Contributors pay lip service to the concepts of *state* and *civil society* (presumably the volume's adhesive substance), but they are never defined (other than in a largely irrelevant way by Markus in chapter 4), and *rulers* and *ruled* would have proved just as adequate terms anyway. From the acknowledgements the reader ought to have been forewarned something was amiss in this study: four of the six speakers (the most senior) invited to the conference on this subject did not contribute to the edited volume. A couple of the substitutes (Lewis and Szajkowski) rehashed impressive research published previously. While the contributors consist of an otherwise distinguished group of East European specialists, the chemistry was lacking here. Misztal's concluding chapter is a critique of (mostly) American literature on—just for starters—the state, social movements, urban sociology, and political

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economy. He is very unkind to it all, no more so than when evaluating the innovative ideas of urban theorists. The proofreading is catastrophic, ending up, fittingly, with an author named "Croom Helm" listed in references to Misztal's final chapter.

Political scientists and political sociologists ought, perhaps, to take a respite from theorizing about Solidarity, and to resume empirical research, which they appear to have suspended with material law in 1982. Let the Polish opposition movement work on model building; whatever one's attitudes to them, Kuron and Michnik are brilliant theoreticians with few equals among Western scholars. Finally, one longs to learn more about Solidarity, but perhaps from memoirs written by those who were involved. In this respect—unfortunately for Solidarity sympathisers—the government's former "contact man" with the union, Rakowski, has been most prolific and revealing.

RAYMOND TARAS

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Small States in World Markets: Industrial Policy in Europe. By Peter Katzenstein. (Ithaca, NY and London: Cornell University Press, 1985. Pp. 268. \$29.95, cloth; \$10.95, paper.)

The volume under review is one part of a twin; both volumes are concerned with the political economy of "small states." The present study deals with the industrial policy of the small European states as a group, while the companion volume, *Corporatism and Change: Austria, Switzerland, and the Politics of Industry*, examines two closely related, yet contrasting national economies, Austria and Switzerland (Ithaca, NY and London: Cornell University Press, 1984). Yet the two can be read and understood independently of each other, and both deserve the serious attention of a variety of scholars, for they deal with issues crucial to modern industrial states.

Katzenstein's study of small state industrial policy is exemplary in several ways. First,

Katzenstein engages in comparative analysis at the national aggregate level. In doing so, he uses a sizeable body of number without ever descending into any of the inanities of that methodology. The author has a thorough grasp of the national and social contexts from which these data arise, and of what they mean in their respective contexts.

Second, the book provides testimony to the effective use of history by a social scientist: Katzenstein does more than just embroider an otherwise bare cross-sectional contemporary study. History is used here in broad strokes, yet carefully, to document both the commonalities that characterize the political economies of the small states of Europe and what distinguishes them from each other. The history chapter does not appear at the outset, where it is usually placed, only to be put aside in any subsequent analysis. History comes *after* used to help explicate the origins of the various small state features already examined fully in their contemporary setting. That, it seems to me, is the proper use of history by a social scientist.

Third, students of corporatism can be grateful to Katzenstein for aiding them in their defense against the strong tendency in the discipline to dismiss their concerns because (a) "there ain't no such thing" in the U.S.; (b) the concept is nothing more than typical European myth making; and (c) we know that all corporatism ever was, or can be, is a "fig leaf for absolutism," as the late Herman Finer pointed out correctly in the case of Italy. Katzenstein demonstrates that corporatism is alive and well and living in a number of European places, that it refers to concrete social arrangements, and that it is fully compatible with well-functioning democratic institutions.

The structure of the argument, and of the book as a whole, is simple, yet persuasive and exceedingly well developed. What distinguishes the small states of Europe from the large ones, according to Katzenstein, is the manner in which they combine a liberal external economic policy that keeps the country open to the forces of the international economy with a policy of domestic compensation through which the country's economy is buffered, to some extent, from the consequences of liberal openness. However, this small-state industrial policy does not become a straight jacket, as is the case in some large states; nor

do the small states neglect industrial policy altogether, as do some other large states. At the same time, it is demonstrated that not all small states have settled on the same mix of international liberalization and domestic compensation in pursuing their industrial policy. Some pursue a much more active industrial policy than others. It is also worth noting that the particular small-state industrial policy Katzenstein specifies is not presented as resting entirely on the logic of size—that is, size is not seen as exclusively determining industrial policy. The historical foundations for the various types of small-state industrial policies enter the explanation in a significant way.

After laying out, in chapter 2, the three principal elements of the analysis—international liberalization, domestic compensation, and flexible response—and after demonstrating interstate differences using Austria and Switzerland as case studies, Katzenstein devotes chapter 3 to a presentation and analysis of corporatism, the particular arrangement that makes possible the domestic compensation for the international liberalization policies pursued by small states. Eschewing rigid ideal-typical constructs, such as “societal corporatism” and “state corporatism,” he begins by observing that what he comes to characterize as corporatism is essentially a democratic corporatism. No extended arguments are necessary to so characterize the politics and societies of the Scandinavian countries, Benelux, Austria, and Switzerland. The small-state democratic corporatism that makes possible the mechanism of domestic compensation has three defining characteristics: an ideology of social partnership, a centralized and concentrated system of economic interest groups, and an uninterrupted process of bargaining among all the major political actors. Corporatism is also a result of the distinctive party systems of the small European states, in which the parties of the right are divided and the systems of proportional representation favors coalition government.

The European democratic corporatism is not monolithic, however. Katzenstein has been able to specify two major types: “liberal” corporatism, characterized by strong, centralized business organizations and weak, decentralized labor unions, as found in Switzerland, the Netherlands, and Belgium; and “social” corporatism, characterized by strong labor and weak

business groups, as found in Austria, Norway, and Denmark. Sweden is said to constitute a mixed type. Chapter 3 then proceeds to examine in detail the workings of these various manifestations of democratic corporatism, and the analysis succeeds in demonstrating the success of the author in specifying a range of similarities and differences between the small states and the large ones (France, Great Britain, Federal Republic of Germany), and among the small states themselves. The principal lesson the author seems to draw from his analysis is the superior flexibility exhibited by the small states, as compared with the large states, with regard to their economic and industrial policies, both domestic and international.

Finally, in chapter 4 the author proceeds to look to the historical dimension to explain democratic corporatism—except in Austria, whose development in the 1920s and 30s (a civil war and a native fascist regime) deviated from that of the other small states. For all the other small states, the roots of post-World War II corporatism must be sought in the sociopolitical and economic developments of the interwar years. Following the initial presentation of the workings of contemporary corporatism, the historical analysis becomes a constituent element of the total explanation.

A final warning: this is not a book for casual reading. The argument is taut and concentrated; it is constructed with care and every part of it has its essential place. In short, it is what a scholarly work should be; not too many come up to this standard.

ALFRED DIAMANT

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Regime Change and the Coherence of European Governments. By Mark Irving Lichbach. (Denver, CO: University of Denver Graduate School of International Studies, 1984. Pp. xvii + 158. \$24.00.)

Since 1963 the University of Denver's Graduate School of International Studies has published a stimulating and eclectic monograph series on world affairs. Although not without its flaws, Mark Lichbach's book represents a solid addition to this series.

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The goal of the author is to analyze changes in the authority structures of nineteenth- and twentieth-century European states, in order to determine "the extent to which European political history was dominated by three trends—democratization, bureaucratization and coherence" (p. 5). The general thesis developed by Lichbach is that regime changes have not followed a unilinear pattern of change toward greater democratization and bureaucratization (a view he ascribes to scholars he terms "developmentalists"). Rather, he sees the underlying pattern of change as one of coherence, in which changes occur in such a way that the various authority dimensions of a polity are consistent with each other.

The method of presentation is straightforward. Following initial chapters that discuss various views of regime change and the method of analysis to be pursued, separate chapters discuss changes in the democratic and bureaucratic character of European polities. Lichbach then moves on to an analysis of the coherence of these political transformations, which is followed by a brief concluding chapter. The raw data for the analysis are taken from the POLITY dataset collected by Ted Gurr, permitting the author to estimate the character of the authority structures of 49 current and historical (e.g., Bavaria) European and "European-settled" states for each year from 1800 to 1970.

The contribution of this volume clearly lies in its careful and comprehensive assembly of these cross-sectional time series data. The theoretical framework of the book, including the general procedures involved in the operationalization of the key concepts, is almost entirely taken directly from the works of Eckstein and Gurr (e.g., Harry Eckstein and Ted Robert Gurr, *Patterns of Authority: A Structural Basis for Political Authority*, New York: Wiley, 1975). The derivative nature of the conceptual framework may make some readers question whether the book offers a fair test of competing theoretical expectations (developmental versus coherence) concerning regime change. For example, while Lichbach rather unquestioningly uses Gurr's group competition measure to track changes in participation, plausible alternatives (like the extent of the franchise) that might have supported an alternative view are ignored.

There are other problems in the analysis that may lead readers to question the overall conclusions. The principal one is the simultaneous analysis of political systems that may not be comparable with respect to the hypotheses being examined. The core of the regime transformation analysis is the placing of systems into five categories (entrenchment in one of two polar states, unidirectional shifts to either polar state, and oscillation between states) for each of the several variables examined. Yet it is difficult to see how the experiences of Sardinia (1815-1860), Ireland (1922-1970), and Austria (1800-1970) are comparable and equivalent cases of democratic and bureaucratic regime changes. This problem is compounded by the author's decision to classify systems on the basis of any changes, no matter how brief, in the entire span of time and only during this time period. Thus a state that is democratic throughout the period except for a two-year intermediate period of autocracy is classified as oscillating, while a nation that is democratic throughout the period is viewed as "entrenched." In neither case will the states' experience be viewed as consistent with a long-term developmental trend toward democratization.

These sorts of problems will make many readers question the extent to which Lichbach has made his case concerning the nature of regime change in modern European systems. However, the basic presentation of the data, particularly the tables showing aggregate patterns of authority structures at ten-year intervals throughout the 1800-1970 period, should prove of great interest to scholars of both European and general patterns of political development. Although not all readers will agree with the author's findings and conclusions, they will find this book to be thought-provoking and engaging.

JOHN P. FRENDREIS

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Ideology, Modernization and Politics in India. By V. R. Mehta. (New Delhi: Manohar, 1983. Pp. xii + 232. \$18.50.)

This book and Mehta's earlier work, *Beyond Marxism: Towards An Alternative Perspec-*

tive, follow in the tradition of modern Indian thought, particularly the works of M. N. Roy after 1947, Vinoba Bhave, Jayaprakash Narayan, and J. B. Kripalani. These theorists sought to develop concepts inherent in the thinking of Vivekananda, Aurobindo, Tagore, and Mahatma Gandhi. Central to this tradition is the attempt to restructure society so that morally autonomous individuals may find self-fulfilment in a politically and economically decentralized society. This "integralistic-pluralistic" view of society, stemming from the Indian Renaissance, represents a synthesis of East and West.

Mehta argues that both the liberal-democratic political system, superimposed upon Indian society at Independence, and Marxism are seriously flawed, particularly in their suitability to contemporary Indian society. The decision by the framers of the Indian constitution to adopt a parliamentary system, rather than Mahatma Gandhi's vision of a decentralized polity, has resulted in "cleavages between politics, economy and society" (p. x).

In Mehta's view, the incompatibility of the liberal-democratic model is grounded in the structure of Indian society, which includes various autonomous "wholes": the individual, family, village, province, nation, caste, and class. For Aurobindo the ideal relationship between the individual and the community was "complex communal freedom," described by Mahatma Gandhi as "oceanic circles." Each individual "must try to develop and realize the soul through seeking greater intercourse with other members" (p. 24). Every whole has its functions and duties, and the social system would ensure a "cosmic symphony" of wholes and their fullest development. Individuation is possible even while "the individual . . . exists within a complex of other wholes" (p. 24). What is required today is a new "functional balance" among these wholes.

The existing system, which encourages group loyalties, has led to politicization along lines of caste, linguistic, and religious community, increasing the fragmentation of society. The legitimacy of these groups is grounded in tradition, while the universalistic and humanistic values inherent in the Indian Renaissance were largely forgotten after 1947. For Mehta, the creation of Pakistan was a "surrender to communalism" that encouraged "regionalism," "linguism," and sub-nationalism.

Mehta's discussion of the liberal-democratic and Marxist-Leninist models updates the critiques of Roy and Narayan. It is argued that in India, the liberal-democratic system, reversed the classical process of both economic and political development. Classical economic development demanded "a flow of capital from agriculture to industry," while in India this has been reversed (p. 44). Rural conservative elites ("middle castes") have attained dominance over the middle class, and the government has failed to mobilize agricultural revenues adequately. This has retarded the social welfare of the underclasses. Similarly, classical political development envisioned the secularization of society, whereas in India, linguistic, ethnic, and regional groups have been strengthened. Marxists also failed to understand the importance of castes and religion, and consequently could not generate a relevant analysis of Indian society.

Mehta argues that Indian reformers, including Mahatma Gandhi, "could not bridge the gap between politics and society." This would have required a "concrete strategy of institution building . . . by which a strong political centre corresponding to the communal character of the nationalist movement could emerge" (p. 128). For Mehta, the creation of a strong political center along with "a powerful political ideology" (p. 188) is not contradictory to the Gandhian model. Rather, these are necessary conditions for it. He distinguishes between "survival functions," such as defense and communications at the center, and "developmental functions," which would be decentralized to promote economic development. He urges a new debate on the Gandhian model to seek appropriate institutions for modernization.

This is the most ambitious attempt to come to grips with the enormous gap between theory and practice in Indian society since the works of Roy and Narayan. Although Indian polity was influenced by Gandhian thought, particularly in the *Panchayati Raj* system and in the integration of small-scale industries into the planning models, the political and economic structure of society has become increasingly centralized. Mehta's criticism of the Western political model in the Indian context is extremely relevant, but tends to place excessive emphasis upon structural change. Chauvinistic and parochial attachments to the "wholes" of Indian society must also be

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blamed for contemporary social turbulence. Both Roy and Narayan, as well as Mahatma Gandhi, placed emphasis upon changes in both structures and values. Also, while Gandhian principles should serve to guide public policy in India, major changes in the political structure to adopt Gandhian institutions cannot easily be made today, as the Janata Government learned in the late 1970s. Tensions will continue as the predominant values of India's large middle class continue to clash with parochial sentiments, and as India undergoes rapid and uneven development. Nevertheless, Mehta skillfully draws upon a broad range of literature, making this work a landmark contribution to our understanding of modern Indian society.

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Human Needs and Political Development: A Dissent to Utopian Solutions. By Han S. Park. (Cambridge: Schenkman Publishing Company, Inc., 1984. Pp. viii + 270. \$19.95, cloth; \$11.95, paper.)

In his *Human Needs and Political Development: A Dissent to Utopian Solutions*, Han S. Park explains political development in terms of needs he claims are universal. Rejecting the idea that various cultures have values distinctive to themselves, he identifies basic needs which are species-wide. As similar needs produce similar behavior, it follows that all people will react in a similar manner to given political circumstances. Thus Professor Park's paradigm on political change applies to all and is neither ethnocentric nor partial to a particular ideology.

At the outset, Professor Park evaluates several prominent theories that compare political systems, and suggests criteria by which to define political development (chapter 1-2). He next outlines his paradigm (chapter 3) and discusses each of its four stages in detail (chapters 4-7). He concludes by mentioning the consequences of his theory for the earth's resources, and by proposing how to mitigate

his paradigm's negative implications about human nature (chapter 8 and postscript).

Three assumptions underlie the paradigm. The author concedes that they are debatable and must continually be reassessed. First, one consistently strives to satisfy one's needs to the greatest possible extent. Second, the prevailing needs of individuals determine the course of political development; this supposition rejects formulations such as Rousseau's "general will" that derive an interest from society as a whole. Lastly, government and politics are legitimate insofar as they can satisfy a people's changing needs (pp. 59-60).

Four drives constitutes the hierarchy of needs pertaining to the first assumption: one desires foremost to survive, next to interact and to share companionship with one's fellows, then to enjoy leisure, and finally to attain superiority over others by acquiring superfluous material goods and social status (pp. 61-63). Four stages of political development correspond to these needs: regime formation, political integration, resource expansion, and conflict management (pp. 78-85). Main attributes of each stage include means of meeting prevalent needs, predominant institutions, personality traits, governing style, type of political leader, policy priorities, and the resulting political situation (pp. 86-87). These characteristics are a plausible reflection of how a government may respond to the progression of human needs. As indicated in the work's title, the paradigm dissents from utopian conclusions. Whereas many development theories portray development as inevitable, beneficial, and culminating in an ideal state, Professor Park's model specifies the conditions leading to a regression in development and argues that the most advanced development stage is also the most flawed (p. 88).

Professor Park comments that his bleak predictions are founded on an empirical rather than a normative definition of development (p. 261), and therefore describe political development as it is instead of as it should be. One may question, however, whether his theory is accurate even in empirical terms. His paradigm would not be definitive if the needs on which it is based are not universal. Of the four needs, the urge to survive is perhaps the only desire which is indisputably inherent throughout the species. The other three needs—to belong, to enjoy leisure, and to exert con-

trol—may not be essential qualities of being human, but artificial needs obtained through adverse influences; as such, it is likely that not all people possess them. The need to control other is especially questionably on these grounds. As Professor Park observes, this desire is an attribute of Hobbesian man, and he regards it as "morally undesirable but empirically undeniable" (p. 63). Subsequent political theorists, though, have argued persuasively against this interpretation of human nature; Rousseau is notable among these.

LINDA LUM

U.S. Department of State

The Political Economy of Zimbabwe. Edited by Michael G. Schatzberg. (New York: Praeger, 1984. Pp. x + 276. \$29.95.)

The essays in this volume all reflect upon the formidable choices facing the Zimbabwean state. The focus on state choice in a volume on political economy is particularly appropriate in the case of Zimbabwe because of both the expanded post-independence demands placed upon the state, and the weighty character of an already extremely interventionist colonial apparatus. In fact Lee Cokorinos, in a sweeping essay on party and state formation, asserts that the Unilateral Declaration of Independence in 1965 and subsequent economic sanctions against Rhodesia marked the "creation of one of the most directed capitalist economies in the world" (p. 29).

Perhaps the most central choice facing a state is that between two basic development strategies: technocratic and populist (sometimes termed *capitalist* and *socialist*). David Gordon's lucid essay on development strategy suggests that the regime's use of ideas from both camps has judiciously balanced requirements for growth and equity. Of course, choices are not made in a vacuum, and in Zimbabwe there are constituencies loosely grouped around the two competing strategies. Writing on factions behind the strategies, Ronald Libby asserts that none of the major spokespersons of the technocratic position were participants in

the seven-year guerilla war, and most are professionally trained intellectuals now working in state ministries. Their allies are businessmen and larger farmers. They did not inherit the radical Marxist-tinged ideology that grew up in rural branches during the war, and many of them are not prominent in the highest councils of the Zimbabwean African National Union (ZANU). According to Gordon, the technocrats see growth premised upon an open capitalist economy, with exports maximized and foreign private investment encouraged. Productive commercial farmers, most of them white, will remain, while there will be some land reform with encouragement of peasant production. There will also be an enlarged state apparatus in order to manage expanded social service and agricultural sectors, as well as to renegotiate and regulate relations with multinational corporations.

The populists are rooted in ZANU. Their development strategy begins with large-scale land reform, which was the primary demand of African nationalism. Government expenditure will be targeted at the peasantry, in order to reverse prior bias toward large farmers, and there will be a high minimum wage for urban workers. A self-reliant and nationally integrated economy will be built with minimal ties to South Africa. All foreign private investments will be critically assessed, as in the technocratic strategy, the state will greatly expand in order to provide basic social services and to direct the development effort.

Both factions expect a lot from the state. While it is too soon after Independence for these essays to make an assessment, the most important test of the state will be whether it rises to the occasion or begins to decay in the face of declining economies and gathering political tensions. Eukudzo Murapa provides a careful account of racial politics behind the civil service hiring practices of colonial Rhodesia and independent Zimbabwe. Yet, while he is properly concerned about the ideology of the bureaucracy in an officially declared transition to socialism, he is uncritical of the state's enormous expansion after Independence. The obvious danger is that the state will become parasitic and exploitative, regardless of ideology.

There are factions roughly correlated with competing development strategies, but we should not assume that the correlation is im-

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mutable—that an official triumph of strategy is necessarily a political triumph of faction. The fortunes of all factions will most broadly depend upon the presence or absence of democratic rights won in the liberation struggle. Both Libby and Cokorinos hint that the mass base of ZANU, the ruling party, is losing ground to the state apparatus. Ronald Weitzer, in a convincing essay on colonial and independent state security, finds a steady expansion of executive powers. He also finds considerable continuity between colonial and independent state repression. Searching for explanations, Weitzer uses what he terms a *state-centric* analysis, which assumes that the state, though far from homogeneous, has distinctive interests of its own that provide sufficient incentives for repressive measures. Weitzer does not address the question of what enhanced state power and repression means for the fortunes of the two broad factions defining policy choice, but his account does imply that factional leaders on all sides are willing to use the state's repressive apparatus when it suits their interests. Because of the overwhelming dominance of state over society, one or the other development strategy may be adopted by the executive core without much of a popular base. The result could be "reform" or "revolution" from above, with the majority progressively distanced from the state.

The outcome of these state-society struggles will be highly conditioned by growing tensions with South Africa. The essay by Carol Thompson on Zimbabwe's role in the Southern African Development Coordination Conference details Zimbabwe's extensive economic dependence on South Africa. As liberation forces gather strength within South Africa and as international sanctions are applied to the apartheid state, Zimbabwe must again choose. Prime Minister Mugabe supports sanctions knowing full well that South Africa is likely to retaliate by applying sanctions of its own against Zimbabwe. In the face of a hostile and increasingly desperate neighbor like South Africa, the Mugabe regime has recently made gestures of peace to rival Joshua Nkomo and ZAPU. This effort to bind up severe internal political wounds is made in order to meet the growing external threat. Should a modicum of internal reconciliation emerge, it will have great significance not only for the range of policy choice mentioned above, but also for

the future of democratic rights and state-society relationships in general.

Most of the essays were presented to an April 1983 conference at the Johns Hopkins University's School of Advanced International Studies. A brief introduction by editor Michael Schatzberg, an essay on housing the urban poor by Diana Patel, a study of private capital's assessment of opportunity in Zimbabwe (foreign capital sees more risk than local capital) by Pauline Baker, and a lengthy bibliography by Jonathan Evans round out this extremely useful resource and guide to understanding Zimbabwe's crucial choices in the early years of independence.

FRANK HOLMQUIST

Hampshire College

Ordinary Stalinism: Democratic Centralism and the Question of Communist Development. By Ronald Tiersky. (Winchester, MA: Allen and Unwin, Inc., 1985, Pp. xiii + 209. \$19.50)

Ronald Tiersky is impressed with the deadlock of Communism: the consistent failure of attempts at reform of "ordinary Stalinism," the orthodox praxis and legitimating doctrines of Communist regimes and parties. Unlike many who have observed this deadening and debilitating stasis, however, Tiersky does not regard it as inevitable. "Such conclusions are either unjustifiably exaggerated or . . . simply wrong" (p. 7). Instead, he finds ". . . in democratic centralism—evidence of *autonomous political developmental potentialities* on the basis of communism itself" (xi).

Tiersky seeks to build this argument through a combination of analysis of general trends in Communist internal politics and case studies of postwar developments in the Italian and French Communist parties, especially in the last decade. The first half of the book traces the evolution of ordinary Stalinism. Much of the analysis is conventional, but Tiersky argues that the system of Communist internal governance was the result of choices that sacrificed the dialectic premises of Lenin's original formulation and the possibility of in-

ternal diversity to the primacy of unity and the monopoly power of party, state, or leader.

The result was the emergence of the "holy trinity" of ordinary Stalinism's legitimating doctrines—democratic centralism, dictatorship of the proletariat, and proletarian internationalism—each corresponding to an appropriate level of Communist politics—party, state, or international movement. These buttressed practices at each level that eliminated all possibility of meaningful diversity, much less debate. Today, however, the latter two of these doctrines are in an advanced state of decay and democratic centralism is itself under sharp attack, at least among the nonruling parties.

It is here that Tiersky brings his case studies into play. He seeks to show that "communist political development in West European countries can be read as an internal struggle waged essentially over the reform of democratic centralism, in its large rather than narrow meaning" (p. 95). By this he intends that the core theme of ordinary Stalinism—monolithism, the attempt to transform all politics into internal politics and thus totally to dominate flanking organizations, allies, and the society as a whole—is at the heart of the conflicts and changes that have occurred in the two major nonruling parties. The parties are, it is argued, faced with a profound dilemma: developments in the Communist states and the international movement have meant that the only Communist source of legitimacy lies in the claim to have a superior form of party organization; yet it is precisely this form that cripples the parties' ability to succeed in the societies within which they compete. The choice, therefore, is between orthodoxy and a future in which the parties can be expected to "wither and decline" (p. 96), or "reforming the prohibition on faction", thereby creating the possibility of successful adaptation (p. 172).

The French and Italian parties are well chosen to demonstrate this choice and the consequences of alternative resolutions. The French party, inconsistent and probably insincere in its willingness to reform its internal practice, has condemned itself to a loss of electoral and political influence. The Italians, by contrast, have gradually but consistently pursued reform, expanding the possibilities for internal diversity even while successfully maintaining the formal prohibition on organized

factions. They too, however, may have reached the limits of the attempt to change the substance of internal politics without permitting factions. Tiersky's more intimate knowledge of the French part is apparent, but he has effectively used the extensive research on the Italians to make his case.

Nonetheless, upon completing this study one remains more convinced by the description than by the analysis. Tiersky's general assertion—that reform of Communist politics can come from within—is not demonstrated. The sole case of real reform, the Italian, is possibly anomalous. Tiersky himself speaks of "the force of circumstance" and the "quality of leadership" as possible explanations (p. 96). Furthermore, the Italian party is, among Communist parties, almost unique in the extent to which it has made its organization open to permeation by the larger society. To speak of reform from within is, therefore, misplaced. Tiersky's book provides a good analytical description of Communist orthodoxy and its sources of doctrinal crisis. He offers much less for those seeking to explain change in Communist parties, and thus fails to convince us that "communist development is inevitable and the real issue is not whether, but when" (p. 176).

PETER LANGE

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Nicaragua: The First Five Years. Edited by Thomas W. Walker. (New York: Praeger, 1985. Pp. 561. \$17.95, paper.)

Thomas Walker began studying Nicaragua in 1970. Since 1979 Walker has graciously served as unappointed coordinator for the swarm of social scientists bent on studying the Sandinista revolution. In addition to assisting individual scholars, he has organized numerous forums for the dissemination of their research. *Nicaragua: The First Five Years* is such an effort. The mammoth text includes contributions by 34 scholars, organized into 25 chapters, and buttressed by an introduction written by Walker. An appendix presents a summary of the report of the Latin American

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Studies Association's delegation to observe the Nicaraguan elections held in late 1984.

Chapters address a plethora of topics, ranging from the armed forces, to women, to housing policy, to relations with the nonaligned movement. Despite 25 chapters there are some glaring gaps, particularly concerning the church and the counterrevolution. More problematic: Even if the book is read from page 1 through page 561, an organic view of the Nicaragua: Revolution does not emerge. The chapters are individual slices into a whole that remains elusive. The book, contains a wealth of data on a wide variety of subjects, however, which should be useful for those attempting comparative and theoretical analyses of contemporary revolutions, as well as for those who simply want to know more about Nicaragua.

Given the number and variety of scholars

presenting research in the volume, there is also an opportunity to observe in comparative fashion the way "area specialists" conduct their inquiries and the resulting success they enjoy. There are some interesting studies, such as Dennis Gilbert's fascinating portrayal of the contradictory role played by the "bourgeoisie" in the Nicaraguan Revolution. Comparing his and similar successes in the volume with their less successful counterparts suggest a number of maxims for those studying revolution in a hot country: Be sensitive to time, avoid being state-centric, use multiple data sources, have a healthy sense of skepticism, opt for specificity, avoid sweeping generalizations, and pose questions that admittedly cannot be answered but that nonetheless deserve to be asked.

FORREST D. COLBURN

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INTERNATIONAL RELATIONS

Hawks, Doves, & Owls: An Agenda for Avoiding Nuclear War. Edited by Graham T. Allison, Albert Carnesale, and Joseph S. Nye, Jr. (New York: Norton, 1985. Pp. xii + 282. \$14.95.)

This valuable, and in various ways provocative, book is subtitled "An Agenda for Avoiding Nuclear War," but as the editors confess, "many important causes and dimensions of war are deliberately ignored here" (p. 17). The book looks for possible steps to reduce risks of nuclear war. It consists largely of chapters focused on the possibilities of nuclear war by accident (written by Paul Bracken), by surprise attack or preemption (Richard Betts), by escalation in Europe (Fen Osler Hampson) or in the Middle East (Francis Fukuyama), and by catalytic war (Henry Rowen).

The editors frame these analyses with an introduction focusing our attention on three levels of causation (precipitating, contribu-

tory, and deep) and a conclusion admittedly caricaturing three positions: the familiar hawks who fear weakness and doves who fear arms races, and the editors' own self-characterization, "owls," who fear "loss of control and nonrational factors in history" (p. 210). The editors are owls because of their assessment that the greatest risk of nuclear war at present lies in inadvertence. Individual chapters by "specialists" (they assert that there are no "experts," because we have no nuclear wars to serve as materials for expert study or experience) speculate comprehensively on possible, if not plausible, routes to superpower nuclear war and possible measures to lessen their likelihoods.

Arguing that "the primary objective of the United States in either building up or building down nuclear arms should be to protect and defend U.S. values and institutions by *avoiding nuclear war* with the Soviet Union" (p. ix), the editors compile the conclusions of the spe-

cialists into an agenda of 10 general principles incorporating 27 "dos" and 24 "don'ts." The principles are largely unexceptionable: maintain a credible nuclear deterrent, enhance crisis stability, invigorate nonproliferation efforts, etc. Some of the specifics are more interesting: "DON'T adopt a non-first-use policy;" "DON'T pursue a comprehensive freeze;" "DO intensify the search for alternatives to deterrence."

The book repays reading by those interested in the debate over U.S. military policy, but like many products of committee deliberation, it tends toward "common sense" or a "lowest common denominator of views" rather than toward innovative approaches. The book is a product of the Kennedy School's "Avoiding Nuclear War Project" funded by the Carnegie Corporation. As such, the authors were asked by the editors, all of the Kennedy School, "to assume that some deep causes and factors—such as U.S.-Soviet rivalry, superpower interest in Europe, and Third World instability—will continue unchanged" (pp.19-20). The editors say that considering possible changes in these underlying factors will require another book, but at the end of the study they venture that "the search for less conventional, more imaginative alternatives [to nuclear deterrence] must begin. Bold, creative approaches to the subject must be stimulated, nurtured, and rewarded" (p. 246).

Such approaches are ruled out of this book by its short-term and largely technical focus. Also largely absent—as it is in most writing on strategic and foreign policy—is careful attention to underlying assumptions about how policy advice should be developed and what claims to authority it can make and justify.

The volume's authors make more than the usual reference to historical events—especially to the outbreaks of the world's wars and the conduct of the superpowers' crises. And yet Francis Fukuyama, formerly of the State Department's policy planning staff and now "senior analyst on Soviet and Middle Eastern Affairs with the RAND Corp.," can assert that "wars arise less often where superpowers stakes are asymmetrical than where they are both strong and evenly balanced" (p. 118). Leaving aside the (unaddressed) question of how one would determine the strength and balance of stakes, we are offered as a factual, indeed historical, generalization a statement

for which there is utterly no historical basis, in that there has never been a single superpower war, let alone enough to allow the conclusion that "such wars arise less often where... than where...." In a similar, although less egregious, instance, Paul Bracken asserts that "indeed since 1945 [nuclear] deterrence has worked," (p. 28) ignoring the important recognition that the absence of something cannot be attributed to the presence of something else without compelling evidence of a causal connection. That evidence—that U.S. nuclear forces actually deterred the Soviets from attacking the U.S.—is not available, of course, either here or elsewhere. Indeed, the book's all-too-short single chapter on "Soviet perspectives on the Paths to Nuclear War" by Stephen Meyer makes the point that we suffer from insufficient and conflicting sources on Soviet decision making.

One wonders whether the analysis might not have profitably ventured further, into the difficult and demanding political and diplomatic context of these dangers of nuclear war, and indeed into the domestic context so significant in the Reagan years. Further, one might hope for presentation of a more developed understanding of the connections and disjunctions between empirical theory based in limited historical experience of even more limited relevance, and policy theory, which, like policy itself, must begin with uncertainty, take and specify risks, and structure analysis and choices to provide for both more venturesome trial and protective reversal in the face of error or surprise.

The editors of this book assert that "human beings can think and, through thought, take initiative to shape their social institutions.... The United States and the Soviet Union must discover ways to overcome history-as-usual" (p. 8). This volume struggles, in a sense, to perpetuate our nuclear history-as-usual of "successful deterrence" by offering its collective agenda. In the end, the editors pledge to "join with others in exploring the realm of ideas beyond deterrence" (p. 246). How about a Carnegie Corporation project on Creating Post-Nuclear-Deterrence Peace?

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Economic Statecraft. By David A. Baldwin.
(Princeton: Princeton University Press,
1985. Pp. ix + 406. \$55.95, cloth; \$15.95,
paper.)

The central argument in Baldwin's study is that critics of the use of economic statecraft have not studied their case histories thoroughly and have looked at specific uses of economic statecraft, such as sanctions, through a very narrow analytic prism. To prove his point, Baldwin reexamines a number of key cases, such as the ill-fated League of Nations sanctions against Italy, the U.S. embargo of oil and scrap iron to Japan prior to World War II, the U.S. embargo against Cuba, the U.N. sanctions against Rhodesia, the post-World War II East-West trade embargo, the U.S. economic sanctions against Iran following the seizure of the U.S. embassy in 1979, the partial grain embargo against the USSR following the invasion of Afghanistan, and the Reagan administration's use of economic sanctions against the USSR following the imposition of martial law in Poland in 1981. He also deals, in a more limited way, with Soviet uses of economic sanctions against Yugoslavia, Albania, and China.

In calling for a reevaluation of the utility of economic sanctions and other forms of economic statecraft, Baldwin contends that there are no fewer than nine factors that must be understood in evaluating the success or failure of these undertakings. The most important of these are the realization that targets and goals of economic statecraft are usually multiple; that the use of a tool of economic statecraft should be evaluated—both as to costs and desired outcomes—against alternative tools of statecraft such as military action; that success is usually a matter of degree and that neither perfect success nor perfect failure is likely; that imposing economic costs on a target nation for noncompliance is itself a measure of success; and that economic statecraft, when used, has important symbolic aspects that can affect a nation's image in the international arena. On this last point, Baldwin, citing Robert Jervis, asserts that the symbolic aspects of economic statecraft will influence the "psychological environments and policies" of foreign decisionmakers (p. 100).

Baldwin, in answering the critics of the utility of economic sanctions, goes beyond merely

rebutting their arguments, however. While noting that economic sanctions lie somewhere between war and appeasement in terms of a continuum of "toughness"—and hence are open to attack from both "hardliners" and "soft liners"—he notes prescriptively that "techniques that enable policy makers to demonstrate firmness while reassuring others of their sense of proportion and restraint can be highly useful, especially to nuclear powers" (p. 105). In addition, in citing the desirability of using economic statecraft as a means of registering approval or disapproval, Baldwin notes that "economic techniques usually cost more than propaganda or diplomacy and thus tend to have more inherent credibility. Military techniques, of course, usually entail higher costs and even more credibility, but their costs may be too high" (p. 107). Baldwin concludes that "compared to other techniques of statecraft, economic measures are likely to exert more pressure than either diplomacy or propaganda and are less likely to evoke a violent response than military instruments." Hence, "they are not merely inferior substitutes for force but first-best policy alternatives" (p. 110).

Given Baldwin's perspective, it is not surprising that the often-criticized American use of economic sanctions against Iran and the Soviet Union during the Carter administration receives very high evaluations. In both cases, the U.S. demonstrated that there could not be "business as usual" while the target state undertook policies that angered the United States; that the U.S. was itself willing to undergo economic costs to demonstrate its firmness (Baldwin notes that in the case of the grain embargo the cost to American farmers was far less than alleged, and the cost to Moscow far greater); and that the U.S. was able to demonstrate its anger prudently, that is, without either driving Iran into the arms of the Soviet Union—something that might have happened if the U.S. employed major military force against Iran—or escalating the Afghanistan crisis with the USSR to the brink of nuclear war.

In looking to a general evaluation of Baldwin's study, it is clear that his discussion of the utility of Western uses of economic sanctions is by far the most useful section of his book, and political scientists will read it to their benefit. His discussion of Soviet uses of

economic sanctions, however, is somewhat weaker. In the case of Yugoslavia, I would disagree with Baldwin's view that Stalin wanted to send signals to other communist countries about the danger of noncompliance with Soviet wishes via economic sanctions (p. 230). Soviet soldiers occupied all the other East European communist states except Albania, which was allied with the USSR against Yugoslavia, so there was little need to send signals to them. Indeed, this was a case where Stalin appears to have thought (incorrectly) that propaganda, diplomacy, and economic sanctions alone might topple Tito.

Other than this small weakness, David Baldwin's book, *Economic Statecraft* can be highly recommended, both to scholars in the field of international relations, and to statesmen grappling with the problem of exerting influence in a very dangerous world. Indeed, those who have criticized President Reagan for his January 1986 economic sanctions against Libya might consider changing their views after reading this book.

ROBERT O. FREEDMAN

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Law, Force and Diplomacy at Sea. By Ken Booth. (Winchester, MA: Allen & Unwin, Inc., 1985. Pp. xiii + 231. \$25.00, cloth; \$12.50, paper.)

Much has been written about the UN Convention on the Law of the Sea (UNCLOS III) in the years immediately preceding and following the signing of the agreement at Montego Bay, Jamaica, on December 10, 1982. This rather expansive body of literature has tended to focus either on the purely legal interpretations of the convention's articles or on the changing strategies of the principal actors, particularly the United States, during the negotiations on a comprehensive treaty. In *Law, Force and Diplomacy at Sea*, Ken Booth, an expert on the law of the sea and a member of the faculty at the University College, Wales, Aberystwyth, goes beyond these limited approaches to present a thorough examination of the changing nature of the law of the sea, particularly as it affects the strategic planning of the world's great naval powers.

The book spells out in detail the background and outcome of UNCLOS III from a strategic perspective, focusing on the major military considerations; the rights of passage through "choke points" (straits and archipelagos); and traditional rights in the seabed, territorial sea, and the exclusive economic zone. This discussion is characterized by attention to the problems of the major naval powers as they threaded their way through years of debate with those nations who argued for "creeping jurisdiction" over the oceans, or what Booth terms the "territorialization" of the sea—extending rights and duties concerning order, the exploitation of resources, and the exercise of sovereignty by coastal states. The story of how the U.S. changed its position from advocacy to one of opposition is also told in detail.

From an analysis of the 1982 convention, Booth draws four major conclusions. First, it satisfies the military requirement of the naval powers. Second, the text is a "masterly compromise" between the interests of the naval powers and the remaining members of the international community. Third, there are, nonetheless, ambiguities and potential sources of dispute with respect to the military domain. And fourth, the naval powers will judge success by what happens in practice.

It is to this last conclusion that Booth turns in the second half of the book. In it he establishes a future agenda for discussion of the military implications of the changing law of the sea. Despite his fundamental conclusion that naval interests have been well served by UNCLOS III, he argues rather persuasively that the compromises by naval powers in earlier conventions may not be enough in future negotiations on the law of the sea (UNCLOS IV, UNCLOS V, etc.). The classical paradigm *mare liberum*, or freedom of the sea, is now under attack, as more and more states argue in favor of *mare clausum*, or a closing of the sea. This push for a new ocean regime manifested itself in several ways at UNCLOS III, most notably in the provisions for a 12-mile territorial sea, the 200-mile exclusive economic zone, and the international authority for deep-seabed mining. In Booth's thinking these represent a clear sign that the future bodes ill for the world's naval powers, if not in 5 years perhaps in 50. The last chapter is devoted to a thoughtful analysis of how navies might address this trend in the years ahead.

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The book is designed for multiple audiences: students of naval strategy, international lawyers, and those interested in international affairs. Despite such a multifaceted audience, Booth has succeeded in bringing clarity to a complex subject while simultaneously raising important issues for future discussion, and it is recommended reading for serious members of the three groups named above. This reviewer's enthusiasm would be even greater were it not for the frequent and sometimes caustic anti-United States commentary that finds its way into the book. Such value judgments have no place in this otherwise excellent display of scholarship.

JAMES E. HARR

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Revolution and International Politics. By Peter Calvert. (New York: St. Martin's Press, 1984. Pp. 222. \$25.00.)

Social Science and Revolutions. By Stan Taylor. (New York: St. Martin's Press, 1984. Pp. 176. \$25.00.)

These two books by British political scientists have little in common other than their lexical concern with revolutions. Calvert offers disappointingly discursive commentaries on the international system and on the forms, causes, and ramifications of revolution, with occasional observations about the political linkages between international and revolutionary politics. Taylor contributes an incisive and critical set of summaries of the scholarly theories of revolution written during the 1960s and 1970s; this is substantially the best book of this genre yet published.

Calvert's general objective is "to explore the nature of revolution in the context of international politics" (p. 1). He opts for a broad definition of revolution as "the forcible overthrow of a government or regime" (p. 2), a definition that accommodates wars of national liberation (chap. 3) and what are said to be the three main forms of revolutionary action: coups, guerrilla action, and urban insurrection (chap. 6). The traits of each form are discussed with extensive examples. Constituents of a theory of the internal causes and processes of revolution—for example, a sketch of the strategic stages in revolution: preparation, action, and consolidating—are scattered

throughout the book. A thoughtful discussion of the psychological and social bases of revolutionary leadership and recruitment (chap. 5) is bracketed with an irrelevant and factually questionable chapter on the culture of violence (chap. 4). There is a stale quality to these chapters. For example there is no attempt to incorporate any of the arguments of theorists of revolution such as Barrington Moore, Samuel Huntington, Charles Tilly, or Theda Skocpol, among others; Huntington and Tilly are not even cited in the bibliography. In fact in a bibliography of 156 items, only 37 were published after 1970.

The international dimensions of Calvert's analysis include a discussion of force in the international system (chap. 2); an accounting of the problems revolution poses to traditional diplomacy, especially regarding the recognition of new regimes (chap. 7); and an analysis of diplomatic and military intervention in potentially revolutionary situations (chap. 8). At several points Calvert refers to dependency theories and to economic intervention, but virtually dismisses them. Since economic penetration does not involve specific acts by governments to influence politics in other countries, it is not "intervention" (pp. 165–66), and the United States is not "neocolonial" because it lacks effective checks on private investment abroad (p. 54). Theories and evidence that international banks and corporations influence both the foreign policies of capitalist states and the domestic policies of host countries, with profound implications for political stability and revolution, are not discussed.

In sum, the book has little to offer to the conflict analyst aside from some obscure but interesting illustrative materials, such as a nine-page summary of *Armed Insurrection*, a 1928 Comintern manual on urban insurrection. One may applaud the author's "principal conclusion" that "citizens should in future expect and demand of their governments a policy of enlightened self-restraint" with regard to revolutionary conflicts elsewhere (p. 200), but one should not expect to find a sustained or coherent argument that leads to this or any other general conclusion.

The core of Taylor's *Social Science and Revolution* is a set of summaries and critiques of four different bodies of theory on the causes of revolution. No favorites are played. Unlike Barbara Salert's *Revolutions and Revolutionaries* (New York: Elsevier, 1976), this book

gives equal treatment to each of the bodies of theory and evidence without the *a priori* conviction that one is superior to another. Nor is it the author's purpose to lay waste to other theories as a preliminary to demonstrating the superiority of his own. Far from least, the summaries of the theoretical arguments are masterfully done. The reviewer is closely familiar with most of the dozen theories reviewed here, and did not find a single serious misstep in their presentation. Each is characterized fully, accurately, and with sensitivity to their nuances.

Following a brief but useful intellectual history of the concept of revolution, a cluster of "sociological" theories of revolution are evaluated. These divide into two sharply distinct groups: functionalist theories, rooted in Parsonian analysis and represented by Chalmers Johnson; and conflict-coercion theories, based on some of Marx's arguments and represented here by Moore and Skocpol. Particularly useful is the reduction of each theory to a schematic model showing the alternative sets of conditions said to lead to each specified kind of revolutionary or nonrevolutionary outcome. These sketches alone are worth the price of admission for anyone teaching a course on revolution or looking for a quick summary of the underlying logic of the theories. Johnson's theory of revolution is faulted because of the profound difficulties of making its concepts operational, whether in quantitative or case studies. Moore and Skocpol are criticized for their neglect of voluntarist psychological and political causes of revolution. The summary judgment of Skocpol is particularly succinct and telling:

Her claims concerning inevitability and the primacy of structural variables could only be fully substantiated if she had developed her model independently of particular cases and then demonstrated its predictive powers . . . or if she had transformed conclusions based upon case studies into hypotheses which could be tested by reference to other causes. (p. 50)

Taylor's analysis of "socio-psychological" theories distinguishes between those which rest on Festinger's theory of cognitive dissonance, represented here by the work of James Geschwender and David Schwartz; and those using frustration-aggression theory, in particular Davies' J-curve theory of revolution and Gurr's relative deprivation theory of

political violence. The early macrocomparative research inspired by these theories is carefully summarized and critiqued on the familiar ground that such empirical work could not demonstrate directly the existence of any connection between psychological processes and the occurrence of political violence. Unaccountably missing is any mention of Edward N. Muller's theoretical and microempirical research on the social psychology of aggressive political behavior, which developed out of just such an empirical assessment of these theories. Taylor also makes another point often overlooked in critiques of these theories: they are concerned not with explaining revolutions *per se*, but with the extent of political violence and individual participation in it.

The author's evaluation of "economic" or rational-choice theories of revolution is less satisfactory, because it is limited to three early statements of such theories—by Ireland, Tullock, and Silver—and does not take into account subsequent theoretical and empirical attempts to rescue rational explanations of revolutionary action from the free-rider problem.

Two distinct "political" theories of revolution are reviewed: Huntington's functionalist theory (actually a theory of political instability, which subsumes revolution) and Tilly's resource mobilization approach. As in chapter 2 figures are used to summarize the essential arguments. Taylor praises these theorists for their efforts to link economic, social, and political variables, and to go beyond narrow etiology to consider both the development of revolutionary situations and their outcomes. The principal criticism of Huntington is his failure to give enough weight to the ability of regimes to maintain themselves by coercion. Tilly is faulted both for tautological elements in his argument and for his exclusion of institutional and sociopsychological factors.

Since Taylor often criticizes one body of theory by pointing out that it fails to take account of variables that are central to other theories, one might expect that his concluding chapter would move toward synthesis. Instead it becomes mired in a metatheoretical discussion about the ideological basis of scholarly theories of revolution. He begins with the accurate observation that revolutionary phenomena are defined very differently in the

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theories reviewed. He next assumes that "Revolutions are not naturally-defined phenomena comparable to those studied in the natural sciences" and concludes that any attempt to define them is therefore inherently ideological (p. 152). The conclusion does not necessarily follow. While there is some inherently irreducible element of "interest" in a scholar's initial definition of the phenomena under study, the central purpose of a theoretical definition is to identify an unambiguous subject matter that is susceptible to analysis and explanation. There can be little doubt about the inherent observability of such explananda as "participation in revolutionary movements" or "extent of political violence" or the "forcible overthrow of governments or regimes." The differences among definitions of revolutionary phenomena reflect differences in disciplinary paradigms and the personal interests of researchers more than they do ideological dispositions. Given an operationally defined subject matter, the processes of explanations and comparative research can proceed irrespective of the interests that may have influenced the initial definition.

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Consultation and Consensus in NATO: Implementing the Canadian Article. By Edwina S. Campbell. (Lanham, MD: University Press of America, 1985. Pp. xviii + 209. \$26.00, cloth; \$12.75, paper.)

This book discusses the origins and development of the NATO Committee on the Challenges of Modern Society (CCMS). CCMS was born out of an American proposal in response to the ever-increasing problems of protecting the environment. The idea for CCMS was based on article 2 of the North Atlantic Treaty, better known as the Canadian Article.

The idea of greater social and cultural cooperation between members of the Atlantic Alliance is an interesting one indeed, and raises several important questions. One would expect, as the preface suggests, that a book dealing with the development of international

cooperation within NATO through CCMS would address the following questions. First, what are the theoretical premises and assumptions of CCMS as a cooperative international organization within the international system? Given the presence of CCMS as a cooperative organization, how does the dominant neorealist conception of international relations fit or reject the situation of CCMS? Second, what was the relationship between CCMS and other international organizations with competing jurisdictions such as the Organization for Economic Cooperation and Development (OECD), the UN, and the Council of Europe? Third, what was the importance of external environmental factors and the inter-Alliance pressures on the internal decision-making process within NATO as well as within CCMS? Finally, what does the failure to implement the Canadian Article indicate about either the implicit value of the proposal or the workability of the idea of non-military cooperation in a strategically-defined alliance?

Consultation and Consensus in NATO unfortunately fails to answer any of these questions sufficiently, and addresses only the question of internal political processes with some degree of clarity and comprehensiveness. The book provides virtually no significant theoretical discussion of international organization. The author fails to put the existence of CCMS into the context of its international environment and in terms of its implications on international order and state cooperation on non-military issues. Campbell does best with the query on the subject of the internal decision processes of the Committee and of NATO. She describes the impact of the American influence upon the formation as well as the operation of CCMS. Finally, by initially maintaining that CCMS was a workable solution to international environmental problems, Campbell fails to acknowledge and therefore address the question of the value of CCMS.

Campbell's study makes three modest contributions to our understanding of the subject of the principles behind the Canadian Article and the relationship between the U.S. and NATO. First, she does present, although roughly organized and questionably documented, a case study of the development and operation of CCMS. *Consultation and Consensus in NATO*, while not comprehensive, sheds some light upon the process and progress

of CCMS, and upon the subsequent unsuccessful implementation of the Canadian Article. Second, the author features an interesting look at an example of the phenomenon of "close-of-business" (COB) attention in the bureaucracy by citing the behavior of U.S. State Department foreign service officers. Finally, the book gives us some idea, albeit with limited predictive quality, as to how an issue with strong or sole U.S. backing in NATO will be handled.

Consultation and Consensus in NATO disappoints and frustrates the reader much more than it rewards him with understanding and knowledge of cooperation within NATO. The lack of a theoretical base throughout the book is most disappointing. Along with organizational unruliness, the book has many irrelevant statements and quotations, some of which have incomplete citations or no citations at all. In addition, the prose quality of the book is so choppy that reading it becomes something of a chore. Additional criticisms of Campbell's work center on her less than professional and impartial scholarship. She makes little attempt to neutralize her adoration of the policies of the first Nixon administration and her disdain for liberals who call for stringent environmental controls: "Their case was similar to that of the supporters of complete nuclear disarmament, and it did the environment the same disservice which the all-or-nothing anti-nuclear movement has done the supporters of arms control" (p. 50).

ROBERT S. JUNN

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Measuring Military Power: The Soviet Air Threat to Europe. By Joshua M. Epstein. (Princeton: Princeton University Press, 1984. Pp. xxx + 288. \$22.50.)

Accurate threat assessment in American defense planning has long been hampered by overconcentration on such static measures as overall force size, known or assumed weapons characteristics, and similar tangles of enemy military capability. More often than not, this narrow approach has ignored those "softer" variables relating to human performance (such as training, tactics, sustainability, and operational prowess) that could be pivotal in deter-

mining the actual combat leverage of an adversary's force posture. Although military balance appraisals based solely on the technical dimensions of enemy weapons inventories may or may not routinely overstate the resultant "threat," they can be counted on to yield a misleading portrait of enemy capability if viewed in isolation from operational context.

Using Soviet tactical airpower as a case in point, Joshua Epstein seeks to take on this problem by illuminating those stylistic attributes of Frontal Aviation, the Soviet Air Force's tactical air arm, that would bear most heavily on NATO commanders in the event of a major nonnuclear confrontation in Europe. His main premise is hard to quarrel with:

Virtually the entire defense debate . . . concerns itself not with wartime outputs, but with peacetime inputs—static inventories of men and machines. Negligible attention is paid to the operational inputs involved in taking those peacetime inputs (e.g., tanks, planes) and producing a wartime output—achieving any specific military goal. (p. ix)

His avowed objective is equally laudable: To consider whether the Soviet Air Force actually possesses the needed wherewithal to execute the canonical mission commonly attributed to it, namely, destroying NATO's nuclear response capability by means of a massed and carefully-orchestrated conventional attack. The central argument of his book is that the practical capacity of Frontal Aviation in this assumed primary role is, for a variety of reasons, much less than one would gather from a superficial look at its order of battle.

As far as it goes, this is a reasonable point of view. The problems crop up in the way Epstein develops his argument. To begin with, over a third of the book is devoted to complex mathematical gymnastics that address variables (based largely on second-order assumptions about maintenance efficiency and sortie-generation capacity) whose relevance to Soviet combat effectiveness—especially during the critical early days of a conflict—the author merely asserts but does not explore. He thus begins by failing to ask the right questions. He does not adequately distinguish between Soviet peacetime and wartime practices. He gives insufficient credit to the ability of even hide-bound Soviet generals to ride a steep learning curve once the pressure is on. Most of all, he expends a great deal of needless energy

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manipulating "data" that are too fragmentary to support his sweeping generalizations and that have a less-than-immediate bearing on Soviet combat capability in any case.

True enough, one can advance a cogent argument that Frontal Aviation, as currently equipped and trained, would have a difficult time defanging NATO's nuclear option by conventional means alone. However, Epstein overlooks those factors that would probably have the greatest effect in blocking the success of any such operation, namely, Moscow's lack of adequate hard-structure munitions, the quality of NATO's air defenses, the poor adaptability of Soviet aircrews in the face of rapidly changing combat conditions, and the abysmal weather environment of Europe that would frustrate the precision high-angle delivery of conventional bombs—which would be required to destroy NATO's runways, hardened aircraft shelters, and nuclear storage bunkers—throughout most of the year. Had he more carefully examined the public record on Soviet tactical air equipment and operating routines, he could have advanced a more plausible case for his hypothesis—and one that did not need all the mathematical contortions to stand on its own feet.

Leaving aside this misdirected number-crunching, Epstein does not provide much accounting of Soviet tactical air competence, even in qualitative terms. Indeed, for all its claim to empiricism and analytical rigor, it is remarkable how little of a factual nature his study provides on the Soviet tactical air forces. He offers a potpourri of interesting quotations from the Soviet press about various issues that Soviet pilots and commanders periodically complain about, yet he presents no systematic treatment of Frontal Aviation as a combat organization, how it fits into Soviet theater war planning, or what it can and cannot do with its diverse assets in specific mission profiles.

Although the book purports to undertake a "close and careful job" (p. xx) of examining Frontal Aviation, its conclusions rest on no more than a couple of dozen translated Soviet articles randomly scattered over a number of years, most of them concentrated around 1977. At best, this material offers only a coarse-grained snapshot of certain aspects of Soviet Air Force activity that the Soviets at that time found appropriate to depict in their

open literature. It certainly does not give us an aggregate or even a representative partial picture of Frontal Aviation operations and training today. One gets the impression that the author based his analysis entirely on what was available to him in English, which borders on looking for one's keys where the light is best. At no point does he critically allow for the various internal functions of the Soviet material he draws upon. Instead, he repeatedly accepts the sources he cites at face value as straightforward reflections of underlying Soviet reality. Necessarily, the result is a skewed image of Frontal Aviation. Not only does it exclude treatment of some important (and documentable) Soviet operational weaknesses that would affect Soviet employment potential even more acutely than the maintenance inefficiencies he postulates; it also overlooks many of the persistent strengths of Soviet tactical airpower that must be considered in any assessment of offsetting deficiencies.

Had the author provided a more thorough account of Frontal Aviation's operational complexion (even within the limits of the available evidence) and shown a way of applying metrics to those intangibles that really matter—aggressiveness, leadership virtuosity, tactical acumen, adaptability under stress, and the like—he might have produced a real breakthrough toward greater sophistication in assessing Soviet combat capability. As it stands, he has succumbed to the common error of measuring with a micrometer, marking with a grease pencil, and cutting with an axe. In the end, his judgment that Frontal Aviation would have a hard time carrying out a successful air operation against NATO's key rear-area targets emerges less from his quantitative manipulations than from his largely intuitive assumptions about Soviet maintenance efficiency and aircrew skill. The judgment itself is scarcely objectionable and, indeed, is widely shared by many of those defense professionals whom the author indiscriminately castigates as "bean-counters." Yet Epstein is deluding both himself and his audience if he believes that this conclusion stems from anything more than intellectual Kentucky windage, at least to the extent his analysis has been carried in this book.

To recall an old phrase of Stanley Hoffmann's, *Measuring Military Power* can best be summed up as a well-intentioned misstep in the

right direction. Clearly the author is to be commended for having highlighted the need for a kind of analysis that is long overdue in the field of threat assessment. However, this book offers far too little substantiated insight to be of much practical value to U.S. force planners—even those who share Epstein's predilections and could sorely use the help. A "close and careful" treatment of Soviet tactical air strengths and vulnerabilities is very much the kind of volume that would stand in welcome stead in the defense research community. Unfortunately, it is also a volume that remains yet to be written.

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The United States and Multilateral Resources Management. Edited by Robert S. Jordan. (New York: Praeger Publishers, 1985. Pp. ii + 200. \$25.95.)

The majority of the eight essays in this volume stem from a conference on "The United States and Multilateral Resource Management" held at Brigham Young University in 1984. Renninger begins the volume with a brief introduction in which he presents the major issues: (1) the extent to which multilateral approaches to managing marine mineral, energy, and food resources serve or advance U.S. national interests; (2) the current trend in U.S. policy in favor of unilateral and bilateral approaches as opposed to multilateralism, and the trade-offs involved in these choices.

The energy issue is examined by Joan Edelman Spero in "Oil and the Cartel Power," and by Richard J. Kessler in "No Fuel for Development: United States Policy toward Managing International Resources." Both authors provide exceptionally exciting overviews of the changing structure of the international energy market. However, Edelman Spero's essay, like several other chapters, fails to focus directly on the supposed theme—the question of how multilateralism in resource management can serve U.S. interests.

In a richly documented, descriptive contribution, "American Multilateral Food Diplomacy," Hopkins and Puchala show how the

policies of major food organizations can serve U.S. food goals. Janet Schmidt addresses "Some Issues in the Multilateral Management of Food Resources." Her findings that "the United States cannot unilaterally ensure adequate levels of price stability and food security" (p. 90) whet the reader's appetite for policy-relevant prescriptions.

The weakest point of the essays mentioned above lies in their lack of an explicitly theoretical approach, although what these authors lack in theoretical perspective, they make up for in their impressive display of knowledge of the issues.

In many ways, the most interesting essay is that of Robert A. Coate, who provides a remarkably lucid demonstration of the linkage between the International Seabed Authority (ISA) and the call for New International Economic Order (NIEO). His effort to explain ISA and NIEO with reference to Wallerstein's "world system" theory is intriguing, although not everyone will be comfortable with his attempts to tie the marine mineral issue to Wallerstein's mega-theory.

In the final chapter Jordan underscores the volume's theme: In food, energy, and marine minerals, the U.S. can not work out its problems in isolation from the states affected by these issues.

Not all chapters are equally enlightening. The volume's sketchy historical treatment and limited theoretical insights preclude its emergence as a pioneer on the subject. On balance, though, this is an interesting and worthwhile contribution to the international political economy and U.S. foreign policy literature. Several chapters would make excellent assigned reading for graduate and undergraduate courses, especially those on energy and food resources.

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People in Space: Policy Perspectives for a Star-Wars Century. Edited by James Everett Katz. (New Brunswick, NJ: Transaction Books, 1985. Pp. ix + \$29.95, cloth; \$14.95, paper.)

As this review is written, the search continues for remains of the space shuttle

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Challenger, which lies in ruins on the ocean floor. With this tragedy the future of the American space program itself seems submerged in doubt. Even before the shuttle disaster, a national space commission was considering future goals for American space policy. Clearly now some soul-searching assessments of our future in space are inescapable. Against a backdrop of policy transition this collection of essays can lay claim to special attention. From a policy perspective, however, it is impossible to ignore some of this volume's significant shortcomings.

Why should space be of interest to political scientists and policy analysts? A number of the essays in this book suggest answers. They highlight issues of direct interest to political scientists: the international implications of the militarization of space; the future of international organizations and agreements for the regulation of extraterrestrial resources; international disputes over the use of remote sensing data or over the allocation of increasingly scarce space within the geostationary (Clarke) orbit. Further, these essays direct our attention to the politics of large-scale R&D projects; to the sociology of spaceflight as a major public policy "movement"; and even to the politics underlying the search for extraterrestrial intelligence. As James Katz notes in his introductory essay, many of the policy issues underlying spaceflight have been ignored by social scientists, and as public funding has declined for the space program, so has much analytic interest.

At the same time that this volume directs our attention to important issues for political analysis, the utility of these essays for such analysis varies widely. Some, like those of former astronaut Harrison Schmitt and John Joseph Moakley are rather empty polemics for particular space policies. (Schmitt argues that as a counter to the Soviets we should simply make a commitment "to go into space and stay there permanently." Moakley offers a denunciation of the Reagan administration's space-based defense or "Star Wars" program.) Some of the essays offer simple descriptions of current space activities without much in the way of analytic elaboration. There is an inventory of international agencies devoted to orbital telecommunications; a chronicle of international agreements covering space exploration and celestial exploitation, and a listing of

disputes that have arisen regarding remote satellite sensing.

Among the most interesting analytic essays are Nathan Goldman's exploring the politics behind the Senate's rejection of the 1979 Lunar Treaty, George Robinson's questioning the power of legal agreements to regulate future space settlements, and the opposing essays by William Sims Bainbridge and James Katz on whether popular movements or bureaucratic politics are the driving force behind programs of space exploration. Despite the subtitle of the book ("Policy Perspectives for a Star-Wars Century") readers interested in an analysis of space-based defense issues will find little to learn from here, and would be well-advised to look elsewhere for a more useful treatment.

Ironically, one of the issues ignored in this book is defined by its title. Nowhere in these essays is there an explicit argument for people in space, nowhere a defense against purely automated approaches to exploration and science-based space research. This question is precisely at issue in the future of the space program. It has been dramatically restated in the space shuttle disaster.

Books like this are suffused with a special kind of technological optimism. It no doubt reflects the psychology of most of the authors, and it is a major drive behind the compilation of a reader such as this. Yet it is important to realize that from a policy standpoint, technological optimism is not programmatically neutral. It tends to bias its adherents to discrete policy positions regarding the uses of space and the pace and scale of those programs we pursue in its exploration. For example, Hans Mark bases his support for space-based defenses almost solely upon Arthur Clarke's maxim of technical optimism: "When a distinguished scientist says that something is possible, he is almost certainly right. When he states something is impossible, he is very probably wrong" (p. viii). Similar psychologies closely attend endorsement of manned-centered approaches to exploration—including the construction of large space stations and human colonies in space. To be sure, there are some essays critical of the Star-Wars program, and some which do not regard new space technologies as unalloyed blessings, but the major problem ignored throughout this book (and one that has plagued the space program from its beginnings is precisely that of balance-

ing and containing its reinforcing psychology. How can political support for space exploration be sustained without the psychology of exploration overinflating the policy and distorting its thrust?

The American space program has been unable to locate a stable policy "niche" between the "driving dream" of manned missions at an accelerating pace and what would probably be a marginal (and politically vulnerable) policy of purely science-based instrumented flight. This is a peculiar dilemma of space policy. It is not, as this book declares in its preface, "time for policy analysts to move from viewing space as an exceptional phenomenon to simply a part of everyday life" (p. 1). Indeed, it is just such an assumption that makes books like *People in Space* implicit promotions of a policy doctrine more than detached exercises in policy analysis.

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Korea and the United States: A Century of Cooperation. Edited by Youngnok Koo and Dae-Sook Suh. (Honolulu: University of Hawaii Press, 1984. Pp. xiv + 397. \$37.00.)

As a global superpower the United States has forged innumerable sets of bilateral links with nations large and small. One of the more noteworthy of these is with Korea. Not only did the United States fight a war in Korea, it has also committed itself to the defense of South Korea, a commitment that is embodied in a mutual defense treaty as well as in the stationing of 40,000 U.S. troops in that country. Moreover, South Korea has emerged as America's seventh largest trading partner.

In 1982, in commemoration of the centennial of diplomatic relations between Korea and the United States, a scholarly conference was held in Honolulu under the joint auspices of the Center for Korean Studies of the University of Hawaii and the American Studies Institute of Seoul National University. The volume under review is an offshoot of that conference. Sixteen scholars from South Korea and the United States examine the whole gamut of American-Korean relations. The principal topics discussed are the genesis of American interest in Korea, mutual perceptions and

images, the American-South Korean alliance system, the role of the other Pacific powers, economic relations, and the impact of American-Korean relations.

Ten of the authors are political scientists; the remainder represent sociology, economics, business, law, history, and philosophy. Such a diversity in the authors' training and interests helps account for variations in the approaches used and in methodological rigor. Nonetheless, the overall caliber of the papers remains high; taken together, they present an excellent overview of the track record of Korean-American relations from their inception to 1982.

The relations officially commenced with the signing of a treaty of amity, commerce, and navigation in May 1882 at the Korean port of Inch'on by Commodore Robert W. Shufeldt of the U.S. Navy and Sin Hon, president of the Royal Cabinet of the Kingdom of Korea. Earlier attempts by the United States forcibly to open Korea had failed in the face of stiff resistance by the Koreans, and it was through the mediation of Chinese Viceroy Li Hung-chang that Shufeldt finally succeeded in negotiating the treaty. The role of China in the early stage of American contacts with Korea not only bespoke China's influence in Korea, which was soon to be eclipsed by Japan's, but also foreshadowed the Sino-American military confrontation there seven decades later.

Despite their initial resistance, Korea's ruling elite, particularly the king, enthusiastically welcomed Americans to their soil, perceiving them as a counterweight to the predatory maneuvers of Korea's powerful neighbors. Cultural differences and wishful thinking had combined to spawn the hope that America would help Korea preserve its independence, for Article 1 of the 1882 treaty provided that "if other Powers deal unjustly or oppressively with either Government, the other will exert their good offices . . . to bring about an amicable arrangement, thus showing their friendly feelings." The Koreans were rudely disillusioned in 1905, when the U.S. in the secret Katsura-Taft memorandum, recognized Japanese suzerainty over Korea in return for Japanese recognition of American supremacy in the Philippines.

When the Americans reentered the Korean scene 40 years later, they came as liberators bent on helping the Koreans to rebuild a viable

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political order. However, the "temporary" division of the peninsula into two occupation zones hardened into a permanent partition. If, as many Koreans believe, America erred in that decision, it has paid dearly for the error. Not only did it fight a costly war against both North Korea and the People's Republic of China from 1950 to 1953, but it has been entangled in the Korean quagmire ever since.

With an impressive growth in South Korea's economic capability over the past two decades, however, the alliance system between the U.S. and South Korea has undergone profound change. By 1978 South Korea had attained self-sufficiency in national defense. Both direct and indirect American military aid ceased altogether. Since North Korea's arms buildup has continued unabated, however, both Seoul and Washington believe that the American military presence in South Korea, coupled with the mutual defense treaty between the two countries, serves a useful deterrent function.

In the realm of economic relations, too, the nature of American-South Korean ties has been transformed from "unilateral asymmetry" to "bilateral symmetry." No longer a recipient of significant American aid, South Korea has emerged as one of the principal trading partners of the U.S. Not only has South Korea stopped being a burden on the U.S., but Seoul has become a formidable competitor of Washington in the American market and beyond.

While the Koo-Suh volume elucidates these and other aspects of Korean-American relations in an authoritative fashion, it is not totally devoid of problems. First, its treatment of North Korea leaves something to be desired, not because its chapter on North Korea is flawed in any way, but because it devotes only one chapter to that significant half of Korea. Second, anyone who is looking for an in-depth analysis of friction between Seoul and Washington is bound to be disappointed. Neither Syngman Rhee's friction with Washington in the waning days of the Korean War nor Park Chung Hee's difficulties with Washington over human rights and lobbying efforts receive any sustained attention. Finally, one can detect the understandable tendency of South Korean-based scholars to exercise self-censorship.

All in all, this is a welcome addition to the

growing literature of Korean studies. An impressive testimony to the pervasive impact of the U.S. on Korea is that all of its 16 authors, of whom 15 are of Korean descent, have received their graduate training in the U.S.

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The Soviet Union and Strategic Arms. Robbin F. Laird and Dale R. Herspring. (Boulder, CO: Westview Press, 1984. Pp. x + 160. \$28.50, cloth; \$16.95, paper.)

Most studies of the Soviet Union and strategic arms fall victim to at least one of three major methodological flaws. First, some studies fail to approach strategic issues from within the contexts of Soviet perspectives and Soviet strategic culture. These studies inevitably see Soviet strategic policy as "aggressive" and dominated by a desire to achieve nuclear superiority. Second, some studies fail to acknowledge even the possibility that the Kremlin's strategic doctrine and policy may have changed over time. These studies blithely maintain that formulations of strategic doctrine and policy that were valid in the 1960s remain valid today. Often they use quotes mined from journals of that earlier period to "prove" points about Soviet strategic doctrine and policy in the 1980s. Finally, some studies eloquently support one or another thesis about Soviet strategic doctrine and policy, using Soviet quotes and weapons deployments as proofs while ignoring or rejecting the legitimacy of quotes and deployments that tend to support alternate theses. In short, this third methodological shortcoming essentially allows some studies to overlook the existence of disagreements within Soviet decision-making elites over strategic doctrine and policy.

Happily, *The Soviet Union and Strategic Arms* by Robbin F. Laird and Dale R. Herspring avoids these methodological pitfalls, and in so doing presents a lucid, balanced, and objective assessment of the Kremlin's attitudes towards nuclear weapons in the age of strategic parity. Laird and Herspring examine a large quantity of qualitative and quantitative evidence in this rather short book. Equally im-

portant, they examine the entire spectrum of evidence, not just those segments that would tend to make the USSR appear an "evil empire" or a "threatened innocent."

Indeed, the real strength of the Laird-Herspring effort is the willingness of the authors to come to grips with the range of information available on Soviet strategic policies. To their credit, they attempt to develop a set of explanations about Soviet policies consistent with that range of information, and to a great extent, they succeed. They underline the uncertainty that pervades many of the USSR's calculations about nuclear peace and nuclear war, and they provide thoughtful and worthwhile discussions of Soviet perceptions of the political utility—and limits—of nuclear weapons. Additionally, Laird and Herspring offer an insightful but overly brief overview of Soviet perceptions of U.S. strategic policies, an area of inquiry too often overlooked, but nevertheless critical if one wishes to comprehend the intricacies of the Kremlin's own strategic doctrine and policy.

On the whole, *The Soviet Union and Strategic Arms* succeeds well in its stated purpose of providing "a general overview of the Soviet strategic arms effort" (p. 1). Ironically, however, if there is one area in which the book may be criticized, it is that the authors were sometimes too intent on providing an overview. On some occasions, issues of major debate should have been addressed in greater depth than they were. For example, the Soviet approach to the conduct of nuclear war probably deserves more attention than the 18 pages devoted to it. This is not to find fault with the authors' observations and conclusions about the Soviet approach to the conduct of nuclear war, but to argue that, in places, more depth should have been provided to the overview.

This criticism, however, is quibbling. Laird and Herspring have written an objective book, devoid of polemics, on a most critical issue. They offer explanations and interpretations of Soviet behavior in the strategic nuclear arena that are consistent with the range of evidence available in that field. In short, they have provided a real contribution to the understanding of the Soviet strategic arms effort.

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Soviet Military Interventions Since 1945. By Alex P. Schmid, with case studies by Ellen Berends and foreword by Milton Leitenberg. (New Brunswick, NJ: Transaction Books, 1985. Pp. xix + 223. \$29.95.)

Military Intervention in the Third World. Edited by John H. Maurer and Richard H. Porth. (New York: Praeger, 1984. Pp. xix + 239. \$29.95.)

Anyone picking up *Soviet Military Intervention Since 1945* would be well advised to turn immediately to page 123, for it is only here that the authors attempt to set out for the reader their working definition of *intervention*. They define by listing 11 categories of activity ranging from the mobilization of troops in border areas to the direct engagement of Soviet combat troops. The book is divided into four parts: The first part deals with intrabloc interventions (i.e., Soviet interventions in socialist states), the second with interbloc interventions (i.e., against core Western states), the third with extrabloc interventions (i.e., in the Third World); the final part consists of conclusions and projections. Each of the first three parts begins with a general survey by Alex P. Schmid, followed by case studies by Ellen Berends (both researchers at the Centre for the Study of Social Conflicts, State University of Leiden, the Netherlands).

The introductory summary in chapter 1 traces the incorporation of Eastern Europe into the Soviet orbit, but no real attempt is made to explain Soviet motives and behavior, and the larger global context is ignored. The United States, France, and Great Britain are barely mentioned. There are five cases in this section: the incorporation of the Baltic states into the USSR, the East German uprising of 1953, the 1956 Hungarian uprising, the Prague Spring of 1968, and the Sino-Soviet border dispute during the 1960s. Most of these case studies, like those in the other sections, are little more than short descriptive essays with no overall framework of analysis tying them together. There are four interbloc case studies: the Iranian crisis (1945-1946), the Greek Civil War (1944-1949)—which is presented as a case of "nonintervention"—the Korean War (1950-1953), and the Soviet role in the allied occupation of Austria (1945-1955). The one extrabloc case study is the Soviet intervention in Afghanistan.

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For case studies of Soviet military intervention to have any value, they need to be comparative in focus and structured, employing a single, consistent framework of analysis. Unfortunately, this book falls down on each of these counts. It is also a pity that each of the case studies concerns Soviet interventions in states that form what one might term the USSR's geostrategic security belt. Thus, any generalizations one could draw from the study might not be applicable to Soviet interventions, say, in Africa or Latin America (although these are mentioned in the general surveys). The sources used are entirely secondary and not terribly comprehensive. It is certainly not the case, as is claimed, that there is no historical material "which would reveal what Stalin's intentions for the postwar period were with regard to the conquered territories" (p. 2). In places the English is clumsy, but not so as to make the basic arguments unclear.

Military Intervention in the Third World consists of a collection of essays originally presented at a conference held in December 1981 by the Foreign Policy Research Institute, and designed to offer policy options for the U.S. government in meeting the Soviet challenge to American interests in the Third World. Following an introduction by John H. Maurer comparing U.S.-Soviet rivalry in the Third World with the European powers' competition in the lesser-developed regions during the period 1875-1914, the book is divided into three parts.

The first part deals with the nature of the (Soviet) threat. Alvin Z. Rubinstein examines post-Stalin Third World military interventions, differentiating between *intervention*, defined as the "direct and intrusive projection of military power on behalf of a Third World client in order to bring about a preferred political outcome" and *involvement*, such as the provision of arms and personnel (p. 20). Examining interventions in the Congo (1960), Cuba (1960-62), Egypt (1970), the Middle East (1973), Angola (1975-76), the Horn of Africa (1977-78), and Afghanistan (1979) leads Rubinstein to a number of generalizations regarding Soviet military interventions: (1) strategic factors are most important (i.e., not ideology); (2) the USSR will intervene only to support "progressive," anti-imperialist regimes; (3) once committed, they will stay the course; (4) they show little sensitivity to

regional dynamics, and (5) no consideration of the central strategic balance, but (6) will intervene even where this risks superpower relations (nos. 3 and 6 might well be questioned by the Congolese, Egyptians, Syrians, Cubans, and Angolans). Rubinstein predicts that it is likely that Syria, Libya, South Yemen, and Iran "will require some kind of direct Soviet military intervention" (p. 31). Gordon H. McCormick examines the participation of Soviet "proxies" in regional conflicts. His basic assumption, which is not adequately documented, is that Cuba, South Yemen, and East Germany operate abroad "less as independent forces than as surrogates in support of Soviet interests" (p. 45). Dov S. Zakheim looks at locally generated contingencies (e.g., in Lebanon and Central America) to which the United States should develop a positive response and forward strategy.

The second part of the book assesses constraints on the U.S. in conducting such a forward strategy. The three chapters here, by W. Scott Thompson and Andrew B. Walworth, Terry L. Deibel, and Norman Friedman, set out internal constraints (Congress, the media, and public opinion), external constraints (opposition from major security allies and friends in the Third World), and technical and logistical constraints (regarding military capabilities for intervention) that together hamper American potential to project its power in regional conflicts in competition with the Soviet Union.

The last three chapters offer policy advice to American decision makers in formulating strategies to counter direct Soviet intervention or intervention by proxies, and to respond to locally generated contingencies. Among other things Kevin N. Lewis calls for a firmer balance of resolve, William J. Taylor for the recruitment of U.S. proxies, and Michael E. Vlahos for a new set of interventionist strategies whilst educating the American public to accept the concept of American global leadership. One finally puts this book down with a sense that it is designed to offer policy prescriptions for the United States to regain the global dominance it enjoyed from the mid-1940s through the late 1960s. It would have been useful to have a concluding essay tying all the arguments together—and it would also have been useful to have a chapter assessing the potential of diplomatic solutions to regional conflicts (i.e.,

the *Contadora* process, a peace conference for the Middle East, and U.N. sponsored negotiations over Afghanistan).

PETER SHEARMAN

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Arms and Oil: U.S. Military Strategy and the Persian Gulf. By Thomas L. McNaugher. (Washington, D.C.: The Brookings Institution, 1985. Pp. xiii + 226. \$26.95, cloth; \$9.95, paper.)

The Persian Gulf rests at the core of the Middle East, the part of the Third World that is increasingly the new international heartland. Possessing over 60% of the world's proven reserves of oil, this region, sometimes referred to as the Petroleum Gulf, also carries enormous geostrategic significance. It is in the gulf where both superpowers stand in close confrontation, and where political turbulence is the order of the day. While the Soviet Union maintains an uneasy military occupation of Afghanistan only 300 miles from the gulf, the Iranians and Iraqis continue to fight for hegemony of the gulf itself in one of the most punishing and painful wars of the century. Meanwhile, traditional political systems such as those that mark Saudi Arabia, Oman, and the five gulf shaykhdoms struggle to survive against increasingly difficult odds. The United States has clearly indicated that it considers the gulf to be within its sphere of "vital interests," and has served notice that it will not hesitate to involve itself militarily if these interests are threatened by "any outside force."

Given this critically important situation, a flurry of books has recently appeared, most of them generated out of think tanks in Washington. These analyze in great detail the military dimensions of American policy toward the gulf states. The emphasis continues to be on military tactics, military strategies, and military scenarios of all kinds involving American muscle and movement. Consultants have written thousands of pages indicating how the United States can move its forces to the region with the greatest dispatch in the fastest possible time. Thus, most discussions of the politics of the gulf have focused on Rapid Deployment Joint Task Forces (RDJTFs), Central Commands (CENTCOMs), and Readiness Com-

mands (REDCOMs). Also, much American discourse on the gulf concerns itself with terms such as *prepositioning*, *tripwires*, *preemptive strikes*, *intratheatre lifts*, *air interdiction campaigns*, *hit-and-run tactics*, and *surging the force*. Important regional states such as Saudi Arabia are viewed in these same terms, witness the emphasis upon SANGs (the Saudi Arabia National Guard) and SNEPs (the Saudi Naval Expansion Program).

The book under review is no exception. A research associate at the Brookings Institution, Thomas McNaugher focuses his study upon the military dimensions of gulf politics. However, McNaugher successfully suppresses the urge to count military beans, and instead attempts to recognize the social and political environment in which the military must operate. His effort to do so is both a success and a failure. It is a success in its very recognition of the importance of understanding the socio-political environment within which any military strategy must be framed. Moreover, it provides some sober, informed, and sensitive conclusions—conclusions that need to be carefully considered by foreign-policy makers in Washington.

The author stresses the continuing importance of the gulf to American national interests. He recognizes the complexities of the social and political systems that compose the littoral states; discusses the subtle and intertwining network of relations that marks internal, interregional, and international politics—a network that can easily unravel; and understand that the precarious stability that marks the gulf must be fundamentally understood "from the inside out" (p. 91). It is this last contribution touching on policy recommendations that is the most significant.

McNaugher convincingly states that "the United States can charge hastily into this region only at considerable expense, perhaps even at its own peril" (p. 2). He concludes that American policy must be based on a cautious, low-key approach designed to work quietly in support of regional cooperative security efforts. In particular, this means "understated" support and encouragement for the Gulf Cooperation Council (GCC). Any heavy military or political presence will only hasten the collapse of the moderate, conservative systems now in place. In the end, American policy must be delicately made, flexibly arranged,

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and politically sensitive. Military muscle may rest in the background, but deeply ingrained social and political problems cannot be bombed into submission or blown away. This, for example, is the lesson that America could learn from Iran and Lebanon, that the Soviets might learn from Afghanistan, and that Israel still needs to learn in South Lebanon.

Despite these important and informed policy conclusions, this book falters when it comes to examining the dynamics of the internal social and political systems that compose the eight gulf states. The reader must then accept at face value the writer's general conclusions concerning the primary issue of political stability. There is no discussion of current leadership problems and the thorny issue of succession, and very little attention paid to such gathering forces as the Islamic revival and the increasingly disaffected middle classes in the region. If the gulf region is to be understood from the inside out, then, of course, it is essential for the writer to provide more inside analysis.

Finally, the author fails to draw any conclusions with respect to American policy concerning the emotional Arab-Israeli issue, an issue that bleeds over into the gulf. There is a very large Palestinian population residing in the gulf countries; Kuwait alone is the home of nearly half a million Palestinians. The close American association with Israeli policy is a major political problem in the gulf, yet there is no discussion of what American policy should be with respect to this fundamental issue. Also, after explicitly recognizing the fundamental long-term strategic importance of Iran, the author seems to support without question the American tilt to the Iraqi position with respect to the Iran-Iraq war. The population of Iran is twice that of all the other Gulf states put together, and its military forces and revolutionary ideology are perhaps the most powerful in the region. In its visible tilt to the Iraqi side (ironically joining the Soviet Union in so doing), is not the United States once again betting on a long-run losing regime? The author fails to discuss this issue seriously.

In the end, however, this book is a major contribution. Although he does not analyze them in any depth, McNaugher does understand the central significance of domestic social and political forces in the region. U.S. military strategy can only be understood within this complex context. The author seems to agree

then with the admonition of Christopher Van Hollen that the United States must not attempt to "engulf the Gulf." If this were to be attempted, it could not only suck American into a Middle Eastern military quagmire located in the backyard of the Soviet Union, but also could unleash forces of political violence that would quickly sweep away the political systems now in place in the traditional gulf states. This study soberly assesses military realities within the context of social and political systems under great stress. American foreign-policy makers would do well to read it with care.

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The Geography of Peace and War. Edited by David Pepper and Alan Jenkins. (New York: Basil Blackwell, 1985. Pp. 222. \$45.00, cloth; \$14.95, paper.)

Political Geography: Recent Advances and Future Directions. Edited by Peter Taylor and John House. (Totowa, NJ: Barnes and Noble Books, 1984. Pp. vi + 239. \$27.50.)

Although quite different in substance and intent, these two collections offer the reader a representative sampling of current research directions in political geography and politically relevant geography, respectively. The Taylor-House volume grew out of an international conference of political geographers held at Oxford in July 1983. It was prepared on a very tight schedule, so as to be available at the International Geographical Union (IGU) conference in Paris in August 1984—to its detriment. It contains numerous errors in typography and several in pagination, and in general suffers from a lack of editing. In contrast, the Pepper and Jenkins volume consists of solicited papers, all but one by geographers with special expertise in some aspect relevant to peace studies. It evinces all the trappings of simply splendid organization and editing.

Taylor in his introductory essay in *Political Geography: Recent Advances and Future Di-*

rections attempts to theorize about the issue of scale in political geography by asking why politics occurs at three different scales: international, national, and local (urban). His answer, drawn from the scope-based conflict model of Schattschneider and Wallerstein's world-systems theory, is as insightful as it is controversial. Taylor provides a strong theoretical rationale as to why relations between scales and the different political meanings of geographic scales form central themes in modern political geography.

The remaining chapters reflect the renewed interest and rapid growth in political geography in the 1980s. Few, however, come to grips very directly with theoretical issues in the politics of geographic scale. Instead, most contributors seek to convince the reader and the IGU of why particular research questions should command a central place on the future agenda of the field. Readers expecting delivery on the promise of Taylor's introductory essay will be disappointed. Falling short of its intended objective, this work suffers from two maladies often found among collections. It lacks any clear sense of direction and contains essays of uneven quality. Although disappointing, it is not fatally flawed, because several of the chapters are very good indeed.

Jurgen Ossenbruggé's essay reviews and critiques public choice and social structural theories of the local state from the perspective they bring to bear on the analysis of local conflict. He argues convincingly for a theoretical approach to local conflict and urban political geography that is concerned with space both as a scarce political-economic resource and as a social construct dialectically imbedded in the reproduction of social relations.

Written from a Third World perspective, Bertha Becker's essay provides a valuable and provocative political geographic interpretation of the crisis of the nation-state as a manifestation of both regional legitimation crises and the current world economic crisis. Her chapter and those of Taylor and Johnston come closest to following the book's central theme.

Taylor's and Ron Johnston's chapters attempt to relate electoral geography to theories of state set within a dynamic capitalist world economy. Taylor contends that in liberal democracies there are two geographies underlying elections; one produced by what he calls the *politics of power* and the other by the

politics of support. Basically, he calls for investigations in the changing relationships between the geographies of accumulation and legitimation. The degree to which these two geographies are congruent or contradictory can provide an alternative means of analyzing paradoxes in electoral geography. Unfortunately, Taylor fails to provide much in the way of historical analysis to support his reinterpretation. Also, his failure to recognize that the liberal democratic ideal of congruency between the two geographies is inherently unstable under capitalism undermines the analytical value of the framework. Johnston's essay is an interesting companion piece to Taylor's, in that he too analyzes the politics of accumulation/legitimation. His emphasis is on explaining the sporadic nature of elections in the Third World and the advocacy of PR electoral systems in Britain on the grounds of "fairness." Unlike Taylor, Johnston argues that in parts of the First World, and especially in Britain, electoral politics as currently practiced is not much better-suited to the promotion of accumulation than it is in the Third World. Together, these two essays clearly delineate one of the most important controversies in modern political geography: how to develop a theory of the state in which accumulation and legitimation are central, interacting processes rather than merely functional necessities. This debate has obvious relevance well beyond political geography.

Other chapters in the Taylor and House volume deserving mention are David Knight's critical analysis of the concept of self-determination and John O'Loughlin's ambitious attempt at integrating aspects of political geographic research with those in the field of quantitative international relations. Both of these essays contain excellent reviews, and should be of interest to specialists in nation building and international relations in political science. The remaining chapters are often interesting, but some are flawed by methodological weaknesses, some are unfocused literature reviews, and others appear to be pure flights of fancy or ungrounded exhortations aimed at fellow geographers.

The Geography of Peace and War consists of three parts. The first reviews processes and events that have led to the present situation of international tension, nuclear escalation, and cold war; the second examines in detail the

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spatial and human consequences of global nuclear war; the third attempts to identify ways in which further geographical research and teaching can contribute to the advancement of peace.

Part 1 contains the strongest contributions, with Herman van der Musten's chapter on the classification of postwar armed conflicts and O'Sullivan's on the geopolitics of the postwar world being particularly excellent. Together, these two chapters set both the tone and context for the remaining essays in this section: Tony Ives's analysis of the geography of arms dispersal, Alan Burnett's well-illustrated discussion of propaganda maps used by groups favoring and opposing nuclear deterrence, and Charles Anderson and Walter Isard's analysis of arms manufacture and regional economic dependence in the U.S.

Geographical analysis of a different kind characterizes part 2. First is Openshaw and Steadman's detailed spatial analysis of the expected death and casualty patterns that would result under possible nuclear attack scenarios for the United Kingdom. They show quite conclusively that the local geography is such that nuclear war of virtually any sort would be national suicide. Derek Elsom's chapter presents an up-to-date review and critique of the "nuclear winter" hypothesis, while Donald Zeigler questions the scientific basis of civil defense planning in the U.S. Zeigler suggests that nuclear relocation planning, the dominant civil defense strategy in both the U.S. and USSR, may actually fuel the arms race, because its success is predicated on the "home" country initiating the preemptive strike.

The final part of the volume is somewhat weaker and less coherent than the earlier sections. Most interesting is Frank Barnby's essay, which maintains that nuclear weapon-free zones and antisubmarine warfare sanctuaries are among the more implementable and appropriate "geographical" devices for averting nuclear war. Also useful is the chapter by Jenkins on geographical pedagogy and the peace movement.

Although *The Geography of Peace and War* is directed primarily to geographers, it should also find an appreciative audience in political science. It would be a very useful text in global studies and political science courses dealing with the arms race and nuclear disarmament. To my surprise, I found this a compelling

book; it deals creatively and intelligently with what is clearly one of the most pressing issues in the world today.

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Collective Clientelism: The Lomé Conventions and North-South Relations: By John Ravenhill. (New York: Columbia University Press, 1985. Pp. xxi + 389. \$35.00.)

Two competing myths cover the institutionalized relations between the European Communities (EEC and others) and the states of black Africa plus islands and enclaves in the Pacific and the Caribbean (ACP). One, heartily voiced by the EC Commission, is that the three Lomé conventions of 1975, 1980, and 1985 are a model for North-South relations in their skillful embodiment of both impartiality and redistributive economic justice. The other myth, expressed with theoretical coherence in the *dependencia* literature, is that Lomé institutionalizes both neocolonial exploitation and postcolonial neglect. A sound analytical study will not still these contending schools, which obviously have more at stake in the argument than their mere positions. It should, however, be welcomed by those observers and practitioners of North-South relations who are interested in a sound judgment on the difficult state of unequal relations and on ways to improve them.

Ravenhill's book is that study, and it will long stand as the authoritative work on the first two Lomé conventions and the basis for evaluating and improving performance under the third. The book has two major strengths: its clear conceptual framework, which provides both a sound basis for judging the Lomé relationships and a set of guidelines for prescribing corrections, and its wealth of knowledge and analysis about the workings of the relationship itself. Either would be a contribution: the combination makes the work exceptional.

Let us consider the second strength first. After an exposition of its conceptual framework, the book presents a history of the institutional evolution from the 1957 treaty of Rome, whose fourth part covered the Euro-

pean members' colonies, through the two Yaoundé conventions of association in the 1960s, to the negotiation of the first Lomé conventions were the provisions of export stabilization funds (STABEX) and of supports for EEC mineral imports (SYSMIN). Although the programs look like the commodity supports that exponents of the New International Economic Order (NIEO) have been demanding, Ravenhill thoroughly documents the limitations, use and misuse, and advantages of the two funds. The next chapter then presents a thorough analysis of the limitations imposed by the European Common Agricultural Policy (CAP)—rules of origin, sensitive products and safeguard clauses (threatened but never invoked)—on the promise of preferential access to the European market.

In the following chapter, there is an impressively comprehensive statistical study of the effect of the conventions of ACP-EEC trade, with breakdowns by ACP subgroup and comparisons with other relevant categories of developing countries, showing that the conventions did not improve the ACP trade standing, and if (as is not provable) they prevented it from further deterioration, that double negative compared badly with official promises and expectations. Finally, on trade, a chapter examines the performance of the convention countries in regard to three difficult products covered by separate protocols: sugar, bananas, and rum, where in the first two cases ACP interests were furthered by the arrangements. Another element in the promises of the treaty concerns the possibility of cooperation for industrial development. Ravenhill shows that expectations have not been met because of secondary preoccupations on the part of both the EC—fears of competition from new APC industries—and the APC—fears of infringement on sovereignty from EC conditionality. The penultimate chapter provides a many-factored analysis of the declining aid provisions of the conventions in relative terms, but also showing that Lomé aid was a supplement, not a substitute, for other aid programs. Ravenhill's careful scholarship confirms many conflicting judgments about the Lomé relationship, and shows that it cannot be summarized in any simple grade. That requires a more complex concept, which is the second strength of Ravenhill's work.

For the author, the notion of a relationship

held by its partners was that of collective clientelism, an exchange of complementary benefits between unequal groups of members. Weaker states would receive favored, protected status, preferential treatment, and additional aid; stronger states would receive assured sources of supply, political support, and favored if not formally preferential welcome. The inequality of the partners, as in the analysis of domestic clientelism, need not necessarily mean exploitative relations, since the parties exchange different items valued differently. Since the latter notion is also basic to bargaining literature, the relationship should fit well into institutionalized relations renegotiated at regular intervals.

Yet even in this light the relationship is not entirely successful, as the analytical chapters show. Although the parties were all probably better off with the conventions than without—the only real cost being the aid monies from the EC, the APC—there were no sacrifices on either side to produce new benefits for the two sides; nor was there any creation of new goods for the same purpose. As the Lomé conventions evolved, the two sides pulled away from their initial concept rather than grow into it. The Europeans increasingly have hidden behind their rising protectionism when the convention promises access, yet they seek to protect their sources of supply of traditional products when the convention promises development. The APC countries insist on the contractual equality of relations, yet refuse reciprocity or tradeoffs of interest to Europe. They seek above all to defend their sovereignty, not realizing that it covers economic inequality, not political equality. As a result, the relationship becomes one of aid, hence basically one of inequality, denying the promise and expectation of contractual equality.

The result, the book shows, is a dilemma. Errors abound on both sides when they—in quinquennial negotiations or daily operations—bargain out their relations. Oversold as a relation of equality, the convention falls victim to both dashed expectations and sovereign susceptibilities. Unfortunately, the clear analysis of the basic contradictions in the relationship is so thorough that it overwhelms any notion it might be corrected. Perhaps the best answer is found in such basics as more honesty in advertising—more openness and greater concern for the other party's interest in relations. The clarity of Ravenhill's analysis and

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the evenhandedness of his judgments help us see as deeply as possible into the complex EC-ACP relationship.

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The Militarization of Space: U.S. Policy, 1945-84. By Paul B. Stares. (Ithaca, NY: Cornell University Press, 1985. Pp. 352. \$25.00.)

President Reagan's March 1983 address on the Strategic Defense Initiative put the question of space militarization directly before the public, but as Paul Stares amply demonstrates, space militarization has been with us a long time. It existed as a concept before there was any Sputnik, and since then as an important element in a variety of U.S. and Soviet space programs. Stares reviews the major program initiatives taken by the U.S. as it sought to utilize space to enhance its national security in the postwar era. He surveys a vast array of technological, organizational, and diplomatic activities related to space militarization primarily by marshalling splinters of information from other scholarly writings and from published reports and U.S. government documents, including congressional testimony. He has also uncovered interesting material from first-person interviews and especially from various presidential libraries.

Stares discusses the many forms that space militarization and arms control have taken. However, after all the mass of detail, one hungers for the author to show how the various programs fit into some meaningful pattern, or at least where the major points of congruence or conflict are among all the assorted initiatives and programs.

Overall, though, Stares does a valuable job of assembling the heretofore dispersed and even publicly unknown elements of the story of U.S. space militarization, and does so in a clear style. This makes the book a good contribution to our understanding of what the U.S. has attempted and accomplished in terms of space militarization.

The book appears at a crucial time in the development of U.S. space policy. Although political scientists devoted much analytical attention to space policy during the early days of the space program, their interest waned con-

currently with that of the public. One consequence of this drop in social scientific analysis is that space policy has been allowed to evolve without benefit of the careful and constructive scrutiny of the political science community that has done so much to improve design and implementation in other areas of national security policy. This situation is becoming rectified, however, since President Reagan's "Star Wars" speech.

An important conclusion reached by the author is that the absence of space weaponry has not been due to some farsighted restraint on the part of either of the superpowers, but to technical constraints and perceived national interests that until recently had held back the use of space as a new battleground. This is now changing. Today the superpowers—especially the U.S.—are heavily reliant on space. Reconnaissance and communications satellites, as well as space-transiting ballistic missiles, have become standard, crucial lynchpins in their defense planning. These stable assets are now presenting an inviting target due to recent advances in technology that make it possible to contemplate an attack on them. The short-sighted temptation to seek unilateral advantage in these new technologies is proving irresistible.

To the extent that bureaucratic politics and national policy decision making are discussed by Stares, he suggests that far too often in the U.S. there is a tendency to think about space weapons development apart from the context of its long-term impact on the stability and utility of space operations.

Stares has done a thorough job of pulling together information on space militarization. He has also provided thoughtful analyses of the regrettable impact that accelerating space militarization is having on the prospects of arms control.

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China, Taiwan, and the Offshore Islands—Together with an Implication for Outer Mongolia and Sino-Soviet Relations. By Thomas E. Stolper. (Armonk, NY: M. E. Sharpe, 1985. Pp. xiii + 170. \$30.00.)

The China Question: Essays on Current Relations between Mainland China and Taiwan.
Edited by Yu San Wang. (New York: Praeger Publishers, 1985. Pp. xvi + 164. \$35.95.)

Over the last 15 years, Sino-American diplomacy has succeeded at transforming a once uniformly hostile relationship into an economic and, in some ways, even strategic partnership. However, the Taiwan question has persisted as a nagging reminder of the burdens of historic interests. When diplomatic relations between Washington and Beijing were established in 1979, disagreements over Taiwan were temporarily put on the back burner. Yet they keep emerging, and, as these two studies emphasize, they point up the inherent crisis potential of divided states.

Chinese leaders in the People's Republic of China and Taiwan agree on two things about the island: that it is part of China, and that efforts by outsiders to separate it from the mainland must be resisted. Fundamentally, that is what all three Taiwan Strait crises (1954-55, 1958, and 1962) have been about: the status of Taiwan. Thomas Stolper's impressive study of the first affair drives home the view of the People's Republic, from which it has never deviated, that any attempt to give independent status to Taiwan would be rejected, even to the point of using force. Professor Wang's volume is largely a defense and reassertion of Taipei's case to represent the real China. For Taipei, too—and therefore for the book's contributors—an independent Taiwan (or Formosa) is unthinkable.

Stolper's work is the more interesting of the two because it breaks new ground, both as a case study and as an analysis of Chinese irredentism. Meticulously researched and documented, it is the first full-length treatment of Chinese strategic diplomacy in the first confrontation regarding the offshore islands. He finds that since Taiwan, and not Quemoy or Matsu, was Beijing's primary concern, "the PRC did not wish either to capture the island[s] or to have the Nationalists withdraw [from them]" (p. 84). Beijing would behave similarly in 1958. In both instances it had legitimate fears of a U.S.-backed attack on the mainland that led it to make a preemptive strike against well-fortified Nationalist positions in the offshore island group. Capturing the islands was another, preeminently politi-

cal, matter. It would have facilitated an American claim of Taiwan's independence as an island separate from the others. Mao preferred that the offshore islands continue to be a "noose" around the Americans' neck.

That they no longer are is one of the important features of current U.S.-China relations. The diplomacy of the People's Republic in recent years has again shown that Taiwan is the prize. Various inducements for reunification have been communicated to Taipei. Although, as a matter of principle, Beijing reserves the right to use force to "liberate" Taiwan, it is now pledged to "peaceful reunification" as a "fundamental policy." Stolper cites only a few remote contingencies under which an attack by the People's Republic on Taiwan is conceivable (pp. 140-41). He goes on, however, to observe that the U.S. commitment to Taiwan's sovereignty under the 1979 Taiwan Relations Act lays the basis for a future crisis. So would a unilateral Nationalist withdrawal from Quemoy and Matsu. As always, U.S. policy is the decisive factor, and today, as in past years, it is riddled with contradictions—how else can a "one-China" policy that upholds the right to defend Taiwan be described?—that dismay Beijing and Taipei alike.

If there is a way out of this impasse, Wang's book does not offer one. It is useful in establishing that, even though Taiwan is losing international standing, it still acts very much like a state, and a prosperous one at that, with admirers on the mainland. Most of the authors are too busy pressing Taiwan's case—convinced, in the editor's words, that "the PRC's only course of action is to change the Communist system" (p. 45)—to consider alternatives to all-or-nothing solutions. The case for Taiwanese independence is not considered, for instance. Nor are the implications of Taiwan's serious human-rights problems addressed. The People's Republic's own ideas of Taiwan's future as a special administrative region are mentioned, but not given the serious discussion they deserve. We are left having to believe the Taiwan issue will be with us for many years to come.

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Visions of World Order: Between State Power and Human Justice. By Julius Stone. (Baltimore: The Johns Hopkins University Press; 1984. Pp. xxix. + 246. \$26.50.)

This book, Julius Stone's last before his death in the summer of 1985, has two principal aims. The first is to investigate the connection between international law and its "sociological substratum," an investigation that for Stone centers on current technological, economic, and military developments. The second aim is to consider the adequacy of international law as a means of realizing the demands of justice. The focus here is on international law as an instrument for the promotion of human rights and economic welfare. The work thus seeks to contribute both to the theory of international law and to the discussion of contemporary global problems.

Sociological inquiry, the author argues, is essential for the clarification and development of international law. The main task of a sociological jurisprudence of international law is to understand how the law is conditioned by its social environment, for these conditions define the limits within which a world order based on law is possible. One of the most important of these limiting conditions is the difficulty of communicating across religious, class, and national boundaries—a difficulty sometimes exacerbated rather than eased by developments like increased literacy or the spread of television. For example, better communication within a society may create obstacles to communication across national boundaries. According to Stone, the chief defect of most of the major theories of international law—Jenks's hoped-for "common law of mankind," McDougal's "policy-oriented jurisprudence," or Falk's theory of world order through the growth of "planetary consciousness"—is that they are insufficiently attentive to such sociological realities. By thus insisting on the overriding importance of the social environment of international law, the author continues the attack on legalism begun many years before in his *Legal Controls of International Conflict* and other books. The attack is directed here, however, against fellow soldiers in the sociological camp.

To these theoretical concerns is joined a practical interest in reforming international law. In the tradition of ethical jurisprudence,

Stone seeks to articulate a conception of justice to be realized through international law and on the basis of which the law is to be evaluated. Justice, according to the view developed here, has little to do with the traditional principles of the law of nations, which in the past required respect for state sovereignty, territorial boundaries, diplomatic immunities, treaties, and the laws of war. On the contrary, the author's focus is almost entirely on matters, such as individual rights and economic welfare, that had been largely outside the scope of international law down to the present century. What the reader will encounter, therefore, are the following distinctly contemporary arguments: first, that international law must secure respect for human rights, and therefore can no longer confine itself to the regulation of interstate relations; second, that it must concern itself, with the promotion of economic justice. Justice, moreover, requires that international law move as expeditiously as it can toward requiring the redistribution of economic benefits to all of humanity, and that this redistribution take place *through* and not merely *to* states.

Stone's ideas regarding the sociological and ethical bases of international law are familiar, and they conform to a large body of current opinion. This superficial plausibility is, however, no proof of their validity. The book is disappointing from the standpoint of theory, because it largely asserts rather than attempting to demonstrate its conclusions. The proposition that in order to understand international law one must study its sociological context seems plausible enough at first sight, but why limit our inquiry in this way? What about the economic context? The psychology of statesmen and diplomats? The historical antecedents of the present order? Does the sociology of international law subsume these other inquiries? Can any such inquiry really illuminate the character of legal order? In a work of theory one expects questions of this kind to be addressed systematically. Stone not only ignores them, but fails to defend his central claim regarding the unique validity of the sociological approach.

The discussion of justice is equally superficial and dogmatic. The author offers a number of propositions about the nature of justice and its relation to international legal order, but he does not attempt to defend them against even the most obvious objections.

Clearly this is unsatisfactory, given both the controversial character of the concept of justice and the novelty of Stone's own position in the history of discourse regarding justice in international society.

This is an earnest essay, but it contributes little to the present controversy over the nature and moral foundations of international legal order. Those who wish to follow Stone's work will find here a restatement of many of the themes that occupied him during a long career. Others should look elsewhere for significant discussion of the problems of international justice.

TERRY NARDIN

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Peacetime Unilateral Remedies: An Analysis of Countermeasures. By Elisabeth Zoller. (New York: Transnational Publishers, Inc., 1984. Pp. xviii + 189. \$35.00.)

This book deals with the important subject of the extent to which states can take the law into their own hands when their interests have been thwarted by other countries. Professor Zoller explores the subject of peacetime unilateral remedies in every legal way conceivably possible, determining which are acceptable and the conditions that make them possible. The book would be of greater interest to international relations specialists had it included case studies explaining why states will carry out illegal unilateral remedies knowing they are against the law.

The first part of the book carefully distinguishes between the countermeasures of retortion, reciprocity, suspension and termination of a treaty, and reprisals. On retortion, the author feels that most of the acts are done under domestic jurisdiction and thus are legal. She observes that the United States practices retortion a great deal, giving the examples of the Hickenlooper Amendment on compensation problems of nationalized overseas investment and human rights provisions in legis-

lative acts. In both cases aid to other nations could be denied. Professor Zoller mentions that the intention of retortion should be considered, and that an economic boycott could be unreasonable. The United States economic embargo on Nicaragua, which occurred after the book was written, would be an excellent case to include, since its legality was questioned by many countries in a United Nations Security Council debate on the issue.

The next countermeasure, reciprocity, is described exceptionally well, with excellent examples of the General Agreement on Tariffs and Trade (GATT) system used to show that as long as a state stays within equivalence the concept is legal. The next countermeasure, suspension of a treaty, is evaluated largely from the provisions of the Vienna Convention on Treaties. Finally, there is a long section on reprisals, covering their origins and the viewpoint that Article 2(4) of the United Nations Charter prohibiting the threat or use of force makes most of them illegal. This section on reprisals could have been made more pertinent to international relations if there had been an explanation of why so many states in the postwar period have used reprisals, regardless of their illegality. The Israel reprisals against the PLO could have been evaluated here.

The first part of the book concludes with a chapter on the specificity of countermeasures. Professor Zoller here is interested in getting at the purposes of unilateral remedies. She does this through a threefold classification scheme of reparation, coercion, and punishment. By describing what is involved in each countermeasure and the motives of states in selecting one or the other, the author is able to assess the legality of each. For reparation, the ideal situation would be a return to the *status quo ante*, but since this is never truly possible, monetary restitution is the most acceptable legal procedure. Coercion is defined as coercively affecting the future behavior of the other party in a nonviolent way, and is considered by the author to have a utilitarian purpose of law enforcement. Punishment, however, is considered to be illegal, since its purpose is the infliction of harm, the use of excessive force, the thirst for revenge, and the demonstration of superiority. It also lasts much longer than necessary.

The second part continues with a discussion of the legal framework of countermeasures. In

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one part the author argues that it is legally prohibitive to take countermeasures if they harm third states, although the author feels that it is worth determining if the act is done purposely or unintentionally. Here an account of Israel's reprisals against the PLO in Lebanon could have added to the analysis. In another interesting part, the author feels that there is no legal responsibility to negotiate before committing reprisals, since negotiations could be an odious trap set by the other side to allow the

dispute to drag on and on.

Professor Zoller concludes the book by making a strong plea for the acceptability of certain types of countermeasures, as long as all of the legal conditions described so carefully in the book are met. An excellent twelve-page bibliography of relevant books, articles, and court cases is included.

ROBERT D. TOMASEK

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POLITICAL ECONOMY

Economic Conditions and Electoral Outcomes: The United States and Western Europe. Edited by Heinz Eulau and Michael S. Lewis-Beck. (New York: Agathon Press, 1985. Pp. viii + 248. \$15.00, paper.)

Lewis-Beck organized the University of Iowa's Shambaugh Conference in March 1984, at which these papers were presented; Eulau edited them for *Political Behavior*, where they first appeared. The editors provide a well-rounded introductory essay, setting the stage for the nascent cross-national comparisons inhering in these reports. But do we really need another reader on this topic? Is the whole greater than the sum of its parts?

The volume succeeds admirably by illuminating two important questions: (1) How does the context of elections condition the economy's effect on the vote? (2) What causal process (i.e., what perceptions and decisions by individuals) produces the link? "Context" includes social, economic, or political factors that shape popular political responses to economic events. Social factors, for instance, include cultural beliefs in self-reliance vs. group or national solidarity; economic factors, the country's openness to international trade or monetary conditions beyond the control of national leaders; political factors, the many types of rules affecting the visibility of policy or the ability of citizens to hold governments

accountable—majority vs. coalition governments, large vs. small public sector, or "strong" party systems (cleavage lines are simple and ideologically clear, and virtually the whole population is mobilized) vs. "weak" ones. Assessing the impact of these influences requires cross-national comparisons, and these rely on having at hand the appropriate information about numerous countries.

Schneider provides an initial overview of the literature, noting some salient differences in research traditions and findings among the countries. Except for Germany, most Western European nations receive article-length treatment, with a review of research and a brief introduction to the author's latest statistical analysis of the economic sources of electoral outcomes. Whitley focuses on Britain's 1983 election, an appropriate research site, as the class-based party alignment was then at its weakest and the issue of economic policy under Thatcher presumably highly salient. Lafay's analysis centers on the French election of 1981, contrasting the covariation between economic conditions and executive popularity under Giscard with that under the Mitterand government's first two years. Miller and Listhaug summarize the voluminous literature on economic voting in the Scandinavian nations and present a perceptive and suggestive study of Norwegian voting. Bellucci's study of Italian elections since 1980 depicts a system in

which the strongly ideological nature of party competition makes short-term economic issues largely irrelevant. And Lancaster's study of Spain since the transition to democracy argues that unemployment has only recently emerged as an issue, both because of the moderation of the post-Franco left and because of the early "politics of consensus" on the primacy of building democratic legitimacy. Norpoth's study of U.S. presidential popularity from 1960 to 1980 belongs to this category. A tour de force of ARIMA time-series analysis, its results—neither Kennedy nor Johnson benefit from their management of the economy, while Ford does, according to the model—will convert few skeptics to the method. These studies map the effects of several important contextual variables: they should provide valuable raw material and strong stimulus to explicitly comparative research.

The task of explaining how the political behavior of individuals gives rise to observed aggregate covariation between the economy and elections is a difficult one. While survey research can clear up some of the causal ambiguity inherent in time-series analysis, its own imperfections can be narrowed only by learning how citizens translate objective economic experiences into subjective, politically meaningful understandings. Kiewiet and Rivers review the research on economic voting in the U.S. with a view to contrasting the two approaches, and they provide both a perceptive summary of current knowledge and several promising suggestions for future research. Much of the individual-level research has addressed the question of how economic self-interest guides the vote. Feldman ranges widely over this literature, judiciously evaluating its worth and drawing out (and testing) a number of critical hypotheses about the interplay between personal financial condition and national business conditions as alternative decision referents. Peffley continues this theme, considering several models of the reasoning process through which individuals attribute political significance to economic events. A brief essay by Toinet, suggesting future research directions, concludes the volume.

The editors set out to explain how economic conditions influence elections, but they apparently are not comfortable omitting the other side of the question—How do elections

influence economic policies?—and Alt's article compares British and American experience. From a sophisticated specification of the policy effects of party change within an economic model including international influences, Alt infers that party differences result in transitory, not sustained, shifts of the unemployment rate. Curiously enough, changes in party control cause distinct economic outcomes, but without corresponding differences in policy instrument settings, underlining the fact that the policy side of the question is at least as complex as the election side. Pfiffner has recently edited a comparable volume focusing on these issues.

It is no failing that this collection covers less than the whole field: it takes on a significant portion and treats relevant questions thoroughly, with subtlety and some originality. It is a failing that a host of typographical and proofreading errors mar the text, but with this exception, the volume is a fine investment for the growing number of upper division and graduate courses in political economy.

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Government Finance in Developing Countries. By Richard Goode. (Washington, D.C.: Brookings Institution, 1984. Pp. 334. \$31.95, cloth; \$13.95 papers.)

"Throughout the world," asserts Richard Goode, "many government declarations of intentions are never carried out. Other intentions are partly realized but at high cost" (p. 301). His book, *Government Finance in Developing Countries*, is addressed to the question of how this gap between promise and performance in developing countries may be narrowed through the adoption and carrying out of practical and appropriate financial policies. It aims to provide a comprehensive survey of current theories and practices in budgeting, expenditure analysis, taxation, borrowing, and financing government by money creation, and to examine these topics in relation to institutions and problems in developing countries. The result is a detailed handbook combining prescription and description in the light of the author's 30 years of experience with

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the International Monetary Fund and the Brookings Institution.

Goode's primary approach is that of the familiar public finance text, setting out and discussing accepted principles in the light of advantages and disadvantages. The particular value of his book is that he goes much further, relating those principles to conditions in developing countries, citing research and experience to illustrate their potential and limitations. Nor are the principles themselves immune to criticism—such textbook staples as public goods theory and the concept of Pareto efficiency, for example, are dismissed in a few words as having little practical bearing on the issues at hand, which are the feasibility and usefulness of principles translated into policies.

What, for example, are the prospects for an income tax? Well, although conditions according to the literature would seem to rule it out, there is nothing to stop you trying it and gradually extending its coverage, but don't rely on it as a major revenue source, and if you set beginning rates too low and top rates too high, you have only yourselves to blame, because the hazards of both are spelled out for you. What about a land tax? Or a consumption tax? What are the effects of export taxes? How about capital budgets? For these and a wide variety of financial policy questions Richard Goode carefully sets out the pros and cons, summarizes the research, comments (often critically) on current practices, provides references for further study, and gives cautious advice.

It is quite clear that there are no simple answers. Rather, the answers for each country have to be worked out through a maze of possibilities, economic constraints, political limitations, and value judgments. Criteria lie in effectiveness, (the degree to which intentions are realized) and efficiency (the extent to which they are attained at reasonable cost in scarce resources or sacrifice of other objectives).

This pragmatic approach has its limitations. Financial policies are discussed one at a time, with little attempt to relate them one to another. There is no attempt at a systematic conception of developing countries that goes beyond seeing their difficulties as simply one more example of world-wide fiscal strain. Nor does the author try to distinguish among developing countries in order to relate fiscal

policies to specific sets of circumstances. For example, a table of countries showing relative tax effort is presented, but no explanations or conclusions are given regarding why some countries exhibit lower tax effort than others. Generalized criticisms of current practices are often offered, but then the author hurries on to another topic, providing little or no insight into why things are as they are. Similarly, despite a promise at the outset to include political institutions and administrative capabilities in the analysis, these are really treated as just another set of constraints, and there is no systematic attempt to relate the nature of regimes to financial policies or practices.

Yet the book is still an invaluable practical reference compendium that should be essential reading for anyone concerned with policy making in developing countries. While it concludes that poverty is the major cause of policy ineffectiveness and that financial problems are likely to remain intractable, it emphasizes the degree of choice realistically open to policy makers in adapting the systems they inherit, and the practical possibilities for overcoming or compensating their handicaps. Some countries do much better than others. Why? If there is a single broad maxim generally applicable, it is to economize in the use of scarce resources, and there is much practical advice as to how this may be done. If the book seems unsatisfying because of its generalized approach, the Brookings Institution might well consider publishing a collection of case studies to supplement it and stimulate in-depth discussion of the issues it raises. Meanwhile Goode has provided an easily read and useful handbook for all concerned with financial policies, not just those in developing countries.

NAOMI CAIDEN

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The Politics of U.S. International Trade: Protection, Expansion and Escape. Stephanie Ann Lenway. (Marschfield, MA: Pitman Publishing, 1985. Pp. xii + 228. \$25.95.)

In this fifth work in the Pitman Series in Business and Public Policy, Stephanie Lenway compares four explanations of U.S. trade

policy outputs through a close examination of three highly visible cases. She concludes that only by way of continued U.S. participation in the institutional framework provided by the General Agreement on Tariffs and Trade (GATT) will U.S. national interest prevail over protectionist sectoral interests.

The author begins by identifying four possibly complementary explanations. First, the interest-group explanation portrays U.S. trade policy as the output of a Congress captured by various domestic business and labor interests. Second, a transactional explanation suggests that Congress will act autonomously by removing itself from a direct role in trade policy formulation and only acting as a final arbiter. Third, a state autonomy explanation would see the U.S. as attempting to improve its relative international power through policies to increase its exports as compared with its imports. The author finds each of these inadequate as direct explanations of the pragmatic liberalism consistently identified in U.S. trade policy.

Lenway argues for a fourth explanation, in which trade policy reflects a broad national interest in economic liberalism. Even though U.S. trade policy does aid special interests and maintains domestic social stability, the existence of international norms and rules, as embodied in the GATT, allows the state to resist domestic protectionist sentiment.

Through a careful study of the U.S. stance on the multifiber arrangements associated with the Tokyo Round multilateral trade negotiations, the U.S. response to the increase in Japanese automobile imports, and the U.S. position on the inclusion of the Japanese telecommunications market in the Tokyo Round negotiations on the Government Procurement Code, the author tries to find the links implied in each explanation. Lenway finds that, although none of the explanations failed completely and all are to a degree complementary, "all three cases were consistent in that the final decision involved adherence to the norms, rules, and procedures of the multilateral trade regime" (p. 205).

The literature review succinctly examines the domestic connections of U.S. trade policy, and though the cases are common to the literature, the mix of evidence from interviews, documents, and scholarly works provides a very detailed description of the process. More

importantly, the focus on the four explanations keeps the cases in perspective, permitting a fair degree of comparison in the analysis. The literature review and policy descriptions are well-styled, though occasionally redundant, making the work very readable. Given that path-breaking works on trade policy have laid the basis for some of the most influential models of American politics, this alone gives the work some appeal.

However, the cases most clearly demonstrate that the U.S. also used domestic pressures to carve exceptions from the international norm of liberalism, by emphasizing the strength of domestic protectionist demands being kept at bay (pp. 102, 126, 157). This implies that international restraints on U.S. policy were affected by domestic demands, which to some degree reverses the international constraints explanation. Furthermore, the author argues that the imposition of voluntary export restraints and the construction of a restrictively ordered market under the multifiber arrangement is consistent with the norm of liberalization, a very confusing interpretation of the norms of the international trade regime. Pragmatic liberalism may be standard behavior, but it is not the same as the international standard of liberal behavior.

Finally, though the author specifically avoids using the terms *hypothesis* and *theory*, the interest-group model, the state autonomy model, and the international constraints model are generalizable explanations for trade policy behavior, and a comparative perspective would be of even greater value. Nonetheless, this book is a useful complement to other, more comparative, materials on the domestic determinants of trade policy.

RICHARD T. CUPITT

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Game Theoretic Analysis of Voting in Committees. By Bezalel Peleg. (Cambridge: Cambridge University Press, 1984. Pp. 144. \$37.50, cloth; \$13.95, paper.)

Can a voting system be devised that is guaranteed to reveal voters' true preferences and to choose a desirable outcome? It has been proved that in general, the answer is *no*.

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Unless voters' preferences are assumed to be restricted (say, to single-peaked preferences in a unidimensional space), there is always the chance that some voters can gain by reporting a false preference ordering. This book investigates how serious this problem may be. It is largely a description of recently published research by Peleg, Moulin, and others.

Peleg's approach is to develop formal representations of voting games, and investigate whether these can be proved to be nonmanipulable. I think it is fair to say that the results are mixed, although much has been learned about specifics. For example, Peleg brings Arrow's impossibility theorem and the Gibbard-Satterthwaite manipulability theorem into his framework and relates them to his results. Peleg emphasizes "strong representations" of committees—social choices that accurately represent the expressed preferences and power of the committee members, and that assure that the members cannot manipulate their expression of preferences to make themselves better off. Most work on manipulability has looked for Nash equilibria, but Peleg investigates the harder and more important problem of whether any coalition of voters can make themselves better off by falsely stating their preferences.

"Strong representations" are available for committees with some distributions of preferences and numbers of alternatives, although the precise conditions are still being worked out. Implementing a strong representation can be done by a high-enough majority rule that depends on the number of voters and alternatives. (This result is much like Mueller's voting by veto). Peleg investigates whether agenda structures make implementation of strong representations easier, and comes to negative conclusions. This supports the Romer-Rosenthal and Shepsle-Weingast approach that sees agendas as a means of manipulating outcomes, not of assuring non-manipulable social choice.

Another formal book on this topic is Herve Moulin's *The Strategy of Social Choice* (Amsterdam: North-Holland, 1983). Both books are very difficult for readers without an understanding of the formalism of social choice theory. Moulin's book has a number of exercises that are worth working through, and as a textbook provides a broader coverage of the field and more intuition. Peleg concen-

trates on a narrower set of questions, and provides more proofs.

Some concerns I have with this work include its neglect of incentive schemes of the Clarke-Groves-Ledyard variety, which could reduce misrepresentation. By looking only at rank orderings, this approach neglects cardinal differences in utilities, which might be important in committee decision making. It also excludes binding agreements, side payments, and an external exchange value. These exclusions seem inappropriate for a positive theory based on cooperative solution concepts. Finally, the book's main results favor super-majority rules, which have high decision-making costs that are modeled in the public choice literature. These costs affect the optimal outcome and are inconsistent with the zero coalition costs Peleg assumes.

This book is written for, and will be useful to, social choice theorists interested in the analysis of voting rules.

KENNETH KOFORD

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Capitalism and Social Democracy. By Adam Przeworski. (New York: Cambridge University Press, 1985. Pp. viii + 269. \$39.50.)

This is the second book in a new series, *Studies in Marxism and Social Theory*, and it can be read as both a contribution to and a critique of the Marxist literature on class struggle and the state. The book is a reworking of several prior articles, some cowritten with John Sprague or Michael Wallerstein. The style ranges from historical narrative to formal modelling, including the use of graphs and regressions. The density of the argument is, happily, leavened by a wry humor in examples and footnotes.

Przeworski's points are both political and methodological. Politically, he argues that social democratic struggles are correctly viewed as being focused on material issues within capitalism rather than as stepping stones toward a socialist transformation. Unlike many Marxist critics of social democracy, he does not think this is necessarily a bad thing. It may be rational for workers and their

political parties to struggle for these reforms, rather than face a long period of sacrifices to get to some still-unclear noncapitalist society. Przeworski is not overly optimistic about what can be gained through social democracy, particularly in a period in which Keynesian policies for demand-side growth have little room to operate, but he sees social democracy as preferable to an unorganized alternative. While not unsympathetic to the separate project of a socialist transformation, he sees it as not immediately feasible.

Methodologically, Przeworski is concerned with developing a theory of politics and class behavior based on a formal rationality model. Thus, after his general overview of the history of social democratic parties and their relation to the broader socialist movement (chap. 1), and a history of the debate over class and its formation (chap. 2), he turns to modelling the material basis of class and party organization and strategy. Late in chapter 2 and in its appendix, he argues that not merely class consciousness and cohesion, but the very division of the population into classes, must be explained on a model of individual choices. He gives a provocative example of a woman who may, depending on a variety of factors and choices, end up as a housewife, a factory worker, or in a very small business. "Her objectives and her resources do not classify her as a worker," although he might add they clearly keep her out of the big bourgeoisie. "She decides to become a worker given her objectives and resources" (p. 95). Thus class boundaries as well as class strategy are to be explained. Class analysis becomes "a methodological postulate . . . that all conflicts that occur at any moment of history can be understood if and only if they are viewed as effects of and in turn having an effect upon class formation" (p. 80).

In chapter 3, Przeworski argues that once the socialist and labor movements had begun to organize workers as a class, and move toward class power in a political arena, the parties they created were faced with a choice between broadening their program to attract non-factory workers as voters or narrowing their program. This created tradeoffs. Too much broadening would cost worker support. The theoretical optimal degree of broadening, which might or might not be reached exactly, did not include possibilities of an immediate

drive for socialist transformation. Similarly, working within capitalism, there was an optimal degree of militancy to obtain the highest economic gains. Too much militancy would lead to economic crisis and would thus be as bad as too little. The result was to make class compromise a rational choice for both workers and their parties. (chaps. 4 and 5).

The final technical analysis, chapter 7, a critique of a paper by Roemer, discusses possible rational arguments for socialism, arguing that the case can better be made in terms of increased possibilities for social choice, rather than on purely economic grounds. On either side of this chapter, chapter 6 and a postscript draw political conclusions for the current period. With Keynesianism unable to satisfy both workers and capitalists, the tradeoffs are worse, but it is in the workers' interest to remain organized as a class, despite the difficulties. Socialism in the long run, and social democracy in the short, both appear as desirable, but they are entirely separate objectives.

Students of rationality models will find this book an interesting application. Marxists may object to this particular revisionism, and to the individual-rationality methodology itself, but they should still consider the arguments. There is no inherent incompatibility between materialism at the micro and macro levels, although specific models of their relationship may be too narrow. Certainly a reanalysis of the history of class compromise, a theme recently raised in studies of housing policy and workplace control, as well as of national politics, is due. Przeworski may be faulted on a number of points, particularly for taking the nation as a given unit of analysis when he is going beyond that for class as a unit, and for underplaying the economic forces that erode the payoffs to compromise. His arguments for the methodological attempt are, however, worthwhile.

MATTHEW EDEL

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How the West Grew Rich: The Economic Transformation of the Industrial World.

By Nathan Rosenberg and L. E. Birdzell, Jr.
(New York: Basic Books, Inc., 1986. Pp. xii + 353. \$19.95.)

A distinguished scholar of the history of technology and a legal specialist have teamed up to write still another book on the rise of the West. In *How the West Grew Rich*, Nathan Rosenberg and L. E. Birdzell explore the Middle Ages, the growth of trade, and the evolution of institutions favoring commerce. They look at the development of industry between 1750 and 1850, at technology and organization, at the links between science and wealth, and at organization in terms of the diversity of enterprise, and conclude with a chapter on implications and comparisons.

The central theme of the book is that the rise of the West can be explained by decentralization. That is, there was a gradual evolution of a set of institutions that permitted the economic sphere to become autonomous from the political-military sphere. The development of favorable institutions promoted a freedom to experiment in developing new forms of organization and of innovation. In addition, the development of a morality on the part of those who engaged in economic activity induced trust and contract fulfillment. The book is written in nontechnical language and contains no explicit theorizing; therefore, it is well suited for a general audience.

It is hard to quarrel with the author's argument. It is not a new one. The notion is that the evolving, competitive structure of western economies and the gradual emancipation of the economic sphere from the politics of mercantilism permitted and encouraged rapid economic growth. A long line of authors have in one way or another made this point, and most of them are the building blocks for *How the West Grew Rich*.

In the early part of their story, Rosenberg and Birdzell's emphasis is, of necessity, on the Netherlands and England, since other parts of Western Europe don't really fit it very well. France, Spain, and Portugal not only fail to develop and provide the institutional framework, but the latter two actually stagnate over the next centuries, and only catch up with the western world in very modern times (and then not completely). Here

we have a fundamental issue. In every country, rulers wanted to get their hands on the growing wealth and income of societies as they evolved. Indeed in France and in Spain they succeeded with consequences that were deleterious to the growth of their economies. In the Netherlands and England, on the other hand, the gradual development of an economy that had wide latitude to experiment and whose competitive conditions produced institutions favorable to growth, yielded a completely different pattern. Why was this so? Obviously the underlying game is between the polity and the economy. While the description of the two authors here is fine, it is not an explanation. They do refer to the growing mobility of capital as a deterrent to ruler confiscation, and cite the contrasting policies of the English and the French, but we are not provided with a body of political, political economic, or other theory about how institutions evolve that accounts for this diverse pattern between England and the Netherlands on the one hand and France and Spain on the other.

The same problem holds with respect to the authors' emphasis on the importance of the development of morality and a standard of ethics among merchants, which helped to enforce and lower the costs of contracting. What produced such development, and in what context? They flirt with the Protestant Revolution as an explanation (p. 114), but leave the issue in doubt. Clearly, the increasingly effective enforcement of contracts, whether as a result of morality or of enforceability, is an important factor in the development of markets that require "credible commitment."

A major strength of the book is its emphasis on the importance of institutions and its recognition of the interrelationship between favorable institutions and the development of experimentation and innovation. If it lacks any explicit theory, it is nevertheless rich in description of the institutions and, as is to be expected from one of the world's leading scholars in the history of technology, in discussion of technological change.

For the political scientist this book certainly provides an interesting story, but one that must, at least in part, be disappointing, since there is neither political theorizing nor any integrating of political explanation with that of the evolution of economies. An interesting few

pages in the last chapter on the political sphere (chap. 10) say some very sensible things about the separation in former times and nonseparation in modern times of politics and economics, but the story is not told in the context of serious analysis.

DOUGLASS C. NORTH

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Does Politics Matter? The Determinants of Public Policy. By L. J. Sharpe and K. Newton. (New York: Oxford University Press, 1984. Pp. xviii + 249. \$19.50.)

The topic of output studies evokes a strong sense of *deja vu*—one of those fashions in political science now relegated to the recesses of memory. Sharpe and Newton revisit this child of the early 1970s, demonstrating that premature abandonment of the new baby can be as unrewarding as its enthusiastic and ill-considered adoption.

As ever, the conventional approaches to explaining why policies differ are critically reviewed and found wanting. In their place—the core of the book—the authors demonstrate the superiority of the party and party system model and the unitary model. The former employs a wider range of measures of party effect on disaggregated expenditures (over time). Most important, the analysis distinguishes between party control and type of party system—e.g., non-party, two-party—demonstrating that both influence expenditures. The unitary model demonstrates the effect on expenditure of city type, or differences in economic base and function, and of the effect of a city's place in the urban hierarchy.

Perhaps the most appealing feature of the book is the common sense that pervades the analysis. It relies on relatively simple statistical techniques and insists on linking techniques to a well-defined theory and associated propositions. Rather than reaching for their computer terminal, the authors were prepared to inspect simple "league tables" of service expenditure and point out, for example, that local authorities on the coast spend less on sewerage. The case for the importance of politics is made with equal clarity and directness. Given Jim Sharpe and Ken Newton's high

standing among students of urban politics in Britain, one would expect no less. The problems do not lie in how the study is executed, but in its terms of reference: the theoretical and methodological rationale for output studies.

The academic controversy over the relative importance of economic and political variables is seen as a non-contest. The authors accord the political process "its true primordial status," arguing that "the political system is to public policy what an individual's perceptual set is to his cognition: it abstracts some features of the social and economic environment and gives them political salience" (p. 207). Additionally, "it also extends out into its own environment in order to shape, influence and alter it" (p. 208). As a result, key determinants of public policy "are almost certainly not susceptible to output analysis," most notable "those political factors that play a part in policy formation" (p. 216). Numbered among these factors are the role of the bureaucracy, the role of leadership cadres among the elected representatives, and a polity's decision-making tradition (p. 217). The authors call, therefore, for an alliance of methodologies. Output studies are to form the basis for future case studies of political factors. They will facilitate the precise design of case studies—for example, the selection of cases would be guided by the author's typology of local authorities—and "the inferences we have made from the macro data about the principal determinants of policy may be put to a searching and detailed test" (p. 216).

In thus rejecting the typical "input-black-box-output" paradigm in favor of the primordial political system, the authors undermine the theoretical and methodological rationale of the very approach they seek to rehabilitate: the environment is no longer constitutive, but constituted. To abandon the core model of output studies, to reverse the causal sequence, to redefine political variables and assert their primacy, and to discount the methodology of output studies for studying key independent variables is not to revise a theory or approach, but to abandon it. Although the authors enter a plea for cross-national output studies (which include subnational governments), they emphasize the contribution of case studies. There are no attempts to develop indicators of bureaucratic power. For example, it could be argued that the career

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mobility of officials was related to their influence, with mobile officers seeking to promote policies to further their future advancement. Consequently, it would seem that the authors also see output studies as a cul-de-sac, a tool for providing a systematic sketch map of the terrain to be traversed, but incapable of providing anything but clues for a more detailed analysis of policy. Politics does matter. Unfortunately, output studies do not. The way forward lies with different theories and methodologies.

Sharpe and Newton's research of times past demonstrates how output studies ought to have been conducted, but it does not demonstrate why this approach remains of value for future studies of policy making. Indeed, they are often their own best critics, with their clear appreciation of the limits to output studies. Thus, after criticizing previous attempts to operationalize political variables, they provide their own, more all-embracing indicators, which they then criticize by listing political variables not amenable to numeric expression. As ever, rehabilitation is a grand ideal applied to base material. It is to be hoped that recidivism rates do not increase. Yet more output studies, however well executed, would again direct energies and attention away from the analysis of policy, its formation, and consequences.

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Exploring the Global Economy. By Raymond Vernon. (Lanham, MD: United Press of America, 1985. Pp. vii + 225. \$19.95, cloth; \$8.75, paper.)

The Politics of East-West Trade. Edited by Gordon B. Smith. (Boulder, CO: Westview Press, 1984. Pp. ix + 267. \$32.50.)

Few economists have had the influence of Raymond Vernon on the way political scientists understand issues of trade and investment. In this compilation of Vernon's latest essays, he reexamines many of the key ideas associated with his earlier works. Addressed are four topics: international trade, international investment, the state-owned enterprise, and trade with the Soviet Union. In latter two

sections the work of the economist speaks directly to the set of political issues discussed by the authors in Gordon Smith's collection.

Most fundamentally, Vernon's essays indicate the extent to which the international economy has changed in the 15 years since he published *Sovereignty at Bay*. In 1971, Vernon questioned the viability of the nation-state system; in the 1980s, Vernon's concerns have shifted to the future of the liberal trade regime. Focusing on the growing number of transactions no longer conducted under the General Agreement on Tariffs and Trade (GATT), Vernon's essays examine their implications for states, for multinational enterprises (MNEs), and for the future of the international economic regime.

In an opening essay, Vernon summarizes his position: "The institutions of international trade commonly march to a music that was not written for the GATT script" (p. 19). He argues that a growing proportion of the world's trade is conducted on a basis considerably at variance with the principles that have characterized postwar trade among western nations. The existence of MNEs, state-owned corporations, countertrade arrangements, export-restraint agreements, and bilateral state trading with the Eastern bloc all undermine the rules, norms, and principles that characterize postwar economic arrangements. Domestic pressures for protectionism are not the only threats to the trade regime. In an essay on the future role technology will play in trade relations, Vernon hypothesizes that technical information and training could become a substitute for the shipment of goods. Technology itself, then, will decrease levels of world trade. As opposed to earlier analyses, in which he viewed information as a stimulant to trade, Vernon now sees a world in which information substitutes for trade and innovation places "greater stress on cost-reducing changes and lesser stress on the satisfaction of new wants" (p. 30).

Of the problems confronting the liberal economic order, multinational enterprises remain a key variable to the form future trade relations will take. The MNE, however, looks a lot less formidable in these essays. Although national governments have not been able to coordinate a uniform set of rights and norms for international companies, individual corporations face considerably more opposition

to foreign direct investment than they did in the 1970s. Vernon argues that the convergence of an "obsolescent bargain" and reemerging nationalism fueled governments to reassert their authority over these corporations.

One alternative pursued by nation-states has been to enter the market as corporate owners. Turning to the rise of state-owned corporations, Vernon examines the international implications of these companies. "The substantial growth of state-owned enterprises might inhibit the expansion of MNEs" (p. 110), he argues. More probably, however, the two will strive for mutual adaptation. In replacing the MNE, states face a number of constraints. As domestic champions, they will have a propensity to use domestic inputs and strive for export earnings, even if this is not the optimal economic strategy. Since these companies cannot invest abroad to rationalize the costs of production, they tend to be less flexible and will not produce at economies of scale. State ownership also brings a myriad of management problems: "Governments have great difficulty in defining their purpose in a coherent, internally consistent manner" (p. 151). Within any polity, there will be rival perspectives on the purposes of corporate ownership. Certainly, no state manager has settled for merely dealing with market imperfections. Nations always introduce some set of mixed policies that serve domestic needs: "The state-owned enterprise epitomizes a dilemma that has always existed . . . in a democratic state" (p. 160). Shall it be workers', managers' or ministers' interests that are served by the state when it turns to production? Politics maintain differing balances among these forces that "reflect the values of the country concerned" (pp. 161-62). Vernon concludes, however, that all meet similar "compelling demands that derive from operating in oligopolistic markets" (p. 162).

State ownership among the Eastern nations has led to another set of issues. Vernon sees the current pattern in East-West trade as a threat to the trade regime. Vernon looks to the potential use of commodity power to upset the economic system. He argues the Soviets could do three things: they could use their power to upset the international economy; they could use power to influence the individual behavior of nations, or they could attempt to capture the lion's share of economic gains from or-

dinary international trade. Vernon concludes that the first is improbable. The Soviets are dominant sellers only in chromite, manganese, and platinum; they are not dominant buyers of any commodity. Even so, Vernon argues that they may be able to influence inflationary and deflationary movements out of proportion to the size of the USSR in the world market. This is not probable, however, because he sees "no such subtlety of purpose" evident in Soviet behavior. The second and third options are more feasible. The Soviets can and probably have affected the behavior of the countries in the Council for Mutual Economic Assistance (CMEA). This, however, is not a threat to other trading partners. What is more evident to Vernon is that the Soviets gain asymmetrically from trade. Why? Vernon argues that since the level and composition of trade is determined by the East—that is, they only trade if there are gains from trade—trade with the West, where private gains may not always equal social gains, leads to a Soviet advantage.

In an essay written with Marshall Goldman, Vernon moves away from issues of market control and turns to prescriptions for American policy. Here, Vernon's work and Smith's work converge. Vernon organizes his discussion of American policy around three basic questions. He asks, "How will restrictions on the export of strategic goods and technologies affect the behavior and performance of the Soviet Union and COMECON [same as CMEA] . . . are there common policies with which (Western) countries can be expected to agree . . . and what economic difference will the presence or absence of restrictions have on the U.S. economy?" (p. 184). Vernon's responses are straightforward. The attempt to restrain trade neither helps curtail the Soviet's war-making capacity nor works as an effective signaling device for American displeasures with Soviet behavior. Vernon argues that restraints may actually be counterproductive; they may serve to stimulate the Soviets to find effective substitutes. Given the history of American policies, Vernon is pessimistic about finding a common ground for cooperation among the Western nations concerning trade to the East. Rather, he argues that current policies will serve to weaken the GATT regime: "Under existing institutions and rules of the game, increased East-West trade will strain the trading relations among the

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Western states and will distribute the economic benefits of trade disproportionately to the East" (p. 199). Thus, increased trade under current conditions in which the U.S. attempts unilaterally to mandate restrictions will have a negative impact on the U.S. For Vernon, the alternative is straightforward. "For those who are concerned with maintaining an effective set of economic relations toward the USSR, U.S. efforts to strengthen an open trading system for the world offer the most feasible area for action." (p. 194).

The more empirical studies found in Smith's volume agree, for the most part, with Vernon. *The Politics of East-West Trade* is a compilation of essays by American and European scholars, government officials, and businessmen. Three themes tie these essays together. First, a number of authors address the issue of the degree to which the Soviet economy is dependent on imports to the West. Second, the book looks at the impact of such dependence on the Soviet Union. Third, authors address the reciprocal affects of America's policy on East-West trade.

What is the extent of political leverage the West could garner through trade policy? Five authors examine case studies oriented toward answering this question. John Martens begins with an attempt to model the flow of imports into the Soviet Union. He discovers that the composition of exports to communist countries looks essentially like the general pattern of world-wide exports. Whether because of export controls or because of Soviet initiative, Martens concludes that trade with communist countries has not overly emphasized high technology products. In one of the best essays in the volume, Elizabeth Goldstein traces the impact of technology transfers on a sector in which she expected technology transfer to have a positive influence. In her case study, she finds that the ferrous metals industry suffered low productivity growth and no improvement in quality and yield, even though there were imports of advanced foreign technology. She argues that the combination of a seller's market and a systemic bias against innovation in the Soviet Union leads to disincentives among managers to improve performance. Again, there seems to be neither dependence on nor the ability to diffuse foreign technology.

Three other case studies corroborate Gold-

stein's findings. Jan Vanous argues that Soviet fear of dependence on the West has a major impact on policy. Even though the Soviets have a substantial disadvantage in grain, relative to energy trade, they refuse to specialize. Dependence based on comparative advantage is politically unacceptable to Soviet leaders. S. E. Goodman, in a study of the computer industry, finds numerous problems with the integration of foreign technology. Goodman however, argues that many foreign companies have had similar problems with technology transfer. The Soviets benefit most from technology that is open; that is, from technical and scientific reports that are part of the public domain and not subject to government control. Further showing the lack of Eastern dependence on Western products, Thane Gustafson examines how the Soviets compensated for the American embargo on products for the gas pipeline. He found that the Soviets were able to build their own compressors through a policy of explicit political and resource targetting. He argues that leadership will pay any price to defend their autonomy. In short, Western embargoes lead neither to political leverage nor to the curtailment of essential production. Embargoes do, however, exact a high cost from the Soviet economy. As Smith sums up, "there are tangible limits on the ability of the U.S. to use trade as leverage to alter Soviet policies."

A set of authors argues that there may be tangible costs to the U.S. in restricting trade. In an interesting article on the domestic effects of economic sanctions, H. Stephen Gardner argues that the major cost of restrictions is in the distribution of income. Income is redistributed away from affected sectors. In three other categories he sees the effect of sanctions to be mixed. It is unclear whether there is a substantial balance of payments loss; due to "musical chairs" trade patterns and a decline in exchange rates, the industries he studies had exports more than offset by opportunities in new markets. Similarly, Gardner found that offsetting market forces results in no real losses in employment nor in the allocation of resources due to economic sanctions. Garner does not believe that there is no effect from export controls. Rather he sees the U.S. as "shooting ourselves in the foot" (p. 194). Luckily, there is adjustment. In separate articles, Lothar Griessbach, William McIlvaine,

and Gary Bertsch and John McIntyre argue for the destructive influence of embargoes. In general, the politicization of market transactions leads to a mistrust of American corporations and to distrust of the U.S. among members of the GATT regime.

The lone dissenter arguing for the virtues of trade restraints was Donald Goldstein. Articulating the position of the Department of Defense, Goldstein argues that, "Soviet economic gains from international trade go disproportionately toward military investment that threatens the West" (p. 160). Disagreeing with the case study analysts who saw little benefit to the Russian economy from technology transfer, Goldstein sees the potential of foreign technology undermining America's military position. In essence, Goldstein fears that we are "selling the Russians the rope with which they will hang us." In contrast, other analysts argue that not selling to the East will

lead to the same negative outcome.

Smith concludes with an analysis of the future of trade with the East. Trade will be limited by political uncertainties, mutual concern over trade dependence, and limitations on the East's ability to raise hard currency. On the other hand, Smith argues that trade and technology flows are important assets in American foreign policy, and can serve as an instrument to the furthering of our interests. Trade, however, will further America's interests only if used as an inducement, rather than a threat. Echoing Vernon, the control of trade through measures such as export controls and sanctions will become more difficult with time, and can only weaken the liberal trade regime, which both agree is the more critical vehicle for America's national interest.

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ARTICLES

AN EVOLUTIONARY APPROACH TO NORMS

ROBERT AXELROD
University of Michigan

Norms provide a powerful mechanism for regulating conflict in groups, even when there are more than two people and no central authority. This paper investigates the emergence and stability of behavioral norms in the context of a game played by people of limited rationality. The dynamics of this new norms game are analyzed with a computer simulation based upon the evolutionary principle that strategies shown to be relatively effective will be used more in the future than less effective strategies. The results show the conditions under which norms can evolve and prove stable. One interesting possibility is the employment of metanorms, the willingness to punish someone who did not enforce a norm. Many historical examples of domestic and international norms are used to illustrate the wide variety of mechanisms that can support norms, including metanorms, dominance, internalization, deterrence, social proof, membership in groups, law, and reputation.

An established norm can have tremendous power. This is illustrated by a historical instance of the norm of dueling. In 1804 Aaron Burr challenged Alexander Hamilton to a duel. Hamilton sat down the night before the duel was to take place and wrote down his thoughts. He gave five reasons against accepting the duel: his principles were against shedding blood in a private combat forbidden by law; he had a wife and children; he felt a sense of obligation toward his creditors; he bore no ill against Colonel Burr; and he would hazard much and could gain little. Moreover, he was reluctant to set a bad example by accepting a duel. Yet he did accept, because "the ability to be useful, whether in resisting mischief or effecting good, in those crises of our public affairs which seem likely to happen, would probably be inseparable from a conformity with public prejudice in this particular" (Truman, 1884, pp. 345-48). In other words, the prospect of sanctions imposed by the general public in

support of dueling caused Hamilton to risk, and ultimately to lose, his life—a powerful norm indeed, and yet one that has all but disappeared today after centuries of power over life and death.

Today, norms still govern much of our political and social lives. In politics, civil rights and civil liberties are as much protected by informal norms for what is acceptable as they are by the powers of the formal legal system. Leadership is itself subject to the power of norms, as Nixon learned when he violated political norms in trying to cover up Watergate. The operation of Congress is shaped by many norms, including those governing reciprocity (Matthews, 1960) and apprenticeship (Krehbiel, 1985). Across many nations, tolerance of opposition is a fragile norm that has great impact on whether a democracy can survive in a given country (Almond and Verba, 1963; Dahl, 1966). In international political economy, norms are essential for the understanding of the operations of many

functional domains such as banking, oil, and foreign aid (Axelrod and Keohane, 1985; Keohane, 1984; Krasner, 1983). Even in the domain of power politics, norms have virtually wiped out colonialism, inhibited the use of chemical warfare, and retarded the spread of nuclear weapons.

Not only are norms important for many central issues in political science, but they are vital to the other social sciences as well. Sociology seeks to understand how different societies work, and clearly norms are important in these processes (e.g., Opp, 1979, 1983). Anthropology frequently deals with the unique features of various peoples by describing in great detail their practices and values, as in the case of feuding (e.g., Black-Michaud, 1975). Psychologists are concerned with how people influence each other and the manner in which an individual becomes socialized into a community (e.g., Darley and Batson, 1973; Sherif, 1936). Economists are becoming interested in the origin and operation of norms as they have come to realize that markets involve a great deal of behavior based on standards that no one individual can determine alone (e.g., Furubotn and Pejovich, 1974; Schotter, 1981).

Large numbers of individuals and even nations often display a great degree of coordinated behavior that serves to regulate conflict. When this coordinated behavior takes place without the intervention of a central authority to police the behavior, we tend to attribute the coordinated behavior and the resulting regulation of conflict to the existence of norms. To make this appeal to norms a useful explanation, we need a good theory of norms. Such a theory should help explain three things: how norms arise, how norms are maintained, and how one norm displaces another.

One of the most important features of norms is that the standing of a norm can change in a surprisingly short time. For

example, after many centuries of colonialism, the intolerance of colonial dependence took hold in the relatively short period of just two decades after World War II. Before and after such a transition, the state of affairs seems very stable and perhaps even permanent. For this reason, awareness of a given norm is most intense precisely when it is being challenged. Examples of norms being challenged today include the right to smoke in public without asking permission, the use of gender-laden language, and the prohibition against the use of chemical warfare. Some of these challenges will succeed in establishing new norms, and some will fail altogether. Thus, what is needed is a theory that accounts not only for the norms existing at any point in time, but also for how norms change over time. To clarify these processes, one must first be clear about exactly what is being discussed.

In this next section the evolutionary approach to be used in this paper is explained. Following this, the results of computer simulations of the evolution of norms are presented. The computer simulations are then extended to include a specific mechanism for the enforcement of norms, called *metanorms*. After these formal models are investigated, a wide variety of processes that might help to sustain norms are discussed, along with suggestions about how they too can be modeled. The question of the origin and content of norms is considered, and finally, a summary and conclusion presents the findings of this paper in the broad context of social and political change.

The Evolutionary Approach

Norms have been defined in various ways in the different literatures and even within the same literature. The three most common types of definitions are based upon expectations, values, and behavior.

That these different definitions are used for the same concept reflects how expectations, values, and behavior are often closely linked. Definitions based upon expectations or values are favored by those who study norms as they exist in a given social setting. Such definitions are convenient because interviews can elicit the beliefs and values of the participants, whereas systematically observing their actual behavior is more difficult. Because for many purposes the most important thing is actual behavior, a behavioral definition will be used in this study.

DEFINITION. *A norm exists in a given social setting to the extent that individuals usually act in a certain way and are often punished when seen not to be acting in this way.*

This definition makes the existence of a norm a matter of degree, rather than an all or nothing proposition, which allows one to speak of the growth or decay of a norm. According to this definition, the extent to which a given type of action is a norm depends on just how often the action is taken and just how often someone is punished for not taking it.

To investigate the growth and decay of norms, I have formulated a norms game in which players can choose to defect and to punish those they have seen defecting. The goal of the investigation is to see when cooperation based upon emerging norms will develop. Ultimately, the purpose is to learn what conditions favor the development of norms so that cooperation can be promoted where it might not otherwise exist or be secure.

To see what rational actors would do in a particular setting, a game theory approach can be used. Game theory assumes the players are fully rational and choose the strategy that gives the highest expected utility over time, given their expectations about what the other players will do. Recent work by economists has shown great sophistication in dealing with

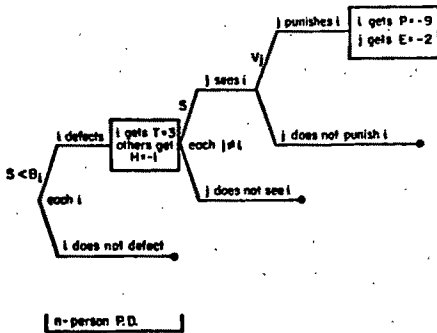
problems of defining credible threats and of showing the consequences of requiring actors' expectations about each other to be consistent with the experience that will be generated by the resulting actions (Abreu, Pearce, and Stacchetti, 1985; Friedman, 1971; Kreps and Wilson, 1982; Selten, 1975).

While deductions about what fully rational actors will do are valuable for their own sake, empirical examples of changing norms suggest that real people are more likely to use trial and error behavior than detailed calculations based on accurate beliefs about the future. Therefore, I have chosen not to study the dynamics of norms using an approach that depends on the assumption of rationality.

Instead, I use an evolutionary approach. This approach is based on the principle that what works well for a player is more likely to be used again while what turns out poorly is more likely to be discarded (Axelrod, 1984). As in game theory, the players use their strategies with each other to achieve a payoff based upon their own choice and the choices of others. In an evolutionary approach, however, there is no need to assume a rational calculation to identify the best strategy. Instead, the analysis of what is chosen at any specific time is based upon an operationalization of the idea that effective strategies are more likely to be retained than ineffective strategies. Moreover, the evolutionary approach allows the introduction of new strategies as occasional random mutations of old strategies.

The evolutionary principle itself can be thought of as the consequence of any one of three different mechanisms. It could be that the more effective individuals are more likely to survive and reproduce. This is true in biological systems and in some economic and political systems. A second interpretation is that the players learn by trial and error, keeping effective strategies and altering ones that turn out

Figure 1. Norms Game



poorly. A third interpretation, and the one most congenial to the study of norms, is that the players observe each other, and those with poor performance tend to imitate the strategies of those they see doing better. In any case, there is no need to assume that the individual is rational and understands the full strategic implications of the situation.

The evolutionary approach is inherently probabilistic and involves nonlinear effects. For these reasons, it is often impossible to use deductive mathematics to determine the consequences of a given model. Fortunately, computer simulation techniques (e.g., Cyert and March, 1963) provide a rigorous alternative to deductive mathematics. Moreover, simulation can reveal the dynamics of a process, as well as the equilibrium points. By simulating the choices of each member of a population of players and by seeing how the players' strategies change over time, the unfolding of a given evolutionary

process can be analyzed to determine its overall implications.

The Norms Game

The norms game is described in Figure 1. It begins when an individual (i) has an opportunity to defect, say by cheating on an exam. This opportunity is accompanied by a known chance of being observed. The chance of being observed, or *seen*, is called S . If S is .5, each of the other players has an even chance of observing a defection if it takes place. If player i does defect, he or she gets a payoff of T (the *temptation* for defecting) equal to 3, and each of the others are *hurt* (H) slightly, getting a payoff of H equal to -1. If the player does not defect, no one gets anything.

So far the game is similar to an n -person Prisoner's Dilemma (see, e.g., G. Hardin, 1968; R. Hardin, 1982; Schelling, 1978). The new feature comes in the next step. If player i does defect, some of the other players may see the defection, and those who do may choose to punish the defector. If the defector is *punished* (P) the payoff is a very painful $P = -9$, but because the act of punishment is typically somewhat costly, the punisher has to pay an *enforcement cost* (E) equal to -2.

The strategy of a player thus has two dimensions. The first dimension of player i 's strategy is *boldness* (B_i), which determines when the player will defect. The player will defect whenever the chance of being seen by someone is less than the player's boldness, which is to say, whenever $S < B_i$. The second dimension of a player's strategy is *vengefulness* (V_j), which is the probability that the player will punish someone who is defecting. The greater the player's vengefulness, the more likely he or she will be to punish someone who is spotted defecting.

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Table 1. Example of Payoffs in the Norms Game Attained by a Player With Boldness Equal to 2/7 and Vengefulness Equal to 4/7

Event	Payoff per Event	Number of Events	Payoff
Defection	$T = 3$	1	3
Punishment	$P = -9$	1	-9
Hurt by others	$H = -1$	36	-36
Enforcement cost	$E = -2$	9	-18
Score			-60

Simulation of the Norms Game

The simulation of the norms game determines how the players' strategies evolve over time. The two dimensions of a strategy, boldness and vengefulness, are each allowed to take one of eight levels, from 0/7 to 7/7. Because the representation of eight levels requires three binary bits, the representation of a player's strategy requires a total of six bits, three for boldness and three for vengefulness.

The simulation itself proceeds in five steps, as follows:

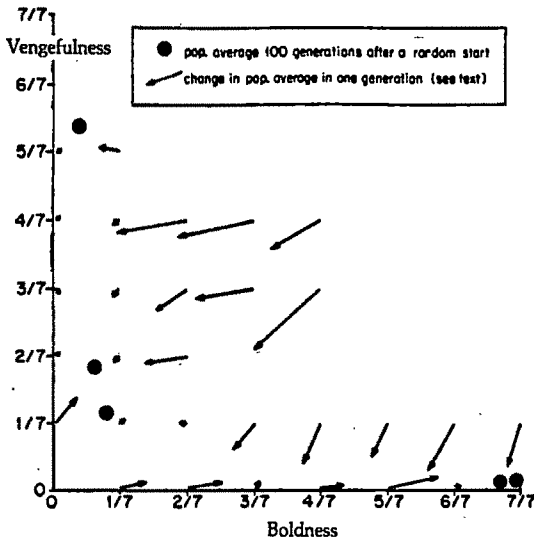
(1) The strategies for the initial population of 20 players are chosen at random from the set of all possible strategies.

(2) The score of each player is determined from the player's own choices and the choices of the other players. Each individual gets four opportunities to defect. For each of these opportunities, the chance of being seen, S , is drawn from a uniform distribution between 0 and 1. To see how the scores are attained, let us focus on an arbitrary player in the initial population of one of the runs, who will be called Lee. Lee has a boldness level of 2/7 and vengefulness level of 4/7. The total payoff Lee achieved was the result of four different kinds of events, as shown in Table 1. Lee defected only once because only one of the four opportunities had a chance of being seen that was less than Lee's boldness of 2/7. This defection gave a temptation payoff of $T = 3$ points.

Unfortunately for Lee, one of the other players observed the defection and chose to punish it, leading to a loss for Lee of $P = -9$ points. In addition the other players defected a total of 36 times, each hurting Lee $H = -1$ point. Finally, Lee observed who was responsible for about half of these defections and chose to punish each of them with a probability determined by his vengefulness of 4/7. This led to a punishment of 9 of the defections at an enforcement cost of $E = -2$ each, for a further loss of 18 points. The net result of these four types of events was a total score of -60 for Lee.

(3) When the scores of all the players are determined, individuals whose strategies were relatively successful are selected to have more offspring.¹ The method is to give an average individual one offspring and to give two offspring to an individual who is one standard deviation more effective than the average. An individual who is one standard deviation below the population average will not have his or her strategy reproduced at all. For convenience, the number of offspring is adjusted to maintain a constant population of 20. A final step is the introduction of some mutation so that new strategies can arise and be tested. This is done by allowing a 1% chance that each bit of an individual's new strategy will be altered. This mutation rate gives a little more than one mutation per generation in the entire population.

Figure 2. Norms Game Dynamics



(4) Steps 2 and 3 are repeated for 100 generations to determine how the population evolves.

(5) Steps 1 to 4 are repeated to give five complete runs of the simulation.

The results of the five runs are shown in Figure 2. The five circles indicate the average boldness and vengefulness of a population after 100 generations. Three completely different outcomes appear possible. In one of the runs, there was a moderate level of vengefulness and almost no boldness, indicating the partial establishment of a norm against defection. On two other runs there was little boldness and little vengefulness, and on the remaining two runs, there was a great deal of boldness and almost no vengefulness—the very opposite of a norm against defection. What could be happening?

The way the strategies actually evolve over time is revealed by the change that takes place in a single generation in a population's average boldness and vengefulness. To calculate this, the data are used from all 100 generations of all five runs, giving 500 populations. The populations with similar average boldness and

vengefulness are then grouped together, and their average boldness and vengefulness one generation later is measured. The results are indicated by the arrows in Figure 2.

Now the various outcomes begin to fit into a common pattern. All five of the runs begin near the middle of the field, with average boldness and vengefulness levels near one-half. The first thing to happen is a dramatic fall in the boldness level. The reason for the decline is that when there is enough vengefulness in the population, it is very costly to be bold. Once the boldness level falls, the main trend is a lowering of vengefulness. The reason for this is that to be vengeful and punish an observed defection requires paying an enforcement cost without any direct return to the individual. Finally, once the vengefulness level has fallen nearly to zero, the players can be bold with impunity. This results in an increase in boldness, destroying whatever restraint was established in the first stage of the process—a sad but stable state in this norms game.

This result raises the question of just what it takes to get a norm established. Because the problem is that no one has any incentive to punish a defection, the next section explores one of the mechanisms that provides an incentive to be vengeful.

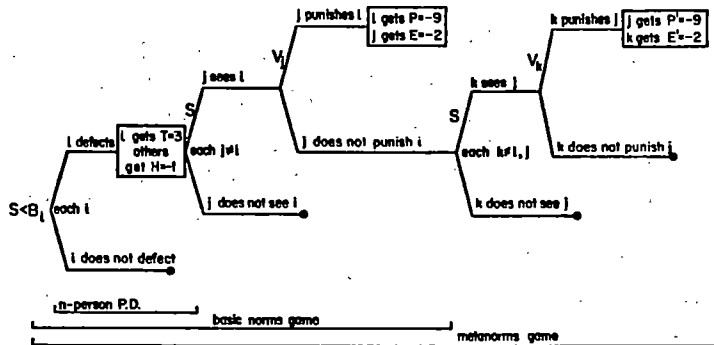
Metanorms

A little-lamented norm of once great strength was the practice of lynching to enforce white rule in the South. A particularly illuminating episode took place in Texas in 1930 after a black man was arrested for attacking a white woman. The mob was impatient, so they burned down the courthouse to kill the prisoner within. A witness said,

I heard a man right behind me remark of the fire, "Now ain't that a shame?" No sooner had the

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Figure 3. Metanorms Game



Key:

- i, j, k individuals
- S probability of a defection's being seen by any given individual
- B_i boldness of *i*
- V_j vengefulness of *j*
- T temptation to defect
- H hurt suffered by others
- P cost of being punished
- E punisher's enforcement cost
- P' cost of being punished for not punishing a defection
- E' cost of punishing someone for not punishing a defection

words left his mouth than someone knocked him down with a pop bottle. He was hit in the mouth and had several teeth broken. (Cantril, 1941, p. 101)

This is one way to enforce a norm: punish those who do not support it. In other words, be vengeful, not only against the violators of the norm, but also against anyone who refuses to punish the defectors. This amounts to establishing a norm that one must punish those who do not punish a defection. This is what I will call a *metanorm*.

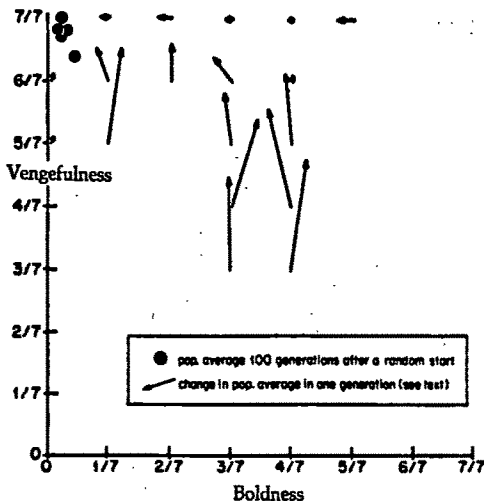
Metanorms are widely used in the systems of denunciation in communist societies. When the authorities accuse someone of doing something wrong, others are called upon to denounce the accused. Not to join in this form of punishment is itself taken as a defection against the group

(Bronfenbrenner, 1970; Meyers and Bradbury, 1968).

As another example, when the Soviet Union supported the suppression of the Solidarity movement in Poland, the United States asked its allies to stop supplying components to the Soviet Union for its new gas pipeline. The allies, not wanting to pay the enforcement cost of this punishment, refused. The United States government then undertook the metapunishment of imposing sanctions on foreign companies that defied the sales ban (*New York Times*, January 5 and June 19, 1982).

The formulation of a metanorms game can help in the exploration of the effectiveness of this mechanism. Figure 3 shows how the metanorms game is based upon an extension of the norms game. If

Figure 4. Metanorms Game Dynamics



someone defects, and Lee sees but does not punish that defection, then the other players have a chance to see and punish Lee. The model makes the critical assumption that a player's vengefulness against nonpunishment is the same as the player's vengefulness against an original defection.² The validity of this assumption will be addressed later, but first let us see what affect it has on the evolution of the process.

A set of five runs was conducted with the metanorms game, each done as before with a population of 20 players and a duration of 100 generations. The results are shown in Figure 4. They are unambiguous. In all five runs a norm against defection was established. The dynamics are clear. The amount of vengefulness quickly increased to very high levels, and this in turn drove down boldness. The logic is also clear. At first there was a moderate amount of vengefulness in the population. This meant that a player had a strong incentive to be vengeful, namely, to escape punishment for not punishing an observed defection. Moreover, when each of the players is vengeful out of self-protection, it does not pay for anyone to

be bold. Thus the entire system is self-policing, and the norm becomes well established.

This result is dependent, however, on the population's starting with a sufficiently high level of vengefulness. Otherwise the norm still collapses. Thus, while the norms game collapses no matter what the initial conditions are, the metanorms game can prevent defections if the initial conditions are favorable enough.

Mechanisms to Support Norms

The simulations of the norms game and the metanorms game have allowed the exploration of some of the important processes in the dynamics of norms. The simulation of the norms game shows that relying on individuals to punish defections may not be enough to maintain a norm. Therefore, the question to be considered now is, What mechanisms can serve to support a norm that is only partially established? The evolutionary approach helps to develop a list of such processes, and in some cases, suggests specific methods for modeling the process by which a norm can be supported.

Metanorms

As the computer simulations show, the existence of a metanorm can be an effective way to get a norm started and to protect it once it is established. By linking vengefulness against nonpunishers with vengefulness against defectors, the metanorm provides a mechanism by which the norm against defection becomes self-policing. The trick, of course, is to link the two kinds of vengefulness. Without this link, the system could unravel. An individual might reduce the metavengeance level while still being vengeful and then later stop being vengeful when others stopped being metavengeful.

The examples cited earlier suggest that

people may well punish those who do not help to enforce a valued norm. The model suggests norms can be supported if people tend to have correlated degrees of vengefulness or anger against someone who violates a particular norm and someone who tolerates such a violation. What the evolutionary approach has done is raise the possibility that metanorms are a mechanism that can help support norms, thus suggesting the interesting empirical question of whether the two types of vengefulness are indeed correlated. My guess is that there is such a correlation. The types of defection we are most angry about are likely to be the ones whose toleration also makes us angry. As of now, however, the possibility of metanorms remains speculative.

Dominance

Another mechanism for supporting a norm is the dominance of one group over another. For example, it is no coincidence that in the South, whites lynched blacks, but blacks did not lynch whites. The whites had two basic advantages: greater economic and political power, and greater numbers.

Simulation of the effects of power and numbers can be readily done with slight extensions of the basic model to allow for the existence of two different groups. The competition between two groups can be modeled by assuming that the defections of a player only hurt the members of the other group and are therefore only punished by members of the other group. Similarly, in the metanorms version of the model, punishments for not punishing a defector would only occur within a group, as illustrated by the pop bottle used by one white against another in the lynching example discussed above. Moreover, in determining strategies for the next generation, the strategies of two groups would be allowed to adapt separately so that whites learn from whites and blacks learn from blacks.

The two advantages of the whites are modeled separately. Their greater economic or political power is reflected in their lessened cost of being punished by a black. This was done by letting $P = -3$ for whites while retaining $P = -9$ for blacks. The greater numbers of whites are reflected directly in the relative size of the two populations, giving the whites a greater chance to observe and punish a black defection than vice versa. This was done by letting the population be 20 whites and 10 blacks.

Analysis of runs based upon these conditions shows that resistance to punishment and increased size can help a group, but only if there are metanorms. Without metanorms, even members of the stronger group tend to be free riders, with no private incentive to bear enforcement costs. This in turn leads to low vengefulness and high boldness in both groups. When metanorms are added, it becomes relatively easier for the strong group to keep the weak group from being bold, while it is not so easy for the weak group to keep the strong one from defecting.

Another form of potential strength is illustrated by the case of a major power interacting with many smaller nations. For example, the U.S. may not only be in a favorable position on a given bilateral interaction but also may have many more bilateral interactions than others. Thus, its behavior has a greater impact on the development of norms than would the behavior of a minor power. When Libya wanted to modify the international norm of the twelve-mile limit of territorial waters to include the entire Gulf of Sidra, the United States fleet deliberately sailed into the Gulf and shot down two Libyan planes sent up to try to change the norm. Clearly the U.S. was not only stronger but had incentives to enforce the old norm based upon its naval interests in other parts of the world.

While the process of frequent interactions by a single strong player has not

yet been simulated, it is plausible that such a process would help to establish a norm against defection because the central player would have a greater unilateral incentive to be vengeful against defections.

Norms can also be promoted by the interests of a few major actors, such as the U.S. and the Soviet Union's both working to retard the proliferation of nuclear weapons. Their actions need not be coordinated in detail as long as together they are important enough to others to enforce a norm of the major actors' choice. The logic is somewhat analogous to Olson's "privileged group" in a collective action problem (Olson, 1965, pp. 48-50).

Internalization

Norms frequently become internalized (Scott, 1971). This means that violating an established norm is psychologically painful even if the direct material benefits are positive. This is frequently observed in laboratory experiments where subjects are more equitable than they have to be and explain their behavior by saying things like "you have to live with yourself." In terms of the norms game, this type of internalization means that the temptation to defect, T , is negative rather than positive. If everyone internalizes a given norm this strongly, there is no incentive to defect and the norm remains stable. Obviously families and societies work very hard to internalize a wide variety of norms, especially in the impressionable young. They do so with greater or lesser success depending on many factors, including the degree to which the individual identifies with the group and the degree to which the norm and its sponsors are seen as legitimate.³

Clearly, it is rare for everyone in a group to have a norm so strongly internalized that for each the temptation to defect is actually negative. An interesting question for future modeling is, How

many people have to internalize a norm in order for it to remain stable?

The logic of the norms game suggests that lowering the temptation to defect might not be enough. After all, even if most people did not defect, if no one had an incentive to punish the remaining defectors, the norm could still collapse. This point suggests that we look for internalization, not only in the reduced incentive to defect, but also in an increased incentive to punish someone else who does defect.

An increased incentive to punish, through internalization or by some other means, would lead some people to feel a gain from punishing a defector. For them, the payoff from enforcement, E , would actually be positive. Such people are often known as self-righteous busy bodies and often are not very well liked by those who enjoy a defection now and then. Given enough people who enjoy enforcing the norm, the question of its maintenance then becomes whether the chance is high or low that the defection will be seen.

Deterrence

In the norms game and the metanorms game the players do not look ahead. Instead they try a particular strategy, see how it does, compare their payoff with the payoff of others, and switch strategies if they are doing relatively poorly. While trial and error is a sensible way of modeling players of very limited rationality, it does not capture the idea that players may have a great enough understanding of the situation to do some forward-looking calculations as well as backward-looking comparisons with others. In particular, a person may realize that even if punishing a defection is costly now, it might have long-term gains by discouraging other defections later.

A good example is the strong U.S. response to New Zealand's refusal in February 1985 to allow a U.S. destroyer into

Auckland harbor without assurances that it did not carry nuclear weapons. The U.S. government presumably did not care very much about nuclear access to New Zealand ports, but it did care a great deal about deterring the spread of a new norm of "nuclear allergy" among its many allies in other parts of the world (Arkin and Fieldhouse, 1985).

Social Proof

An important principle from social psychology is "social proof," which applies especially to what people decide is correct behavior. As Cialdini (1984, p. 117) explains,

we view a behavior as more correct in a given situation to the degree that we see others performing it. Whether the question is what to do with an empty popcorn box in a movie theater, how fast to drive on a certain stretch of highway, or how to eat chicken at a dinner party, the actions of those around us will be important in defining the answer.

The actions of those around us serve several functions. First, they provide information about the boldness levels of others, and indirectly about the vengefulness of the population. Hence, we can infer something about whether it pays for us to be bold or not. Second, the actions of others might contain clues about what is the best course of action even if there is no vengefulness. For example, people may be driving slowly on a certain stretch of highway, not because there is a speed trap there, but because the road is poorly paved just ahead. Either way, the actions of others can provide information about how the population has been adapting to a particular environment. If we are new to that environment, this is valuable information about what our own behavior should be (Asch, 1951, 1956; Sherif, 1936). The actions of others provide information about what is proper for us, even if we do not know the reasons. Finally, in many cases, by conforming to the actions

of those around us, we fulfill a psychological need to be part of a group.

Our propensity to act on the principle of social proof is a major mechanism in the support of norms. The current model of norms already has a form of this mechanism built in: when a relatively unsuccessful individual seeks a new strategy, that strategy is selected from those being used by the rest of the population. This is a form of social proof, refined by giving weight to the more successful strategies being employed in the population.

In cases where other people differ in important ways, the principle of social proof tends to apply to those who are most like us. This too is easy to build into simulations with more than one group. In the simulation of blacks and whites, the blacks look only to other blacks when selecting a new strategy, and the whites look only to other whites. This makes good sense because a strategy that is very successful for a white might be disastrous if employed by a black.

Membership

Another mechanism for the support of norms is voluntary membership in a group working together for a common end.⁴ Contracts, treaties, alliances, and memberships in social groups all carry with them some power to impose obligations upon individuals. The power of the membership works in three ways. First, it directly affects the individual's utility function, making a defection less attractive because to defect against a voluntarily accepted commitment would tend to lower one's self-esteem. Second, group membership allows like-minded people to interact with each other, and this self-selection tends to make it much easier for the members to enforce the norm implicit in the agreement to form or join a group. Finally, the very agreement to form a group helps define what is expected of the participants, thereby clarifying when a

defection occurs and when a punishment is called for.

One might suppose it would be easy for a bold individual to join and then exploit a group that had gathered together in the expectation of mutual compliance. Actually, this does not usually happen, in part because the factors just outlined tend to isolate a defector and make it relatively easy for the others to be vengeful—especially with the help of metanorms. Another factor is that, according to recent experimental evidence, cooperators are more likely to stay in a group than are defectors (Orbell, Schwarz-Shea, and Simmons, 1984). This happens because cooperators have a stronger ethical or group-regarding impulse than defectors, a factor that led them to cooperate in the first place.

The metanorms game can be expanded to include the choice of whether to join a group or not.⁵ In general, the value to a person of joining a group would depend on how many others joined. Each player would make this choice at the start of the game. Then the interactions concerning defections and punishments would occur as before, with the interactions limited to those who had actually joined. As an example, an alliance for collective security would include a group of nations that had joined for this common purpose. Once a nation had joined, a defection would consist of not supporting the alliance in some collective security task. A defection would hurt the other members of the alliance, and some of them might choose to punish the defector; they might also choose to punish someone who did not punish the defector. Typically, the larger the number of nations joining the group, the greater the benefits of cooperation would be for its members.

In the political sphere, voluntary membership taking the form of a social contract has been a powerful image for the support of democratic forms of governance. In effect, a mythical agreement is

used to give legitimacy to a very real set of laws and institutions.

Law

Norms often precede laws but are then supported, maintained, and extended by laws. For example, social norms about smoking in public are now changing. As more and more people turn vengeful against someone who lights up in a confined space, fewer and fewer smokers are so bold as to do so without asking permission. As this norm becomes firmer, there is growing support to formalize it through the promulgation of laws defining where smoking is and is not permitted.⁶

A law supports a norm in several ways. The most obvious is that it supplements private enforcement mechanisms with the strength of the state. Because enforcement can be expensive for the individual, this can be a tremendous asset. In effect, under the law the collective goods problem of enforcement is avoided because selective incentives are given to specialized individuals (inspectors, police, judges, etc.) to find and punish violations.

The law also has a substantial power of its own, quite apart from whether it is or can be enforced. Many people are likely to take seriously the idea that a specific act is mandated by the law, whether it is a requirement to use seat belts or an income tax on capital gains. However, we all know this respect for the law has its limits, and we suspect that many people do not pay all the tax they should. Even when enforcement is possible and is attempted, the strength of the law is limited. In most cases, the law can only work as a supplement (and not a replacement) for informal enforcement of the norm. The failure of Prohibition is a classic example of an attempt to enforce a norm without sufficient social support.

In addition to enforcement and respect, a third advantage of the law is clarity. The law tends to define obligations much

more clearly than does an informal norm. A social norm might say that a landlord should provide safe housing for tenants, but a housing code is more likely to define safety in terms of fire escapes. Over the domain covered by the law, the norm might become quite clear. However, this clarity is gained at the expense of suggesting that conformity with the law is the limit of one's social obligations.

Modeling the power and operation of the law is beyond the scope of this project. However, it should still be emphasized that often law is the formalization of what has already attained strength as a social or political norm. An important example is civil liberties, the very foundation of a democratic system. There are laws and constitutional provisions in support of civil liberties such as freedom of speech, but the legal system can only protect free speech if there is substantial support for it among a population willing to tolerate dissent and willing to protect those who exercise it.

In short, social norms and laws are often mutually supporting. This is true because social norms can become formalized into laws and because laws provide external validation of norms. They are also mutually supporting because they have complementary strengths and weaknesses. Social norms are often best at preventing numerous small defections where the cost of enforcement is low. Laws, on the other hand, often function best to prevent rare but large defections because substantial resources are available for enforcement.

Reputation

An important, and often dominant, reason to respect a norm is that violating it would provide a signal about the type of person you are. For example, if there is a norm dictating that people should dress formally for dinner, and you don't, then

others might make some quite general inferences about you.

The importance of dressing formally when the occasion requires is not just that others will punish you for violating the norm (say, by giving you a disapproving look) but also that they will infer things about you and then act in ways you wish they wouldn't. This is an example of the signaling principle: a violation of a norm is not only a bit of behavior having a pay-off for the defector and for others; it is also a signal that contains information about the future behavior of the defector in a wide variety of situations.⁷

There are several important implications of the signaling principle for the origin and durability of a norm. A norm is likely to originate in a type of behavior that signals things about individuals that will lead others to reward them. For example, if a certain accent signals good breeding, then others may give better treatment to those who speak that way. Once this happens, more people are likely to try to speak that way. Eventually, people might be punished (e.g., despised) for not having the right accent. Thus, what starts out as a signal about one person's background can become a norm for all.⁸

The signaling principle helps explain how an "is" becomes an "ought." As more and more people use the signal to gain information about others, more and more people will adopt the behavior that leads to being treated well. Gradually the signal will change from indicating a rare person to indicating a common person. On the other hand, the absence of the signal, which originally carried little information, will come to carry substantial information when the signal becomes common. When almost everyone behaves in conformity with a signal, those who don't stand out. These people can now be regarded as violators of a norm—and dealt with accordingly.

Note that there is an important distinction between a convention, which has no

direct payoffs one way or the other (such as wearing a tie for men), and a cooperative act, the violation of which leads to injury to others (e.g., queuing for service). A type of behavior with no direct payoffs can become a norm once it develops some signaling value, as is the case when fashion leaders adopt a new style (Veblen, 1899). Once this happens, a violator of this style will be looked down upon. Thus the style will become a norm; individuals will usually follow the style, and those who do not will likely be punished.

The Origin and Content of Norms

Eight mechanisms have now been identified that can serve to support a norm that is already at least partially established. What, however, are the characteristics of the behaviors that arise and then become more and more established as norms? Or to put it another way, just what is the content of behavior that might later turn into a norm?

The answer depends on what types of behavior can appear and spread in a population even when only a few people initially exhibit the behavior. This, in turn, depends on what kind of behavior is likely to be rewarded and punished for its own sake, independently of whether or not it is common behavior.

Two of the supporting mechanisms already considered can serve in this initial role: dominance and reputation. Dominance can work because if only a few very powerful actors want to promote a certain pattern of behavior, their punishments alone can often be sufficient to establish it, even if the others are not vengeful against defections. The implications for the substance of norms are obvious: it is easier to get a norm started if it serves the interests of the powerful few.

In fact, many norms obeyed and even enforced by almost everyone actually serve the powerful. This can happen in

forms disguised as equalitarian or in forms that are blatantly hierarchical. An apparently equalitarian norm is that the rich and the poor are equally prohibited from sleeping under bridges at night. A blatantly hierarchical norm is that soldiers shall obey their officers. Both forms are "norms of partiality," to use the term of Ullman-Margalit (1977).

To say that the powerful can start a norm suggests a great deal about the potential substance of such norms. Once started, the strong support the norms because the norms support the strong.

Dominance is not the only mechanism capable of starting a norm. Reputation can do so as well. Consider, for example, the idea of keeping one's promise. In a hypothetical society in which few people kept their promises, you would be happy to deal with someone who did. You would find it in your narrow self-interest to continue dealing with such a person, and this in turn would be rewarding to the promise-keeper. Conversely, you would try to avoid deals with those you knew did not keep their promises. You would, in effect, be vengeful against defectors without having to pay an enforcement cost. Indeed, your enforcement would simply be the result of your acting in your own interests, based upon the reputations of others and your calculation about what was good for yourself.

International regimes depend on just such reputational mechanisms to get norms started (Keohane, 1984). In such cases, countries can be very deliberate about what promises they make and which ones they want to keep when the stakes are high (Axelrod, 1979). Reputational effects can also be based upon the limited rationality of trial and error learning. If a person associates another's response to a particular act (say a refusal to continue dealing as a reaction to the breaking of a promise), then the violator can learn not to break promises.

This learning approach suggests the

importance of being able to link the behavior with the response. Behaviors will be easier to establish as norms if the optimal response of others is prompt and rewarding. Failing a prompt response, learning can also take place if the delayed punishment is explicitly cited as a response to the earlier defection.

Summary and Conclusion

To study the development of norms, the strategic situation has been modeled as an n -person game. In the basic norms game, everyone has two types of choice: the choice to cooperate or defect, which affects everyone, and the choice of whether or not to punish a specific person seen defecting. A player's strategy is described in terms of how these choices will be made. A strategy consists of two parameters: boldness (the largest chance of being seen that will lead to a choice of defection) and vengefulness (the probability of punishing someone observed defecting). To the extent that players are vengeful, but not very bold, a norm can be said to have been established.

To study the dynamics of the process, an evolutionary approach was employed. In this approach, the initial strategies are chosen at random, and the population of players is given opportunities to defect and to punish the defections they observe. The evolutionary approach dictates that strategies proving relatively effective are more likely to be employed in the future while less effective strategies are dropped. Moreover, strategies undergo some random mutation so that new ones are always being introduced into the population.

The computer simulation of this process revealed an interesting dynamic in the norms game. At first, boldness levels fell dramatically due to the vengefulness in the population. Then, gradually, the amount of vengefulness also fell because there was no direct incentive to pay the

enforcement cost of punishing a defection. Once vengeance became rare, the average level of boldness rose again, and the norm completely collapsed. Moreover, the collapse was a stable outcome.

This result led to a search for mechanisms that could sustain a partially established norm. One possibility is the metanorm: the treatment of nonpunishment as if it were another form of defection; that is, a player will be vengeful against someone who observed a defection but did not punish it. Simulation of the evolution of strategies in this metanorms game demonstrated that players had a strong incentive to increase their vengefulness lest they be punished by others, and this in turn led to a decline of boldness. Thus, metanorms can promote and sustain cooperation in a population.

Other mechanisms for the support of norms are also important. These include dominance, internalization, deterrence, social proof, membership, law, and reputation. In some cases, the resulting norms are hierarchical rather than equalitarian, and the cooperation exhibited is coerced rather than freely offered. A good example is the norm of black deference in the old South.

Dominance processes have been simulated by subdividing the population and letting one segment be relatively resistant to the effects of punishment by members of the other segment. Internalization can be investigated by studying the effects of making defection costly rather than rewarding for some of the defectors and by making punishment a pleasure rather than a cost for some of the observers of a defection. A more drastic change in the modeling procedures would be necessary to study some of the other mechanisms in question.

Norms are important in society and, not surprisingly, have been given a great deal of attention in the social sciences, including sociology, anthropology, political science, psychology, and economics.

While descriptions of actual norms abound, investigations of the reasons for people to obey or violate a given norm have been much less common. Even among the strategic approaches to norms, relatively little attention has been devoted to understanding the dynamics of norms: how they can get started, how a partial norm can be sustained and become well established, and how one norm can displace another. An evolutionary approach is helpful in studying these dynamics because it can help show how strategies change over time as a function of their relative success in an ever-changing environment of other players who are also changing their own strategies with experience.

A major goal of investigating how cooperative norms in societal settings have been established is a better understanding of how to promote cooperative norms in international settings. This is not as utopian as it might seem because international norms against slavery and colonialism are already strong while international norms are partly effective against racial discrimination, chemical warfare, and the proliferation of nuclear weapons. Because norms sometimes become established surprisingly quickly, there may be some useful cooperative norms that could be hurried along with relatively modest interventions.

Notes

I owe a great deal to Stephanie Forrest, my research assistant, and to those who helped me think about norms: Michael Cohen, Jeffrey Coleman, John Ferejohn, Morris Fiorina, Robert Gilpin, Donald Herzog, John Holland, Melanie Manion, Ann McGuire, Robert Keohane, Robert McCalla, Amy Saldinger, Lynn Sanders, Kim Scheppele, Andrew Sobel, Charles Stein, Laura Stoker, and David Yoon. I am also pleased to thank those who helped support various aspects of this work: the Harry Frank Guggenheim Foundation, the National Science Foundation, the Sloan Foundation, and the Michigan Memorial Phoenix Project.

1. The procedure used is inspired by the genetic

algorithm of computer scientist John Holland (1975, 1980).

2. For convenience, it is also assumed that the chance of being seen not punishing is the same as the chance of the original defection being seen. The payoff for metapunishment is $P' = -9$, and the metaenforcement cost is $E' = -2$.

3. Marx goes as far as to say that social norms are merely reflections of the interests of the ruling class, and the other classes are socialized into accepting these norms under "false consciousness."

4. I thank David Yoon and Lynn Sanders for pointing this out to me.

5. I thank David Yoon for formulating this variant of the metanorms game and the application to alliances that follows.

6. The same process of formalizing norms applies to private laws and regulations, as in the case of a business that issues an internal rule about who is responsible for making coffee.

7. For the theory of signaling, see Spence (1974). For a theory of how customs can be sustained by reputations, see Akerlof (1980).

8. Signals can also help to differentiate groups and thereby maintain group boundaries and cohesiveness.

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REASON AND WAR

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A new specification of the expected utility model of international conflict places expected utilities within a polar coordinate system; treats third-party choices in a manner more consistent with classical forms; estimates the expected utilities derived from not challenging existing policies; more fully represents the expected costs of conflict; and normalizes expected utilities regardless of system size. By assuming that the probability of escalation of a dispute increases monotonically with leaders' expectations of gain, we derive continuous functions for the probabilities of war, intervention, violence, and peace. The revised theory significantly improves our ability to discriminate between violent and nonviolent disputes and between violent disputes that escalated to warfare and those that did not in Europe between 1816 and 1970.

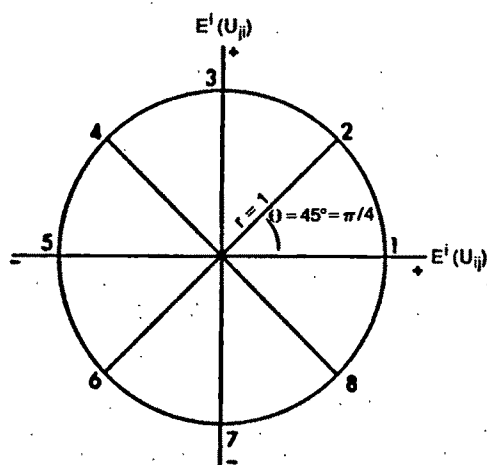
In this essay, we set out a general theory of the escalation of conflict and test portions of that theory on a broad data set. Because the subject is so large, this paper represents only a small portion of a much more comprehensive undertaking. Additional parts of the theory (and additional tests) are described in Lalman (1985) and in forthcoming papers by Bueno de Mesquita and Lalman.

Assume that a senior foreign policy leader in each country formulates and implements national policy as it relates to decisions to escalate international disputes or to negotiate for a resolution of them. Assume that such key leaders are expected utility maximizers who estimate the welfare that will devolve on their state in the course of such disputes. Assume also that they estimate the utility expected by their opponent in the dispute. Assume that the higher one's estimate of an adver-

sary's expected utility, the greater the costs the adversary is presumed to be prepared to inflict in pursuit of its objectives (Bueno de Mesquita, 1983).

Let i be the initiator of a dispute, and let j be the opponent. In our notation, j 's expected utility calculations are represented by superscript j , and i 's calculations are denoted by superscript i . Following this rule, $E^i(U_{ij})$ denotes the net utility i expects to gain (or lose) from the dispute with j . The object of the dispute for i is to bring j 's policies more into line with those of i . This conflicts with the interest of j , which is to compel i to follow the policy objectives of j . The estimate made by i of the net change in j 's expected utility that would be achieved by continuing a dispute with i , denoted $E^i(U_{ji})$, forms the basis for i 's anticipation of the behavior of j . Of course, j is assumed to make equivalent calculations. Assume that the larger i 's (or j 's) expected utility value, the

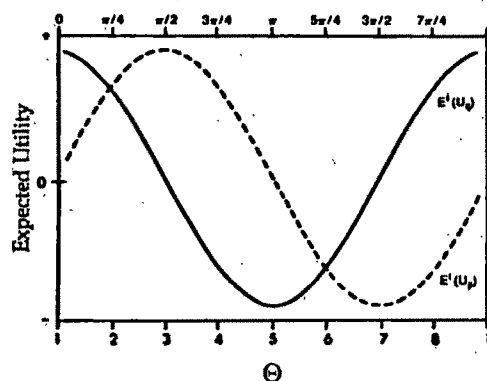
Figure 1. Expected Utilities in a Polar Coordinate System



more inclined i (or j) will be to use force rather than to negotiate to attain its objectives. In particular, assume that the preference for choosing the strategy of using force rather than the strategy of negotiation increases as a monotonic function of the relevant actor's expected utility.

Figure 1 depicts a system of coordinates in which we can represent i 's two expected utility estimates. In previous research, Bueno de Mesquita (1985) developed an ordinal theory of conflict escalation based on the interpretation of this

Figure 2. Expected Utilities as Functions of θ



system using rectangular coordinates. Here, we make use of Lalman's (1985) interpretation of this space as a system of polar coordinates to elaborate a continuous theory of conflict escalation. As Figure 1 illustrates, any pair of expected utility values (and the associated point described by the pair) can be generated as an angle and a vector length. Let r denote the vector length from the origin to the point, and θ denote the angle from the horizontal axis. All points in the space can be represented by varying r , θ , or both. For ease of presentation, we begin our analysis by allowing the angle to vary, while holding r constant at 1, so that Figure 1 illustrates the resulting unit circle.

By unravelling the unit circle from Figure 1, we can more easily depict underlying relationships imbedded in i 's (or j 's) perception of its own expected utility and that of its potential antagonist. Figure 2 shows the values of these variables for all points along the unit circle. Thus, holding r constant and allowing θ to vary produces a cosine curve associated with i 's expected utility and a sine wave associated with j 's expected utility as assessed by i . Allowing r to increase has the effect of increasing the amplitude of the cosine and sine functions. Therefore, θ fixes the location of a point on the two curves, and r establishes the amplitude of the curves.

When a dispute is initiated, two basic courses of action are open to each of the opponents. Each may choose, based on the respective expected utilities, to escalate the conflict by using force or to deescalate the conflict by offering to negotiate. We assume that i 's (or j 's) probability of escalating increases as a monotonic function of the expected utility of i (or j). This assumption stipulates that i prefers more utility to less (thus, larger values of $E^i(U_j)$ are preferred to smaller values) and that i is more willing to use force in pursuit of a larger gain than in pursuit of a smaller one. It is akin to the common assumption in economics that

"more is preferred to less." The assumption merely implies that the more i believes it stands to gain, the more willing i is to fight for those gains.

Functions f and g describe, respectively, the inclination of i and j to escalate. Using f and g we transform the functions in Figure 2 to represent escalatory functions (denoted $P^i(Esc_i)$ for i 's probability of escalating the dispute and $P^j(Esc_j)$ for j 's probability of escalating the dispute, as perceived by i):

$$P^i(Esc_i) = f(r * \cos \theta) \quad (1)$$

$$P^j(Esc_j) = g(r * \sin \theta). \quad (2)$$

By the Pythagorean Theorem,

$$r = [E^i(U_{ij})^2 + E^i(U_{ji})^2]^{.5}$$

and by the rules of trigonometry,

$$\cos \theta = \frac{E^i(U_{ij})}{r},$$

so that

$$P^i(Esc_i) = f(r * \cos \theta)$$

$$= f\{r * [E^i(U_{ij})/r]\} = f[E^i(U_{ij})],$$

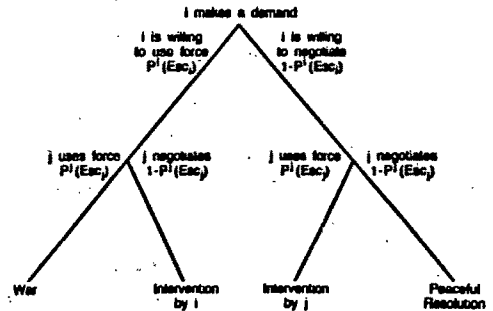
and similarly,

$$P^j(Esc_j) = g(r * \sin \theta) = g[E^i(U_{ji})].$$

These expressions demonstrate that the use of polar coordinates has not altered our assumption that the probability of a nation choosing to escalate a dispute is monotonic with its expected utility.

The escalation functions $P^i(Esc_i)$, $P^i(Esc_j)$, $P^j(Esc_i)$, and $P^j(Esc_j)$ may be used to deduce a broad array of generalizations. Let us begin by examining the strategic options open to j once i initiates a demand. The decision tree in Figure 3 shows that i can make a demand accompanied by an offer to negotiate or accompanied by the use of force; j responds to i 's demand either by selecting the "force" strategy or the "negotiate" strategy.¹ If, at some point in the process, both i and j

Figure 3. Strategic Responses to Threatening Demands and the Probabilities of Specific Outcomes



choose to use force, then, by definition, war results. If both choose to negotiate, then, by definition, the ongoing dispute is resolved peacefully. If one chooses to escalate while the other opts for negotiations, then, by definition, an asymmetric use of force (which we call an *intervention*) results in which one side uses violent means and the other does not.

We now specify four conditions based on Figure 3:

1. $P(War) = P^i(Esc_i) * P^j(Esc_j)$
2. $P(Intervention)$
 $= \{P^i(Esc_i) * [1 - P^j(Esc_j)]\}$
 $+ \{P^j(Esc_j) * [1 - P^i(Esc_i)]\}$
3. $P(Peace)$
 $= [1 - P^i(Esc_i)] * [1 - P^j(Esc_j)]$
4. $P(Violence) = 1 - P(Peace)$
 $= P(War) + P(Intervention).$

Clearly, all four probabilities can be calculated directly from knowledge of the relevant expected utility values and knowledge of f and g . At this point in our research, the expected utility values can be estimated according to the formulation described below. The functions f and g , however, remain unknown. Here we construct f and g as equivalent linear trans-

formations in order to estimate values for the probability of choosing the strategies of fighting or negotiating. We assume

$$P^i(Esc_i) = P^i(U^i(I_{ij}) + 3/6,$$

and

$$P^j(Esc_j) = (E^j(U_{ji}) + 3)/6.$$

These transformations are chosen for their properties as well as their computational convenience. Under our formulation of the calculus of expected utility, values may be obtained in the range +3 to -3. Probabilities, of course, must be in the range 0 to +1. The monotonic functions we have chosen map expected utilities into the [0,1] interval while preserving .5 as the probability of escalation when the expected utility equals 0 (indicating indifference between strategies). These functions also have the characteristic of reaching their limiting values of 0 and +1 when the relevant expected utility attains its minimum or maximum value, respectively. Lalman (1985) examines alternative, logistic forms for f and g to capture the economic notion of declining marginal utility. Eventually we hope to allow f and g to vary from decision maker to decision maker. For now we have chosen an approach that falls in the middle of functions that satisfy monotonicity.

Each of the probability terms delineated above can be estimated to reflect the "objective" likelihood of the relevant event, or each can be calculated from the perspective of i or of j to give that actor's view of the likely evolution of their dispute. The three sets of resulting probability functions would display the differences between the participants' perceptions of their situation and the situation itself. We leave a full discussion of these important perceptual differences to other papers and here merely call the reader's attention to this feature of the model. In the rest of our discussion, we focus on the "objective" probability functions. These

are simply the product of i 's view of what i can do and j 's view of what j can do.

Although each of the probability functions is derived from only the expected utility estimates of the relevant decision makers and from functions f and g , each has its own unique shape and properties. From these characteristics it is possible to deduce several important propositions. We leave for other papers rigorous proofs of most of these propositions but, again, mention some here in passing to draw the reader's attention to the broad potential of this approach. We also note what we believe is a very significant—though now seemingly obvious—insight from Figure 3. War and peace have been discussed in the literature as if they were the two ends of a dichotomy in which one or the other must obtain. Frequently, we have heard the statement "peace is the absence of war" as the justification for a dichotomous war/no war variable in analyses. Blainey (1973, pp. 4, 245), for instance, notes that "any document which discusses an international crisis that ended peacefully is a mirror of both peace and war"; he later adds, "War and peace are more than opposites. They have so much in common that neither can be understood without the other." Yet, the theoretical framework set out here (and in Lalman, 1985) shows clearly that $P(War) \neq 1 - P(Peace)$ and that the logic driving intermediate forms of disputes is readily specifiable and calculable from an expected utility framework. Intermediate outcomes that are neither war nor peace are important in specifying tests of our theory. They are also important because while such outcomes have been observed empirically by others (e.g., Gochman, 1975), they have remained largely unexplained until now.

Before turning to the empirical analysis of several important results from the theoretical structure specified here, let us examine a small number of important propositions.

PROPOSITION 1. *If f and g are strictly monotonic, then necessary and sufficient conditions for war exist only in the limit.*

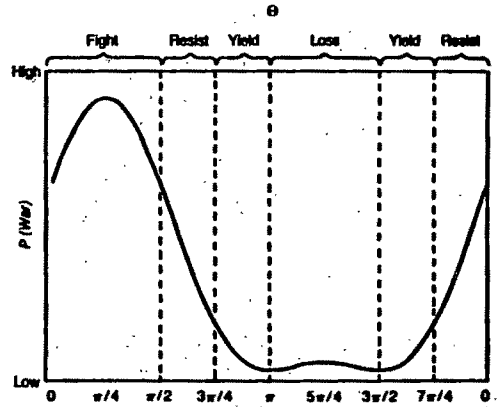
Proof. In order for necessary and sufficient conditions for war to exist, $P(\text{War})$ must equal 1. There is a probability of escalation associated with any given expected utility value. Except in the limit, it is always possible for $P(\text{War})$ to be larger. This value increases as r gets longer if $\theta < 90^\circ$ or if r gets shorter when $135^\circ \leq \theta$, and θ is not more than 215° . At other angles, changes in amplitude can increase or decrease the probability of war depending upon the actual size of r and the specific functions of f and g . Since the amplitude of the relevant escalation function can always shift upward, given strict monotonicity, the probability of escalation associated with the previous expected utility value must have been less than 1.0. Now, necessary and sufficient conditions are satisfied if and only if both parties' probabilities of escalation simultaneously equal 1.0, but we have just seen that with monotonicity neither equals 1.0, except in the limit.

PROPOSITION 1a. *For any f and g , except in the limit, the necessary and sufficient conditions for peace do not exist.*

Proof. Since $P^i(\text{Esc}_i) = 1$ only in the limit, it follows that $1 - P^i(\text{Esc}_i)$ equals 0 only in the limit. The same, of course, is true for $P^j(\text{Esc}_j)$. Now, for necessary and sufficient conditions for peace to obtain, i 's and j 's probability of escalating must equal 0. This condition only arises in the limit. Therefore, except in the limit, $P(\text{Peace}) \neq 1$, and so the necessary and sufficient conditions for peace do not obtain. Peace is never inevitable.

PROPOSITION 2. *The ordinal pattern of the probability of war reported earlier by Bueno de Mesquita (1985) is a*

Figure 4. Probability of War as a Function of Θ

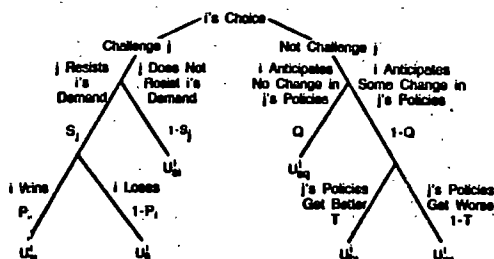


special case of the continuous theory set out here.

Example. Figure 4 displays the probability of war function in generic form, with lines cutting across the function at the boundary between each of the escalatory conditions set out in Bueno de Mesquita (1985). It is evident that the area Bueno de Mesquita called *fight* has a higher probability of war than the area he called *resist*, which, in turn, has a higher probability of war than the area he called *yield*. Finally, this area is higher in terms of the probability of war than is the region he called *lose*.

From the probability functions derived here, it is possible to show when a balance of power (or preponderance) increases the likelihood of peace and when it increases the likelihood of war. With this theory, then, the controversy over the competing hypotheses that peace follows either from a balance of power or from preponderance can be resolved. Likewise, the model allows us to deduce the properties of a power transition that yields war and one that yields peace. Also, the theory allows one to show when an arms race decreases or increases the chance for peace and when arms control increases or decreases

Figure 5. The Decision Problem:
To Challenge or Not to Challenge



the chance for war. The simple theoretical structure set out here and elaborated more fully in Lalman (1985) encompasses structural theories of power distributions, arms races, deterrence, negotiations, bluff, and escalation. It is an important elaboration of earlier work in that it is now possible, at least in theory, to explain variance within as well as between quadrants in the coordinate system described here and in Bueno de Mesquita (1985). It is also possible to incorporate many theories of international conflict, whether focused at the level of individual decision makers or at the level of systemic attributes, into this single framework. However, the efficacy of the theory remains to be demonstrated. That is the task to which we now turn our attention.

The Measurement of Expected Utility

Prior to the onset of a dispute, *i*'s decision problem is to choose between challenging *j*, with the intention of convincing *j* to alter its policies to be more in line with those desired by *i*, and not challenging *j*, leaving *j*'s policies to unfold in whatever manner they are expected to unfold in the absence of pressure from *i*. Figure 5 depicts *i*'s decision problem.

If *i* chooses not to challenge *j*, then one of three contingencies may arise. First, *i* may anticipate that with some probability

(*Q*), *j* will not alter its current policies over the time period of concern to *i*, and so *i* will derive whatever utility it receives from the preservation of the status quo between itself and *j* (U_{sq}^i). Alternatively, *i* may anticipate that *j*'s policies will change, in which case there is some probability (*T*) that, from *i*'s perspective, the policies of *j* are anticipated to improve (with U_{bi}^i being the associated utility) or to get worse (U_{wi}^i). When *i* contemplates challenging *j*, on the other hand, *i* must take into consideration the probability that *j* will not simply give in to *i*'s policy demands (S_j), the likelihood that *i* will succeed in its efforts to enforce its demands (P_i), and the utility associated with success (U_{si}^i) and with failure (U_{fi}^i) in those efforts. The overall calculus of expected utility, then, is

$$E^i(U_{ij}) = E^i(U_{ij})_c - E^i(U_{ij})_{nc}, \quad (3)$$

where the expected utility from challenging *j* equals

$$E^i(U_{ij})_c = S_j[P_i^i(U_{si}^i) + (1 - P_i^i)(U_{fi}^i)] + (1 - S_j)(U_{sq}^i), \quad (4)$$

and the expected utility from not challenging *j* equals

$$E^i(U_{ij})_{nc} = Q(U_{sq}^i) + (1 - Q)[T(U_{bi}^i) + (1 - T)(U_{wi}^i)]. \quad (5)$$

Of course, *j* calculates a comparable equation that reflects *j*'s view of the relative merits of challenging or not challenging *i*. Related to these calculations of expected outcomes are calculations of expected costs associated with the pursuit of the desired outcome, which, as noted earlier, we assume is an increasing function of how *i* (or *j*) thinks its adversary will respond to a challenge—that is, $E^i(U_{ij})$ and $E^j(U_{ji})$.

Estimating the "Challenge" Option

The method used for estimating the utility of success (the U_s terms) and of fail-

ure (the U_i terms) on the "challenge" side of the calculus is explained in Bueno de Mesquita (1985) and elaborated more fully in Bueno de Mesquita, Newman, and Rabushka (1985). The procedure relies on the use of formal alliance data in the European region. The similarity of alliance portfolios is estimated and used as the basic building block for the utility measures. Nations with more similar alliance portfolios are assumed to have higher utility for each other's foreign policy objectives. Utility diminishes monotonically, though not linearly (because of the inclusion of a risk-taking component), as the alliance portfolios become more dissimilar. The estimates of risk-taking propensities among decision makers are taken from Bueno de Mesquita (1985). The procedure assumes that nations whose leaders have adopted foreign policies that leave them near the extreme of their possible range of vulnerability are more accepting of risk than those who locate their foreign policies in a manner that diminishes their vulnerability to external threats (Bueno de Mesquita, 1985; Lusi and Bueno de Mesquita, 1985; Newman, 1982). Differences in risk-taking propensities are viewed as the source of variations in actor perceptions.

The probability of success terms on the "challenge" side of the equation are derived from Correlates of War composite capabilities data (a file of data we acquired from the Inter-University Consortium for Political and Social Research, University of Michigan). The construction of these terms is predicated on the notion of gambling odds so that the proportion of power on one side or the other is used as the estimate of that side's probability of success. Needless to say, alternative formulations are appropriate and will be tested in the future. Especially germane would be functional forms that have a logistic shape. The terms measuring the likelihood of an adversary's not giving in to a demand (S_j in j 's calculus and S_i in i 's

are assumed to equal 1.0, meaning that i 's (j 's) demand is highly contentious (and will be resisted).

In one critical respect, the estimate of probabilities on the "challenge" side of the equation departs markedly from earlier attempts. This departure is linked to the perspective that is taken with regard to the contribution third parties make to the expected utility of i (or j). Because of the manner in which third parties are treated in earlier studies (Bueno de Mesquita, 1981, 1985), the operational estimate of expected utility values for any decision maker could vary between $(2N - 2)$ and $-(2N - 2)$, where N is the total number of nations in the relevant international system. Variations in the size of the international community, then, affected the range of possible values in the expected utility models set out earlier. This is a serious shortcoming in that it makes the comparison of a single nation's expected utility scores in different years difficult. A score of .5 in a system of 10 nations simply does not mean the same thing as a score of .5 in a system of 20 nations, and how those numbers relate to one another is not straightforward. The new formulation fixes the range of values, irrespective of system size, in a theoretically meaningful way. To understand how this is accomplished, let us examine the logic behind third-party choices and what potential opponents might estimate about third-party decisions.

Let us assume an ongoing conflict between some nation i and some nation j . Assuming no conflict will end in a true draw, it is then the case that only two outcomes are possible: i will win or j will win. Thus we stipulate two outcomes: W_i and L_i , where the first refers to the outcome in which i wins, and the second refers to the outcome in which i loses (or j wins). Third party k can choose to intervene in the conflict or remain outside the dispute. We assume abstention is the consequence of k 's indifference to W_i and L_i .

Figure 6. Third-Party Decision Problem

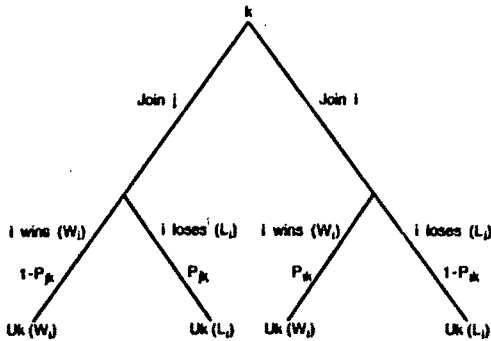


Figure 6 portrays k 's decision problem.

According to the model depicted in Figure 6 (see also Altfeld and Bueno de Mesquita, 1979), k 's choice between joining i and joining j depends on the probability of i winning, given that k helps i (P_{ik}); the probability of i losing, even though k helps i ($1 - P_{ik}$); the probability of j winning (i losing) given that k helps j (P_{jk}); the probability of j losing (i winning), even though k helps j ($1 - P_{jk}$); and the utility k attaches to the two possible outcomes. Expressed algebraically, k 's expected utility for joining i or j equals

$$E(U)_k = [P_{ik}U^k(W_i) + (1 - P_{ik})U^k(L_i)] - [(1 - P_{jk})U^k(W_i) + P_{jk}U^k(L_i)] \quad (6)$$

If equation (6) is positive, k expects more utility from joining i than j . If it is negative, k expects more utility from joining j than i , and if the expression equals zero, then k is indifferent between i and j and so abstains from the dispute. The terms in equation (6) may be rearranged by factoring to yield

$$E(U)_k = [P_{ik} + P_{jk} - 1] \cdot [U^k(W_i) - U^k(L_i)] \quad (7)$$

Assume no nation joins a conflict with the expectation of harming the side it chooses to join so that the a priori probability of i winning if k abstains is not

larger than the probability of i winning if k joins i , and likewise the a priori probability of j winning is not diminished by k joining j . That is, we stipulate that

$$P_{ik} \geq P_{ib}; P_{jk} \geq P_{jb} = (1 - P_{ib}), \quad (8)$$

where P_{ib} and P_{jb} are the respective probabilities of i and j winning a *strictly bilateral* dispute (as estimated by k).

From equation (8) it is evident that $(P_{ik} + P_{jk} - 1)$ is always greater than or equal to zero. After all, $P_{ik} + P_{jk} \geq [P_{ib} + (1 - P_{ib})]$, and $[P_{ib} + (1 - P_{ib})] = 1$. From this it follows that equation (7) can be negative only if $U_{ki} < U_{kj}$. Consequently, k chooses to join j strictly on the basis of k 's preference for j over i . Likewise, k joins i strictly as a function of k 's preference for i over j . How valuable k 's participation is on either side depends, not only on k 's utility for one or the other initial belligerent, but also on k 's probability of altering the outcome. This is exactly what is expressed by equation (7). Indeed, empirical support for this generalization and for the efficacy of equation (7) is found in Altfeld and Bueno de Mesquita (1979).

Once P_{ib} and P_{jb} (which together equal 1.0 and represent the probabilities when i and j act alone) are subtracted (as dictated by $[P_{jk} + P_{ik} - 1]$), all that remains is k 's marginal contribution to the probability of the outcome. This can be seen most easily by adding an operational assumption. Let $P_{ik} = (C_i + C_k)/(C_i + C_k + C_j)$, where C refers to the capabilities or power of the subscripted actor. Similarly, let $P_{jk} = (C_j + C_k)/(C_i + C_j + C_k)$. Then,

$$\begin{aligned} (P_{ik} + P_{jk} - 1) &= \frac{C_i + C_k}{C_i + C_j + C_k} \\ &+ \frac{C_j + C_k}{C_i + C_j + C_k} - \frac{C_i + C_j + C_k}{C_i + C_j + C_k} \\ &= \frac{C_k}{C_i + C_j + C_k} \end{aligned}$$

The above results show that k 's prefer-

ences determined its choice and that the expected value of that choice is determined jointly by the intensity of k 's preference for one side or the other and k 's capacity to affect the outcome of the conflict between i and j . Let us assume that k 's effort on behalf of its preferred side is proportional to k 's expected utility from participation on that side. Of course, i (or j) cannot be certain of the intensity of k 's preference, although the side k prefers can be viewed as known. However, just as i can estimate j 's expected utility, so too i can estimate k 's expected utility for joining i or joining j . Substantively, such an estimate evaluates the reliability of potential allies of i and j and is critical to i 's assessment of its probable success in a dispute with j .

We assume that i and j each are prepared to commit all their resources to their dispute.² The probability of success for i , then, is a function of i 's power combined with the contribution i expects from third parties that support i , relative to the total power that is expected to be in the conflict. We define P_i operationally as

$$P_i = \frac{C_i + \sum_{k:ipj} (C_k) [(U_{ki} - U_{kj})/2]^i}{C_i + C_j + \sum_{k \neq i,j} (C_k) [(U_{ki} - U_{kj})/2]^i} \quad (9)$$

Substantively, the numerator of equation (9) indicates that i is prepared to use all of its resources to achieve i 's objectives. The numerator also includes i 's estimate of the proportion of third-party resources that will be available to i . That proportion is based only on those third parties believed by i to prefer i to j . It is estimated as the sum of the products of the capabilities of each such third party times i 's approximation of each third party's intensity of preference for an outcome favoring i over j . The denominator is i 's estimate of the total resources expected to be involved in the dispute. Because these are subjective estimates, i 's calculation of P_i and j 's calculation of that same term need not be equal. To denote

the difference between i 's and j 's perceptions of these values, we use a superscript. Thus, P_i^i is i 's estimate of i 's probability of success, and P_i^j is j 's estimate of that same probability.

Estimating the "No Challenge" Option

The "no challenge" side of the equation, though discussed in passing in Bueno de Mesquita (1985), is estimated for the first time here. To estimate the utilities associated with anticipated changes in policies when such changes have not been, and cannot have been, observed represents a difficult challenge. We have approached the problem by assuming that preferences among alternative policies are single peaked and that war/peace questions can be represented as if they fall on a unidimensional continuum. With these assumptions—and with the usual assumptions of transitivity, connectivity, and rationality—we are able to take advantage of Black's (1958) median voter theorem. In particular, we assume that in the absence of a policy challenge, international issues are resolved by a tendency for policy to drift toward the "median voter," or Condorcet winner position. Of course, we recognize that voting, in the liberal democratic sense, does not exist in international politics. Rather, we have in mind that the "might makes right" shibboleth is a description of "voting" in contests of power and will. The power or capabilities of nations (denoted C_i for nation i) represent the quantity of "votes" they could cast in a policy debate. The utility they attach to alternative policy choices is their intensity of preference, or the will to see one outcome prevail over another. Thus, the difference in utilities acts as a discount on their exertion of effort, or their "casting of votes." If a nation's key leader, for instance, were indifferent to the two alternatives of a debate, then none of that nation's power would be spent in trying to influence the resolution of that debate. If the same

nation's leader felt intensely about one of the alternatives, then much of its power would be committed to influencing outcomes. Thus, we define the "vote" of each nation in a policy debate over alternatives x and y as

$$V_i = (C_i)(U_x^i - U_y^i).$$

By summing this calculation across all nations and repeating it for all pairwise comparisons of alternatives, we find the Condorcet winner. That outcome, as Black (1958) has shown, is the position of the median "voter." We assume that if issues are resolved purely through the pulls and tugs of international discussion, then each nation in the community of states is pulled toward tacit toleration of this outcome. We infer that i 's best guess as to the policy j will adopt if i does not directly challenge j is that if j shifts its stance, j will move to the position of the Condorcet winner. This inference assumes that if j is willing to alter its stance, then j will move to a policy position that minimizes j 's future vulnerability to defeat.

How likely is j to move to this position? Four approaches to estimating an answer to this question suggest themselves. One approach is to assume that i 's best guess is that j will continue to pursue whatever policies j is pursuing at the moment. This is the assumption made in Bueno de Mesquita (1985). A second assumption is that j will continue to change its policies at the same rate as those policies were changing vis-a-vis i over the recent past. This turns out empirically to be equivalent to the first assumption for the events examined here. A third approach assumes that estimates of the probability of j shifting positions is a function of j 's risk propensity (or i 's perception of that propensity). The fourth approach is to assume that i is maximally uncertain about j 's future behavior—that is, i believes j will continue its current policies with probability .5, and j will move

toward the Condorcet winner position with probability .5. We adopt the last perspective because it assumes the least information about j 's future behavior. Other approaches will be explored more fully in subsequent analyses.

We stipulate that $Q = .5$. We calculate the utility from the status quo in exactly the manner described in Bueno de Mesquita (1985). The Condorcet winner policy outcome is calculated in the manner described above, and i 's utility for a shift by j to that position is the difference between i 's utility for that position and i 's utility for j 's current position. If j is on the opposite side of this alternative from i , then i expects to derive a benefit from j 's move (and so U_{bi} is expected with probability 1.0, given that j is expected to change its position—which occurs with probability .5). If j is on the same side of the "winning" position as i (and between i and the "winning" position), then a move by j to that predicted outcome makes i worse off (U_{wi} is obtained with probability 1.0, given that j changes its current position—which occurs with probability .5). If j is on the same side of the "winning" position as i and is farther away from that position than is i , then the utility i attaches to j 's move can reflect an improvement or a deterioration in relations between i and j , depending on whether j ends up farther from i 's ideal point after the anticipated move than j was at the outset.

Testing the Theory

We test portions of the theory set out here by focusing on 133 conflicts in Europe from 1816 through 1965. These are the same cases that were examined in Bueno de Mesquita (1985). In subsequent studies we hope to expand our analyses to include a broader range of conflict and nonconflict situations from all parts of the globe.

The 133 cases have been categorized

according to the level of escalation the dispute reached. Thirty-four cases became wars, as defined by Singer and Small (1972). Another 48 events escalated to the level of an intervention in which one side used armed force against the adversary (Gochman, 1975). The remaining 51 conflicts were threats (Gochman, 1975) in which neither side escalated beyond talk to direct military action against the adversary. These disputes were resolved peacefully.

We have claimed that this theory represents an improvement over its predecessors. This version has the potential of accounting for variance within as well as between *fight*, *resist*, *yield*, and *lose* situations, where those situations are defined as follows:

- Fight = $E^i(U_{ij}) > 0$ and $E^j(U_{ji}) > 0$
 Resist = $E^i(U_{ij}) > 0$ and $E^j(U_{ji}) < 0$ and
 $|E^j(U_{ji})| < E^i(U_{ij})$, or $E^i(U_{ij}) > 0$ and
 $E^i(U_{ij}) < 0$ and $|E^i(U_{ij})| < E^j(U_{ji})$
 Yield = $E^i(U_{ij}) > 0$ and $E^j(U_{ji}) < 0$ and
 $|E^j(U_{ji})| > E^i(U_{ij})$, or $E^i(U_{ij}) > 0$
 and $E^i(U_{ij}) < 0$ and $|E^i(U_{ij})|$
 $> E^j(U_{ji})$
 Lose = $E^i(U_{ij}) < 0$ and $E^j(U_{ji}) < 0$.

As the decision tree in Figure 3 makes clear, disputants must choose between a strategy of negotiation and a strategy of force. War, as an outcome, is never pitted directly against a peaceful resolution because war can only arise when both parties select the option of using force. Thus, the theory dictates that analyses first pit violent disputes (interventions and wars) against nonviolent disputes (threats) and then pit interventions against war. This means that testing of the model requires splitting the data into two overlapping sets, one that has a dependent variable coded 1 for violent disputes (wars and interventions) and 0 for threats and another coded 1 for wars and 0 for

interventions. We call the first dependent variable *violence* and the second *war*.

Because our dependent variables are dichotomous, we test the theory by using logit analysis. The tests are performed by estimating logit models controlling for whether the context of the dispute is a fight, resist, yield, or lose situation.³ In each logit analysis, we estimate whether an event (threat, military intervention, or war) did or did not happen. Logistic regression is, of course, an appropriate estimation technique for models with dichotomous dependent variables.

Proposition 2 shows that the original ordinal model of conflict behavior is a subset of the continuous model set out here. One test of the efficacy of the theory, then, is to determine whether the continuous version is capable of explaining variance in conflict outcomes *within* the ordinal categories. The earlier theory, of course, could only explain variance *between* categories. The test of proposition 2, then, examines whether the new theory does as well or better than the simpler ordinal theory.

Two sets of analyses follow from the examination comparing threats and violent disputes. The first approach ignores our *post hoc* historical knowledge of which cases became violent. This test relies only on the model to separate candidates for violence from all 133 cases and then to further separate those candidates into interventions and wars. The second approach is to use all the information in the data set by focusing in the second instance only on the cases that actually became violent. This approach assumes that decision makers can readily observe that one or another party to a dispute is prepared to use force. The question then is whether that use of force will prompt negotiations or a reciprocal use of force.

In analyses concerned with the choice between war and intervention, the relevant independent variables are $P(\text{War})$, $P(\text{Intervention})$, $E^i(U_{ji})$ (as an indicator of

costs expected by i), and $E^i(U_{ij})$ (as an indicator of costs expected by j). The variable $P(War)$ should, of course, be positively associated with the dependent variable, as the higher this value is, the more likely it is that an ensuing dispute will become a war. Likewise, $P(Intervention)$, which assesses the probability that one or the other decision maker will choose to use force when his adversary chooses to negotiate, should be negatively associated with the tendency of the ensuing conflict to become a war. The two expected utility values each represent one actor's view of the other actor's expectations. These are, as noted earlier, assumed to be indicators of expected costs. Consequently, each should be negatively associated with the tendency for the relevant actor to escalate the conflict. Each of the probability variables, according to the theory set out earlier, has a different impact under the circumstances described in different parts of the polar coordinate system (reflecting, as they do, different negotiation contexts). This means that we should allow the slope to vary, which we do by creating dummy variables for each of the main areas of the coordinate system (fight, resist, yield, and lose). The theoretical variables — $P(War)$ and $P(Intervention)$ —and the two cost variables are multiplied by these dummy variables.

Analysis concerned with the distinction between nonviolent conflicts and violent disputes, according to the theory presented here, requires the specification of a variable we call $P(Violent)$, which measures the probability of a dispute becoming violent. This variable is defined simply as $[P(War) + P(Intervention)]$, these two probabilities defining the only circumstances under which a conflict can escalate. The theory also requires the specification of expected costs, and so we include $E^i(U_{ji})$ and $E^j(U_{ij})$. For this investigation, dummy variable interactions are again required to reflect the contextual change across the four situations specified

earlier (Bueno de Mesquita, 1985).

The probability variables, the cost variables, and even the sectoral dummy variables are all derived from four factors: $E^i(U_{ij})$, $E^i(U_{ji})$, $E^j(U_{ij})$, and $E^j(U_{ji})$. The independent variables are derived theoretically, and their presence in the analysis is dictated by theory. Thus, they must all be included to avoid biased categorization of the dependent variable, but there is, of course, an enormous amount of collinearity among them. This makes impossible the reliable estimation of parameters or significance levels for individual variables.⁴ Our main concern is with the theory's ability to discriminate between types of conflicts, and that can be reliably estimated using logit analysis. However, because we are sure some readers will want information on the performance of individual variables, we include here correlation coefficients between the probability terms and the dependent variables, and one limited logit analysis that includes probability and cost terms. We are particularly interested in demonstrating the power of the functional forms governing the definition of the probability variables. Thus, in examining the correlation coefficients, particular attention should be paid to the success of the appropriate probability variables in accounting for the relevant dependent variables and the inability of inappropriate probability variables to explain events outside their theoretical domain. Let us turn now to these analyses.

First we report, in Table 1, the simple bivariate correlation between each of the probability variables and whether the dispute became violent. The correlations are interesting in several respects. Note that $P(Intervention)$ is positively correlated with the dependent variable called *violent* but negatively correlated with the variables that assess whether a dispute escalated to warfare. This is exactly what the theory leads us to expect. Second, observe that the correlation of $P(War)$ rises stead-

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Table 1. Bivariate Correlations between Theoretically Derived Probabilities and the Escalation of Disputes

Dependent Variables	$P(\text{Violent})$	$P(\text{War})$	$P(\text{Intervention})$	$P(\text{Peace})$
<i>Violent</i> ($N=133$)	.31	.23	.16	-.31
<i>War</i> ^a ($N=133$)	.31	.41	-.27	-.31
<i>War</i> ($N=82$)	.32	.45	-.52	-.32

^aThe dependent variable *War* is correlated with the independent variables first for all 133 events, and then only for the 82 events known to have become violent (i.e., 34 wars and 48 interventions). The dependent variable *Violent* includes 51 threats, 48 interventions, and 34 wars.

ily as the dependent variable moves closer to the domain of events it should theoretically explain. Thus, $P(\text{War})$ is most powerful when war is a designated outcome on the dependent variable and does least well when undifferentiated violence is a designated outcome. The same is true for $P(\text{Intervention})$ when the dependent variable differentiates these two types of conflict. Third, we should observe that $P(\text{Violent})$ —which is $[1 - P(\text{Peace})]$ —is the strongest predictor when the dependent variable discriminates only between violent and nonviolent disputes; when the dependent variable is more discriminating, the more specific probability terms emerge as the stronger predictors.

We present one illustrative logit analysis in which we do not control for situational context. Four independent variables are used to discriminate between conflicts that became wars and those that remained at the level of an intervention. The independent variables are $P(\text{War})$, $P(\text{Intervention})$, $E^i(U_{ij})$, and $E^j(U_{ij})$.⁵ The theory indicates that the first of these variables should be positively associated with escalation to warfare and that the latter three should be negatively associated. This means that when the probability of an intervention is high, the conflict remains an intervention, and when costs

are expected to be high, this acts as a force to hinder escalation. The actual logit results are⁶

$$\begin{aligned} \text{War} = & -21.27 + 139.78 [P(\text{War})] \\ & (p < .02) \\ & - 26.35 [P(\text{Intervention})] - 10.11 [E^i(U_{ij})] \\ & (p < .40) \quad (p < .02) \\ & - 10.94 [E^j(U_{ij})]. \\ & (p < .02) \end{aligned}$$

The overall logit model fits very well, with $X^2 = 32.71$ and the probability of the results having arisen by chance being less than one in a thousand. The Kendall rank order correlation between predicted and actual outcomes is .65 ($N = 82$), and the signs of the coefficients are all in the predicted direction, with only $P(\text{Intervention})$ being insignificant.

Let us turn now to the results of the fully specified logit analyses. First we examine the predictive capabilities of the theory against the 133 disputes, of which 51 were resolved peacefully and 82 became violent. Table 2 compares the predicted and actual categorization of events. As can be seen, the theory does excellently at isolating the violent disputes but at the expense of over-predicting such events. Of the 82 violent disputes, all but 5 were predicted to be violent by the

Table 2. Predicted Violent and Nonviolent Disputes in Europe, 1816-1965

Actual Event	Predicted Event	
	Nonviolent	Violent
Nonviolent	20	31
Violent	5	77

theory. All 34 wars were correctly categorized as disputes that involved force. The five cases that were erroneously classified as nonviolent were interventions, a lower level of violence. Of 51 threats, 20 were correctly categorized. The quantity X^2 for the model, with 12 degrees of freedom ($N = 133$), is 32, which is significant at about the .001 level.⁷ The Kendall rank correlation between predicted probabilities and actual categorization is .42.

With these results in hand, we turn to our two remaining analyses. In one, we use the predictions of the previous analysis to identify the candidates for violence. Those 108 candidates (77 cases correctly predicted to be violent plus 31 incorrectly predicted) are pitted against the theoretical model that should discriminate between disputes that became wars and those that did not. Here we use only the model to identify candidates for violence and then use the model again to discriminate between those conflicts that were likely to become wars and those that were not. Thus, we rely on theoretical predictions to generate subsequent predictions, running the risk of categorizing as wars

cases known to be nonviolent. The test is extremely stringent because we carry our errors from the first stage of analysis into the second stage. Despite the stringency, the theory performs very well. Table 3 shows that 63 of the 74 contingent events that did not become wars were predicted not to become wars and that 18 of the 34 wars were correctly categorized. The quantity X^2 for the logit analysis is 39.36, which, with 12 degrees of freedom, is significant at less than the .001 level.⁸ The Kendall rank correlation for the predicted and actual outcomes is .70 ($N = 108$).

We turn now to the test that takes advantage of the available information. Here we examine only those cases in which at least one party has revealed its willingness to use force and ask, which of these violent disputes will become wars? With the added knowledge that all of these disputes are violent (whereas the contingent analysis included 31 threats that were mistakenly categorized as candidates for violence), the model should do still better, and it does. Table 4 reveals that 44 of the 48 interventions are correctly identified and that 27 of the 34 wars are likewise correctly categorized.

Table 3. Contingent Analysis Predicting War and Intervention in Europe, 1816-1965

Actual Event	Predicted Event	
	Intervention	War
Intervention (Or carried-over threat)	63	11
War	16	18

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Table 4. Analysis with Information Predicting War and Intervention in Europe, 1816-1965

Actual Event	Predicted Event	
	Intervention	War
Intervention	44	4
War	7	27

This is over 90% of the interventions and nearly 80% of the wars. The result is so strong that the theory contributes a proportionate reduction in error of more than 67%. This means that the theory makes two-thirds fewer errors than would have been experienced if we had predicted that each event would fall in the modal event category of military intervention. The quantity X^2 for the analysis is 72.25, which, with 12 degrees of freedom, is significant at less than the .001 level. The rank correlation is an astonishing .94 ($N = 82$).

The theory presented here has proven to be a powerful tool for distinguishing between violent and nonviolent disputes and, within the category of violent conflicts, between interventions and wars. One final analytic question remains to be addressed: is this a better theory than the ordinal model proposed earlier (Bueno de Mesquita, 1985)? To answer this question we examine the success of the ordinal theory on the same data sets and compare the results. The new theory is superior if it performs significantly better than the earlier version. The strictly ordinal model produces an X^2 statistic of 16.29 when the dependent variable distinguishes only between violent and nonviolent disputes. With 3 degrees of freedom, this is significant at the .001 level. The difference in X^2 for the tests of the ordinal and continuous theories is itself significant at about the .05 level, indicating that the continuous theory set out here is a substantial improvement. This is seen even more clearly when we turn to the performance of the

two models on the 82 cases of interventions and wars. The ordinal model again is significantly associated with the actual outcomes at the .001 level, but the difference in X^2 across the two tests is also significant at less than the .001 level. Thus, the ordinal theory correctly predicted 58 of 82 events, representing a 29% reduction in error. As noted, the improved theory correctly categorized 71 of the 82 events, accounting for more than half of the cases that remained unexplained by the ordinal theory, which was itself a highly significant predictor.

Conclusion

We have proposed a continuous theory linking expected utility estimates to conflict escalation. We have shown that the theory is a powerful tool for discriminating among events likely to be violent or nonviolent and especially among events likely to involve only modest levels of force and those that reach the level of warfare. We have shown that important new propositions can be deduced from the theory and have indicated that many other propositions will be derived later. We have demonstrated that peace is not the opposite of war but that an important contingency arises when one adversary chooses to use force and the other chooses to pursue a strategy of negotiation. We have also shown that the expected utility framework is readily able to distinguish the circumstances under which these alternative strategies are chosen. We have

demonstrated improved predictive ability resulting from the development of a continuous theory of escalation (see also Lalman, 1985) and an improved specification of the expected utility structure. The revised theory incorporates information more effectively, explicates rational calculations in a manner consistent with the expected utility axioms while making weaker assumptions than earlier efforts; it explains many apparent anomalies reported in the literature on international conflict. We believe we have demonstrated strong, cumulative results that encourage further confidence in an expected utility paradigm of international conflict behavior and rational decision making.

Notes

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1. These strategies are similar, but not identical, to the strategies of "stand firm" and "stand down" investigated by Petersen (1983) in his study of the relationship in certain types of disputes, between expected utilities and escalation to war.

2. Recall that we assume S_j and S_i equal 1.0.

3. The last two (yield and lose) are combined in the logit analyses that evaluate the likelihood of a dispute to remain an intervention or become a war because there is too little variance on the dependent variable in the lose category to make computations possible for that condition by itself.

4. Significance tests are used here as a heuristic device to suggest the relative strength of associations.

5. Recall that $[P(\text{War}) + P(\text{Intervention})]$ does not equal 1.0. It is $[P(\text{War}) + P(\text{Intervention}) + P(\text{Peace})]$ that equals 1.0—that is, war is not equivalent to the absence of peace.

6. The probabilities reported in parentheses are derived from X^2 values for the individual coefficients estimated in the logit analysis. Multicollinearity is a problem even in this limited logit analysis. Rerunning the analysis dropping either $P(\text{War})$ or $P(\text{Intervention})$ leaves the remaining probability variable statistically significant.

7. The independent variables for this analysis were $\text{Fight} \cdot P(\text{Violent})$, $\text{Resist} \cdot P(\text{Violent})$, $\text{Yield} \cdot P(\text{Violent})$, $\text{Lose} \cdot P(\text{Violent})$, $\text{Fight} \cdot E^i(U_{ij})$, $\text{Fight} \cdot E^j(U_{ij})$, $\text{Resist} \cdot E^i(U_{ij})$, $\text{Resist} \cdot E^j(U_{ij})$, $\text{Yield} \cdot E^i(U_{ij})$, $\text{Yield} \cdot E^j(U_{ij})$, $\text{Lose} \cdot E^i(U_{ij})$, and $\text{Lose} \cdot E^j(U_{ij})$.

8. The independent variables are $\text{Fight} \cdot P(\text{War})$, $\text{Resist} \cdot P(\text{War})$, $(\text{Yield} + \text{Lose}) \cdot P(\text{War})$, $\text{Fight} \cdot P(\text{Intervention})$, $\text{Resist} \cdot P(\text{Intervention})$, $(\text{Yield} + \text{Lose}) \cdot P(\text{Intervention})$, $\text{Fight} \cdot E^i(U_{ij})$, $\text{Resist} \cdot E^i(U_{ij})$, $(\text{Yield} + \text{Lose}) \cdot E^i(U_{ij})$, $\text{Fight} \cdot E^j(U_{ij})$, $\text{Resist} \cdot E^j(U_{ij})$, and $(\text{Yield} + \text{Lose}) \cdot E^j(U_{ij})$.

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A SPATIAL MODEL OF INTERNATIONAL CONFLICT

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The model presented here assumes that nations initiate conflict to change the international status quo across one or more issues to an outcome they prefer. Their preferences for different issue outcomes are represented by a classical spatial utility function. International coalitions are formed both to enhance the chance of a successful challenge and to counter existing challenges. Disputes are modeled as periods of competition between two coalitions through the recruitment of additional members, erosion of support for the opposing coalition and escalation of the crisis. The two-nation model is analyzed in detail, and the existence of the core for the resulting game is proved. The two main conclusions of the two-nation model are that nations have a general incentive to exaggerate their issue positions in disputes and that the possibility of issue trades enhances the likelihood of compromise.

Because scientific understanding arises from theories, the explication of theories is essential for the development of scientific understanding. To understand international conflict—that is, the resolution of disputes between nations by coercion or the threat of coercion—a theory that can explain such conflict is needed. This paper will present a model drawn from rational choice theory—specifically game theory—that explains national actions in conflicts as strategic choices made in order to gain preferred resolutions of the international issues in dispute. Compared to existing game theory models of international conflict, the model presented here provides an explicit description of how each nation's evaluation of the issues in dispute affects its desire to pursue compromise or conflict; a more detailed sequence of escalation allowing for the detailed study of escalation strategies; and a framework that can explain the timing and extent of

third-party interventions into ongoing crises.

Why a Spatial Model?

The application of game theory to international conflict has not yet supplied a general explanation of crisis behavior, despite some suggestive insights into the nature of crises (e.g., Snyder and Diesing, 1977). There are a number of reasons why this hope has not been fulfilled. First, all game models must assign utilities to the possible outcomes; normally, a utility evaluation is simply given for each outcome. However, rational choice models are most compelling when the actors' utilities for outcomes can be tied to primitive quantities (Simon, 1985), like income or goods in economics or to the resolution of issues, as in the spatial theory of elections. Because the outcomes of international issues do change as a conse-

quence of international conflict (e.g., territory changes hands and political policies change) and because the entire character of a dispute changes as the issues in dispute change (e.g., a U.S.-Soviet crisis over Nicaragua is fundamentally different from a U.S.-Soviet crisis over the Middle East), each nation's utility for the possible outcomes of a conflict will be identified with the possible issue resolutions of the conflict and the material costs that will be incurred during the conflict. The formal representation of national preferences for possible issue outcomes will be given by spatial utility functions (Davis, Hinich, and Ordeshook, 1970) as commonly used in the spatial theory of elections.

Many additional advantages are gained by adopting spatial utility functions identified with international issues. A much greater diversity of possible outcomes becomes immediately possible under this framework, unlike the four outcomes presented in a 2×2 game. The selection of which issues are included in a conflict now becomes an endogenous question in the model: the issues in dispute will be those that the actors include in the dispute for strategic reasons. A detailed picture of the conflict of interest between the parties can be captured by breaking down their differences into separate issues and comparing their positions on those issues with those of third parties. Differences in the actors' evaluations of the importance of the issues and in their willingness to take risks are reflected by a spatial utility function. Because of these advantages, a formalism drawn from the spatial theory of elections is exceptionally appropriate for the representation of national issue preferences here.

The second reason that applying game theory to international conflict has not yet led to a general explanation of crisis behavior is that any game model must constrain and structure the choices it provides the actors. However, many rational

choice models, such as 2×2 games and Bueno de Mesquita's (1981, 1985) expected utility theory, reduce the diversity of choices to a dichotomy between war and peace, firmness and conciliation. As Wagner (1983, 1984) points out, this reduction of choice to two alternatives may oversimplify the decisions, leading to incorrect predictions. In this model, an escalation ladder replaces the simple choices of war and peace that characterize most rational choice models. The parties are then free to stop at intermediate levels of escalation if they believe that further escalation would be counterproductive. This additional detail also allows this model, unlike other rational models, to capture the dynamics of escalation, creating the possibility of scientific crisis management.

Third, almost every dispute involves more than two actors, if only for the unrealized intervention of a third party. Models based on two-person game theory can only attempt to capture the effects of third parties on conflict by modifying the payoffs or probabilities, as in Bueno de Mesquita's (1981, 1985) expected utility model. A better approach is to include all possible intervening nations as actors in the model, creating an n -person game. One focus of the model presented here is the recruitment of uncommitted third parties to the contending sides. Third-party interventions then become endogenous to the model, allowing for the explanation of the timing and magnitude of interventions. Furthermore, because of the model's detailed consideration of the issues in the dispute and its escalation ladder, it can explain why the two sides might shape their issue demands and escalatory actions to encourage or discourage possible third parties from intervening.

There are two previous efforts to use spatial modeling as a means of developing a more detailed rational choice theory of conflict: Morrow (1985a) and Morgan

(1984). The Morrow paper can be considered as a special case of the model presented here, the case of one issue and a one-step escalation ladder. The Morgan paper attempts something rather different: Morgan creates a probability distribution for each side's willingness to accept possible compromises and then derives the probability of each possible compromise and of war. Like Morrow (1985a), Morgan only considers a one-issue case, losing much of the detailed characterization of the issues that a spatial model can provide. Furthermore, Morgan's probability distribution for acceptable compromises is simply assumed (within certain reasonable restrictions), instead of being derived from fundamental terms like the capabilities and issue preferences of the two sides. The model presented here allows the deduction of each side's bargaining position from fundamental terms and could provide a means to derive the probability distributions that power Morgan's model.

This paper will unify three distinct traditions in formal theory—*n*-person game theory, the expected utility theory of war, and spatial modeling—to produce a formal theory of international conflict. The theory developed here produces far more detailed predictions than any of the other approaches alone.

Briefly, this theory assumes that nations act in disputes to obtain policy outcomes preferable to those that would have occurred had they not acted. A coalition (sometimes only one actor) arises to challenge the status quo across one or more issues, in the expectation of improving the status quo. In response to this challenge, a countercoalition (perhaps just one actor) may or may not emerge in the hope of thwarting the challenging coalition. In the sequence of negotiation and threats between the coalitions, escalation and concessions are the tools that each coalition uses to intimidate and entice the members of the other coalition. Eventually,

the dispute will reach a resolution when both coalitions either compromise or resort to the application of force. *N*-person game theory describes the internal struggle within a coalition for domination as well as the contest between coalitions for dominance over the contended issues. The members of each coalition will evaluate their positions within their coalitions by considering their expected utilities calculated over the issues as measured by their individual spatial utility functions.

Basic Assumptions

Because this model explains behavior in disputes as a matter of rational choices within a constrained framework, we assume that actors are capable of coherent choice. The employment of any decision theory based on utility theory requires the assumption that each decider has complete, transitive preferences (Luce and Raiffa, 1957; Riker and Ordeshook, 1973; Savage, 1972). For individuals, this assumption is generally not problematic. However, we cannot necessarily expect aggregations of rational individuals to reach consistent decisions (Arrow, 1963). If we assume the preferences of the members of the deciding group are restricted in certain ways, then the group can arrive at a consistent set of preferences over the alternatives (Sen, 1970).¹

Generally, value restrictedness within the political leadership of a nation is more likely when decisions are reached by a small group. Because crisis decisions are made by the top political leadership of a country, value restrictedness and, therefore, coherent group preferences are likely to be the rule rather than the exception.² Consequently, the actors considered by this model will be restricted to modern nation-states. Nations have governments that are responsible for their foreign policy decisions, and those foreign policy

actions provide a sufficient base of international policy acts to permit the inference of a set of preferences from those actions.

Having defined the participants, we need to clarify the stakes in international conflict. This model postulates that international conflicts arise over international issues.³ An *international issue* is defined as a policy or a set of policies of a government that has international implications and for which all the actors' preferences concerning the outcome are single-peaked; that is, each actor has a single preferred outcome (called its *ideal point*), and an actor's preference for outcomes of an issue drops off as the distance between the ideal point and the outcome increases. Issues provoking conflict include territorial disputes (the policy in question being the possession of a piece of land), external political policies, and those internal political policies that are viewed by at least one government as having international ramifications. For instance, the conflict between Iraq and Iran that has smoldered through their five-year war covers the territorial issue of possession of the Shatt al-Arab waterway, the Iranian policy of Islamic fundamentalist agitation in the Arab world, and the composition of the Iraqi government.

The definition of issues in this model is left deliberately vague to allow the actors to define what issues are relevant to international politics. Each individual issue is cast as a continuum representing the different positions that could be adopted on that issue. The entire realm of issues that could provoke conflict can then be represented formally by an m -dimensional Euclidean space, as is common in spatial voting theory. For convenience, the status quo over all the issues will be defined as the origin.

Each nation's ideal point gives the complete set of issue positions preferred by that nation over all other complete sets of positions. For any nation i , its ideal point

will be denoted x_i . Each nation's preferences for issue outcomes will be represented by a utility function based on the weighted distance of each point from its ideal point. The weights attached to the different issues reflect the relative saliency of each issue from the perspective of that nation. These weights also allow for the interactive effects of multiple issues, representing the perceived dependence of the outcomes of some issues on the outcomes of others.

Each nation's utility function also incorporates its willingness to take risks, as exhibited in the curvature of its utility function. Those nations unwilling to take risks, referred to as *risk-averse*, have concave utility functions, while those nations willing to take risks, called *risk-acceptant*, have convex utility functions. According to this approach, the willingness of a nation to take risks arises out of the difference between its expected utility for a gamble and its utility for a sure thing (Friedman and Savage, 1948).

The general form of the utility function will reflect differences in (1) ideal points, (2) in the salience of issues, (3) in combinations of issues, and (4) in attitudes toward risk. Formally, the following utility function allows for all four of these variations within its structure:

$$u_i(x) = -[(x - x_i) A_i (x - x_i)^T]^{1/2} r_i, \quad (1)$$

where x_i is nation i 's ideal point, A_i is an m -by- m positive matrix, and r_i is nation i 's risk attitude.⁴ An actor's utility for an outcome is the weighted distance between that outcome and the ideal point altered by the risk attitude of the actor.

Sample utility functions in a two-dimensional issue space are given in Figure 1. Each function is represented by a set of indifference curves. All three utility functions have different ideal points given by the cross in the center of each set of curves. The A_i matrix produces both the eccentricity and the orientation of the

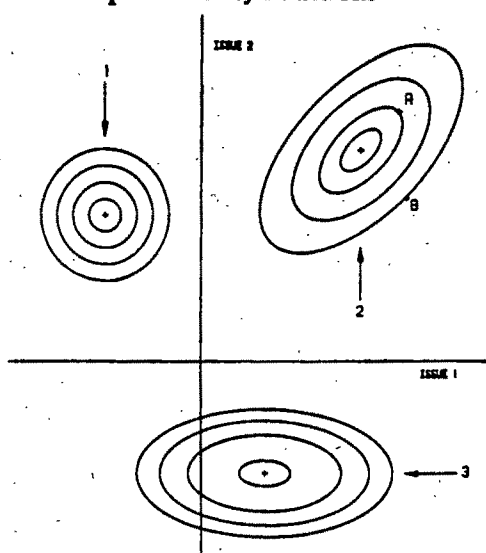
major axis of the elliptical indifference curves seen in the second and third cases. In the first set of indifference curves, the A_i matrix is equal to the identity matrix, producing circular indifference curves and reflecting preferences that view the issues as separable (the outcome of one issue is irrelevant to the desired outcome of the other) and equally salient. The third set of curves reflects the view that the issues are separable but that the issue given on the vertical axis is more salient than the issue given on the horizontal axis (a given change in the vertical direction produces a greater change in utility than the same change in the horizontal direction). The second set of indifference curves represents an evaluation that the issues are equally salient but not separable; the outcome of one issue determines what outcomes are desirable for the other. In this case, outcomes in which the resolution of both issues lies above or below the actor's ideal outcome for that issue, like point A, are preferred to outcomes in which one resolution is above and the other below,

like point B (i.e., the issues are complementary).

The risk attitudes of different actors are captured by the curvature of the utility surfaces, which will be reflected in the spacing of indifference curves with equal drops in utility between each pair of curves. The first set of indifference curves depicts risk-neutral preferences because the curves are equally spaced; equal losses in issue space create equal losses in utility. The second set of curves gives the pattern of a risk-acceptant actor; its utility drops off quickly around its ideal point and then levels off. Consequently, an action providing a chance at its ideal point at the cost of a chance at an outcome far from the ideal point will be preferred to the certainty of a moderate outcome. Risk-averse preferences are mirrored in the third set of indifference curves. Outcomes broadly spaced about the third ideal point are valued relatively equally; then the valuation of outcomes rapidly drops off. As a result of this pattern, moderate compromises will be preferred to gambles. Although the structure of the utility function is complex, its components are simple and yet general.

Now that the stakes of conflict and the reason for conflict have been specified, the means of conflict are needed to complete the basic assumptions of the model. Each coalition will have the choice of many different means of pressing an opposing coalition in order to gain its desired outcome. The available means to coerce an opposing coalition include various applications of military force, economic sanctions, and political penalties.⁵ For each of these different means of coercion, each nation is considered to have a capability score that measures its ability to use each specific means to gain outcomes in the policy space. These means of coercion are ranked in an "escalation ladder" (Kahn, 1965), with the level of coercion increasing with each step up the ladder. The means of coercion are

Figure 1. Three Possible Spatial Utility Functions



ranked, then, from first to last according to the strength of their coercive power, as measured by their ability to seize policy outcomes in the absence of the agreement of the opposition.

Formally stated the preceding assumptions are as follows:

There exists a set of n nations; N ; a compact and convex issue space, Q , with $Q \subseteq R^m$; and a status quo point, $x_{SQ} \in Q$, which is set equal to 0. Each nation, i , has a utility function, u_i , as defined in equation (1), that specifies its preferences for all outcomes in the issue space Q . There also exists a finite, well-ordered set of p coercive means, π .

Note that the issue space is defined as bounded; this prevents, among other possibilities, infinite utilities arising from the utility functions. For convenience, the status quo issue disposition can always be moved to 0 by translation of the axes. The assumptions about the utility functions and the means of coercion have been discussed above.

These assumptions limit and define the basis of the model: what actors we are analyzing, what they are competing for, how they value the outcomes, and what means they have available to gain those outcomes. Now the model's coalitional structure needs to be detailed.

The Contest of Coalitions

Coalitions are central to any n -person game. In simplest terms, a *coalition* is a collection of actors who agree to coordinate their strategies for their mutual benefit.⁶ In terms of the model presented here, a coalition will have to coordinate both the means of coercion employed by the actors and their proposed resolution of the issues. Coordination of the means of coercion does not imply that all members of a coalition adopt identical actions

but rather that they coordinate their actions to implement a common strategy. A coalition will have to agree on one outcome that they will advance as the coalition's position. Coalitions can consist of one actor or even be empty. Formally, we have the following:

DEFINITION 1. A coalition is an ordered pair, $C = (x_c, C)$, such that $x_c \in Q$ and $C \subseteq N$. A nation, i , is a member of the coalition, C (written $i \in C$) iff $i \in C$. A coalition, C , is empty iff $C = \phi$. Two coalitions, C_1 and C_2 , are disjoint iff C_1 and C_2 are disjoint. A coalition, C_1 , contains another coalition, C_2 , iff $C_2 \subseteq C_1$ (written $C_2 \subseteq C_1$).

Unlike conventional n -person game theory, each coalition here is associated with its issue position. Because the payoff the members of a coalition expect to receive depends upon the coalition's ability to sustain its issue position in the face of threats from other coalitions and because that ability will change as the coalition's position changes, we cannot divorce a coalition from its position; the same group of actors advancing a different position is a different coalition.

Disjoint coalitions have no members in common, and one coalition *contains* another if all the members of the second coalition are also members of the first coalition.

Conflict arises from the opposition of coalitions. A complete characterization of a conflict requires the specification of the two competing coalitions and the method of coercion used to resolve the conflict. The current state of a conflict is characterized by the members of each coalition, the position that each coalition espouses, and the method of resolution of the crisis. Each slice of a dispute specifying these five quantities (two coalitions, two coalitional positions, and a means of coercion) will be referred to as a *challenge*. A complete *dispute* will be a series of challenges,

where each successive challenge responds to the previous challenge through escalation, the recruitment of additional members, or compromise. The formal definition of a challenge is as follows:

DEFINITION 2. A challenge is an ordered triple of a pair of disjoint coalitions and a means of coercion, $CH = (C_1, C_2, \pi)$, whose first member (the first coalition) is not empty. The first coalition is referred to as the challenging coalition and the second as the countercoalition. If the countercoalition of a challenge is not empty, then the challenge is opposed.

Furthermore, there exists a function, f , from the set of all challenges to the set of all probability distributions on Q , $f: CH \rightarrow p_Q(x)$ [written $p_{CH}(x)$ for the probability distribution associated with challenge CH], with the following properties:

- a) If CH is unopposed, then $p_{CH}(x) = 1$ for $x = x_{C_1}$, and $p_{CH}(x) = 0$ for all other x ;
- b) If $x \neq tx_{C_1} + (1-t)x_{C_2}$ for some $0 \leq t \leq 1$, then $p_{CH}(x) = 0$; and
- c) If $CH = (C_1, C_2, \pi)$ and $CH^* = (C_1, C_2, \pi^*)$ and $\pi \geq \pi^*$, then $\text{Var}(p_{CH}(x)) \geq \text{Var}(p_{CH^*}(x))$.

There also exists a function g from the set of all challenges to the set of n -tuples with non-negative coefficients, $c = (c_1, \dots, c_i, \dots, c_n)$, that give the costs of the conflict for each actor, $g: CH \rightarrow c$ (written $c_{i,CH}$ for each of the components of the costs associated with a given challenge), with the following properties:

- a) If i is not a member of C_1 or a member of C_2 , then $c_{i,CH} = 0$; and
- b) If $CH = (C_1, C_2, \pi)$ and $CH^* = (C_1, C_2, \pi^*)$ and $\pi \geq \pi^*$, then $c_{i,CH} \geq c_{i,CH^*}$ for $1 \leq i \leq n$.

An actor i 's utility for a challenge CH (written $u_i(CH)$) is

$$u_i(CH) = \int_Q p_{CH}(x) u_i(x) dx - c_{i,CH}. \quad (2)$$

In a given challenge, the two coalitions are labeled as the *challenging coalition* and *countercoalition*, even though the first coalition may not necessarily be challenging the status quo. The f function gives the probability that each different outcome can be imposed by the parties on each other in the absence of a final agreement by the coalitions on the disposition of the issues at hand.⁷ The probability distribution associated with each challenge allows the calculation of likely outcomes for that challenge and depends upon the composition and issue positions of the two coalitions and the means of coercion used to resolve the conflict. This distribution is required to have three theoretically desirable properties. First, if a coalition is unopposed, then it can secure its position with certainty. Second, the set of all possible outcomes of a conflict settled solely by coercion are assumed to lie on the line between the contending coalitions' positions, which will be referred to as the *conflict line*. This assumption captures the notion that the use of coercion creates a zero-sum situation across every issue in the dispute and assumes that overall military success allows a coalition to impose its will across all issues equally.⁸ Third, given identical coalition memberships and positions, the variance of the distribution increases as the means of coercion intensifies, implying that higher levels of coercion produce a greater spread of outcomes and thus more decisive results than lower levels of coercion.

The g function maps each challenge into the costs that each actor faces from the challenge. These costs include the physical destruction that conflict produces, the political costs that the government faces from waging a war, and the economic costs of war production as those costs are perceived by the political leadership. However, they do not include

the costs of changes in the international status quo that are imposed in the resolution of the dispute. The function that specifies the costs must satisfy two constraints: first, any actor not involved in the challenge will incur no costs; second, the costs of all actors cannot decline as the means of coercion increases.

An actor's relative preference for a challenge is given by its expected utility for the consequences of that challenge. The utility attached to each possible outcome is multiplied by that outcome's probability of occurring, and then all of these weighted utilities are summed and the costs of that challenge are subtracted from the total. An actor will prefer one challenge to another if and only if the utility of the former is greater than the utility of the latter.

Conflicts unfold in a sequence of challenges through which the competing coalitions attract support through the addition of members, escalation of the means of coercion when it is to their advantage, and compromise with the opposing coalition when cooperation is more attractive than competition. After a coalition initiates a conflict by making a challenge to the status quo, a coalition opposed to the aims of that coalition may form and attempt to obtain a position of its own or a compromise from the initiating coalition. Of course, this coalition can only form if its formation is preferred to the original challenge by all members of the new coalition. Once there are two opposing coalitions, each will struggle to obtain an advantage over the other coalition. The bargaining between the coalitions will proceed through escalation, recruitment of additional support, and disruption of the other coalition. Each of these bargaining tactics will be reflected by changes in the coalitions' memberships, positions, or adopted means of coercion and will produce a series of shifting challenges that characterize the development of a particular international conflict. For-

malizing this notion of shifting coalitions, we have the following:

DEFINITION 3. One challenge $CH = (C_1, C_2, \pi)$ supercedes another challenge $CH^* = (C_1^*, C_2^*, \pi^*)$ iff one of the following conditions holds:

- a) $\pi = \pi^*$, $C_1^* \subseteq C_1$, and $C_1^* \neq C_1$, for all $i \in C_1 - C_1^*$, $u_i(CH) > u_i(CH^*)$, and for all $i \in C_1^*$, $u_i(CH) \geq u_i(CH^*)$;
- b) $\pi = \pi^*$, $C_2^* \subseteq C_2$, $C_2^* \neq C_2$, for all $i \in C_2 - C_2^*$, $u_i(CH) > u_i(CH^*)$, and for all $i \in C_2^*$, $u_i(CH) \geq u_i(CH^*)$;
- c) $\pi = \pi^*$, $C_1 = C_1^* \cup C_2^*$, and for all $i \in C_1$, $u_i(CH) \geq u_i(CH^*)$;
- d) $\pi > \pi^*$, and for all $i \in C_1$, $u_i(CH) > u_i(CH^*)$.

There are three circumstances under which one challenge can supercede another. The first is when either coalition recruits additional members that find the expanded coalition more attractive than the previous challenge, while the previous members of the expanding coalition value the new challenge at least as much as the previous challenge (conditions a and b above). These new members could be drawn either from the set of nations that are not members of either coalition or from the members of the other coalition.

The second such circumstance is when the coalitions reach a compromise that is at least as attractive to all of their members as the previous challenge (condition c above). In this case, the new challenge will be unopposed, and the issue position of the new challenging coalition will be the compromise between the two coalitions. Including equality in each actor's utility comparison implies that a compromise will be reached if no one objects to it. The carrot of concessions enters the model at this point; presumably, a nation would have to offer concessions on its issue position to get the other coalition to agree to a compromise. Compromises are

considered to be enforced by the means of coercion of the superceded challenge to the extent that, should a compromise break down, the means of coercion of the challenge superceding the compromise will have to be at least as high as the means of coercion of the compromise challenge.

The third circumstance under which one challenge can supercede another is when either coalition wishes to escalate the means of coercion in the conflict (condition d above). All the members of the escalating coalition must find the new, escalated challenge preferable to the previous challenge. Some members of a coalition can choose to drop out of an escalating coalition if the remaining members still find escalation without them preferable to the previous challenge with them. Either the challenging or counter-coalition can escalate the means of coercion, but the escalating coalition is now treated as the challenging coalition of the new challenge.

International conflict, then, is a series of challenges between coalitions in which each successive challenge supercedes the previous challenge.

DEFINITION 4. A dispute is a finite sequence of challenges, $(CH_1, CH_2, \dots, CH_n)$, such that CH_{i+1} supercedes CH_i for $1 \leq i \leq n - 1$.

A dispute is defined as a series of challenges through which the parties to a conflict maneuver for favorable positions by recruiting members to their coalitions, detaching members from the other coalition, and escalating the dispute when such escalation favors them. One challenge in a dispute supercedes another only when it is in the interest of all the members of one coalition to change either the composition of their coalition or their tactics of coercion.

Observed disputes will have to be members of the set of disputes for a given inter-

national situation. However, the set of disputes may be very large for any given situation, and the parties to the conflict may not find all disputes strategically desirable. Different solution concepts rule out strategically unsound disputes, limiting the set of possible disputes. The *core*, for example, is the set of all disputes where there does not exist a challenge that supercedes the final challenge of the dispute. By placing a condition on the final challenge, the notion of a core eliminates some disputes from the set of possible disputes. Other solution concepts, like the *competitive solution* (McKelvey, Ordeshook, and Winer, 1978), can be defined in terms of the characteristics of the final challenge of a dispute. Unfortunately, there is no generally acceptable or experimentally supported solution concept for n -person games.⁹ To demonstrate the type of analysis that can be conducted with the model, the core of the two-nation spatial model will be found. Because only the final challenge of a dispute is needed to judge if the dispute lies in the core, we will simply refer to the *set of core challenges*, with the unstated assumption that the complete core contains all disputes that end in a core challenge.

Before proceeding to the analysis of the two-nation case, three ancillary assumptions about the model will be made. These assumptions were not incorporated in the model formally because they are not essential to its logical structure. They do, however, characterize observed disputes. These assumptions will focus on the probability distribution of the outcomes and on the costs the actors incur for a given challenge. The original structure of the theory places only the weakest constraints on those distributions of outcomes; the ancillary assumptions will flesh out those distributions in an intuitive fashion.

First, the probability distribution of outcomes for a challenge will depend upon only the positions and capabilities of each coalition and the means of coer-

cion used to resolve the conflict. Furthermore, increasing a coalition's capabilities will draw the distribution of outcomes closer to its position. As a coalition grows stronger, it becomes more able to impose its issue position upon the other coalition, and the probability distribution will change to reflect this shift. Formally, this assumption is represented as follows for a coalition C_1 :

$$\int_{x_{C1}} \frac{\partial p(t)}{\partial \text{cap } C_1} dt > 0 \text{ for all } z \in (x_{C1}, x_{C2}). \quad (3)$$

Increasing the capabilities of a coalition shifts the entire probability distribution toward the position of the coalition in the sense that for all possible outcomes z , the chance that the coalition can impose an outcome at least as favorable as z always increases.

The second ancillary assumption is that a coalition's capabilities arise from the contributions of its members to gaining the common goal. Even as it is assumed that nations will contribute all their capabilities to the coalition (eliminating the classic free-rider problem), a coalition's capabilities will not simply be the sum of its members' capabilities for the given means of coercion. A nation's capabilities are extracted from the population of the nation and depend upon the ability of the leadership either to convince or to coerce the population to surrender those resources to the government. Because a nation's utility function reflects the constraints that the populace places on its government's actions, a nation's capabilities in a dispute will depend upon the utility of its coalition's position. As its own coalition's position becomes more attractive to a nation, it should contribute more capabilities to its coalition (all else being equal). Formally, this argument leads to the following assumption for nation i in coalition C_1 :

$$\frac{\partial \text{cap } i}{\partial u_i(x_{C1})} > 0. \quad (4)$$

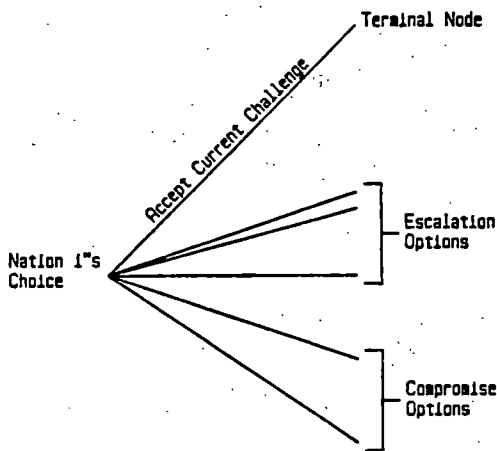
Each coalition's total capabilities for a given means of coercion will be calculated by summing the contributions of all its members.

The third ancillary assumption of the model is that the costs each nation suffers in a given challenge are a function only of the composition of the two coalitions and of the means of coercion of the challenge. Under this assumption, nations cannot reduce the costs they face or increase the costs the other side faces by changing positions.

Using the Model: The Core of the Two-Nation Spatial Model

To demonstrate how disputes can be analyzed using the model, the core of the two-nation model will be found. The core here consists of the set of all challenges that cannot be superseded by any other challenge. Core challenges, both those ending in conflict and those ending in compromise, are stable in the sense that neither nation can benefit by escalating the dispute and that there does not exist a compromise solution that both sides prefer to the final challenge. The existence of core challenges for all two-nation disputes will be demonstrated and the character of core challenges examined. Conclusions about the strategies of each side in a dispute, the likelihood of compromise solutions, and the nature of international disputes will be presented.

With only two nations, the model is greatly simplified. First of all, there are no third parties for either nation to recruit into its coalition; consequently, intra-coalitional dynamics are eliminated. The set of actions the players can take are then reduced to no action (acceptance of the present challenge), escalation of the means of coercion (unless the dispute is already at the highest level of coercion), and offering or accepting a compromise solution to the dispute (in terms of the

Figure 2. A Decision Node in the Extensive Form Game

model, this corresponds to the formation of a coalition of both nations).

Building an extensive form game tree for the two-nation model produces Figure 2 at each decision node, with the players moving alternately. From any challenge, an actor has a choice of acceptance, escalation, or compromise. Accepting the current challenge produces one new branch of the tree and reaches a terminal node if the other nation has accepted the current challenge in its previous move. Because each resulting challenge is different if any coalition in the challenge adopts a different position, there are an infinite number of escalation and compromise moves possible from any challenge. The game terminates when both nations accept the current challenge (which includes both situations of final compromise and situations in which the dispute is deadlocked because neither escalation nor compromise is possible). The two nations alternate moves, producing the complete game tree.

For a given challenge, each side's expected utility gives the attractiveness of that challenge relative to other challenges for that actor. As previously defined, a

nation's expected utility for a challenge is calculated by multiplying the probability of each possible outcome by that nation's utility for that outcome, adding across all possible outcomes, and subtracting that nation's utility for the costs of that challenge. These expected utilities allow us to calculate each nation's preferences over the set of all challenges from the primitive terms of the challenge (position and composition of the coalitions and means of coercion). This comparison can be made even easier because each challenge can be reduced to an equivalent outcome or point for each participant in the dispute (this is established in the first proof in the Appendix). Each challenge then reduces to an equivalent outcome along the line between the two nations' ideal points (equivalent points can be placed on the far side of the other nation's ideal point); for any two challenges, a nation will prefer the challenge with the equivalent point closer to its ideal point.

A nation's equivalent point for a given challenge draws closer to its ideal point as it becomes more risk-acceptant (Morrow, 1985a). As the costs of a given challenge for a nation rise, the equivalent point for the challenge moves away from the nation's ideal point. The effects of escalation on a nation's equivalent point for a challenge (holding each side's position constant) vary depending on whether the nation gains an advantage through escalation and on its risk attitude. If the average outcome (i.e., the mean of the probability distribution) does not shift with the escalation, then escalation will move a nation's equivalent point toward its ideal point if it is risk-acceptant and away from its ideal point if it is risk-averse. This is generally true because escalation increases the riskiness of a challenge, which makes escalation generally attractive to risk-acceptant actors and unattractive to risk-averse actors.

The exact location of the equivalent point of a challenge for an actor will, of course, depend upon the exact composi-

tion of the challenge. Even if we fix the membership of a coalition at one of the two actors, it can still alter the challenge and the resulting equivalent points by changing the positions it advances in the challenge. Consequently, one strategic decision the actors face within a given challenge is their choice of position. At first blush, one would suspect that each actor should advance its ideal point as its position; after all, it prefers its ideal point to all other outcomes, and it will reduce its capabilities if it abandons its ideal point to advance a different resolution of the issues, decreasing its chance of gaining favorable outcomes.

This intuition, however, is completely wrong. In general, both actors will adopt positions that are more extreme than their ideal points (see Appendix for a formal discussion of this idea). Specifically, an actor's optimal position lies outside its own ideal point on the line connecting the two actors' ideal points. According to equation (4), an actor's exaggeration of its position reduces its capabilities because it decreases its utility for its position. Nevertheless, actors exaggerating their positions increase their likelihood of gaining very favorable outcomes by expanding the set of favorable outcomes in the conflict set. This increased possibility of gaining very favorable policy outcomes makes each actor willing to run a higher chance of being stuck with unfavorable outcomes. As should be expected, actors will generally exaggerate their positions more as their willingness to take risks increases.¹⁰

However, at very low levels of coercion, risk-averse actors will actually exaggerate their positions more than risk-acceptant actors will. Because the risk is small at low levels of coercion and because the loss of capabilities is also small for a risk-averse actor, it can justify the gamble of adopting a more extreme position than would be adopted by a comparable risk-acceptant actor. As the dispute escalates, however, the risk-averse

actor will moderate its position towards its ideal point to consolidate its strength, while the risk-acceptant actor will exaggerate its position even more. In fact, there is an equilibrium pair of positions for every possible challenge (see Appendix for a proof). This equilibrium insures that the two sides do not get trapped in a cycle of increasingly exaggerated positions.

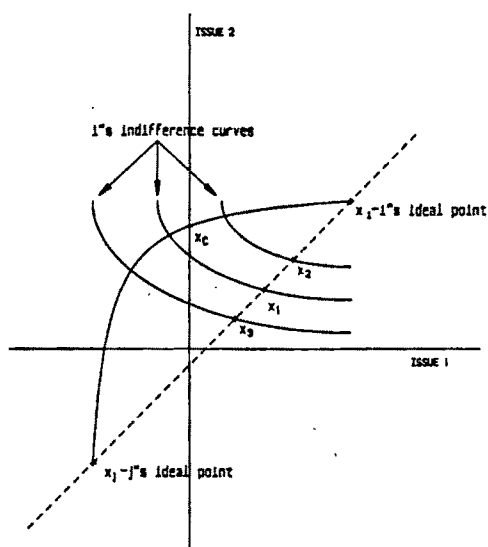
Equivalent points provide a tool for the analysis of conflictual challenges. Central to the analysis of compromise challenges is the Pareto optimal set, the set of all outcomes where any change has to reduce the utility of at least one of the actors. All compromise outcomes should be members of the Pareto optimal set; otherwise, both actors can improve their utility by moving to a point in the Pareto optimal set. Formally, the Pareto optimal set is specified by the following equation:

$$x = [(1-t)x_i A_i + t x_j A_j][(1-t)A_i + t A_j]^{-1}, \quad (5)$$

where x_i and x_j are i 's and j 's ideal points, A_i and A_j are i 's and j 's weighting matrices for the relative salience and separability of the issues, and t goes from 0 to 1 (see Appendix for a derivation). The result is a curve from i 's ideal point to j 's ideal point, the shape of which depends upon each side's weighting of the issues.

If both sides have identical weighting matrices (i.e., they view the issues in exactly the same way), then the Pareto optimal set will be the straight line between their ideal points [set $A_i = A_j = A$ in equation (5)]. When both sides hold the same beliefs about the relative importance of the issues, the set of possible compromises matches the conflict set; that is, the situation is purely conflictual. As the actors begin to differ on the importance of the issues at hand, the Pareto optimal set will bow away from the line between their ideal points. In general then, the Pareto optimal set is a curve between the actors' ideal points, with its

Figure 3. Three Equivalent Points Illustrating Escalation



curvature increasing as the difference in the actors' perceptions of the importance of the issues increases.

To evaluate the attractiveness of one challenge as opposed to another, each actor will compare its equivalent points and will prefer the challenge with the equivalent point closer to its ideal point. The sequence of these equivalent points for a given nation give that nation's preference for escalation. When the sequence of equivalent points approaches its ideal point, a nation will be willing to escalate the dispute; when the sequence moves away from its ideal point, it will not be willing to escalate. Furthermore, the set of compromises that the nation would accept from the current challenge is found by tracing its indifference curve through its equivalent point for the current challenge until that indifference curve intersects the Pareto optimal set; all compromises on the Pareto optimal set between this intersection and the nation's ideal point (inclusive) are at least as good as the current challenge from that nation's perspective.

Figure 3 illustrates this logic; x_1 , x_2 , and x_3 are nation i 's equivalent points for three challenges with escalating means of coercion as the subscript increases. Nation i is willing to escalate from the first means of coercion—say political pressure—to the second—perhaps low-level military action—because x_2 is closer to i 's ideal point, x_i , than is x_1 . On the other hand, nation i is unwilling to escalate the means of coercion up to the third level—say openly waged war—because x_3 lies furthest from its ideal point. The set of compromises that nation i would accept from the challenge contested through low-level military action is found on the Pareto optimal set as given in Figure 3.

The core of this game is the set of all challenges that cannot be superceded; it is the set of all challenges for which neither side prefers to escalate and for which there does not exist a compromise that both sides prefer to the current challenge. The core tells us whether the dispute will end in conflict or compromise. If it ends in conflict, the core tells us at what level of violence and which side should be expected to win; if it ends in compromise, the core tells us what compromises are feasible.

Core solutions always exist for this game. At the highest means of coercion, either a compromise challenge exists that both sides prefer to a conflictual challenge or else the conflictual challenge with the highest means of coercion cannot be superceded. In the former case, the resulting compromise cannot be superceded because further escalation is not possible and because any change in a compromise on the Pareto optimal set reduces at least one nation's utility for the outcome. In the latter case, the dispute will be resolved through a conflict waged with the highest means of coercion (presumably war). In this case, the two sides cannot find a compromise they both prefer to conflict. Thus, there always exists a core challenge with the highest means of coercion.

Core challenges at lower means of coercion can exist if neither side is willing to escalate the dispute further. These disputes can end in either conflict or compromise, depending upon both sides' evaluations of the value they expect to gain through conflict compared with the attractiveness of compromise.

One problem of obvious interest here is predicting the likelihood of final compromise. What conditions make compromise more likely? There are five aspects to the answer. First, as the nations become less willing to take risks, compromise becomes more likely. As a nation becomes less risk-acceptant, its equivalent point for a given challenge moves further away from its ideal point, increasing the range of compromises it will accept. Of course, we should expect this from the definition of risk-aversion; risk-averse actors prefer the certain choice (i.e., compromise) to the risky choice (i.e., conflict).

Second, as the actors' evaluations of the importance of the issues become increasingly divergent, the Pareto optimal set will bend away from the conflict line. The further the Pareto optimal set bends away from the conflict line, the greater will be the range of pairs of the two sides' risk attitudes that are capable of producing a compromise acceptable to both. As the Pareto optimal set bends outward with the growing divergence of the nation's evaluations of the importance of the issues, there will be pairs of nations that will now find an acceptable compromise that could not find one previously. The possibility of compromise, then, becomes more likely as the divergence of the relative salience of the issues for the actors increases. When the actors disagree about the relative importance of the different issues, compromises are reached more easily because the actors can "trade" the issues off to reach an agreeable compromise, with each actor receiving a favor-

able outcome on the issue that is more salient to it. Obviously then, the more issues on the negotiating table, the more likely will be the trading of issues that leads to compromise.¹¹

Third, escalation can make compromises more stable in some situations. Consider Figure 3 again. From x_1 , x_c is an acceptable compromise to the actor because it lies inside the indifference curve through x_1 . However, x_2 is preferable to x_c , implying that the actor would be willing to escalate a compromise challenge of position x_c enforced by the first means of coercion. The same compromise outcome, x_c , cannot be superseded if it is enforced by the third means of coercion because further escalation is no longer possible. In other words, escalating from the first to the third means of coercion has stabilized a previously unstable compromise.

Fourth, as the cost of a challenge increases, so does the likelihood of compromise. Increasing the costs moves each side's equivalent point away from its ideal point, thereby increasing the range of acceptable compromises on the Pareto optimal set for that side.

Fifth and finally, in contrast to one-dimensional spatial models of conflict (Morrow, 1985a), compromise in this model may be possible even when both sides disagree about the likely outcome of fighting. When the two sides disagree about which side would prevail in a conflictual dispute, there is a range of outcomes between the two sides' equivalent points for the challenge, which neither side prefers to a conflict. Nevertheless, there can be compromise outcomes that both sides prefer to a conflict because a compromise provides an opportunity to trade control over preferred issues. The conditions of compromises, then, depend closely upon the value each side attaches to the issues in the dispute, the ability of the sides to add issues to the dispute, and their expectations for the value of conflict.

Conclusion

This paper has presented a spatial model of international conflict. The actors are assumed to behave rationally, and their preferences for the resolution of international issues can be represented by a spatial utility function. Their abilities to employ different means of coercion against one another and the desire to form coalitions to enhance their chances of gaining their desired ends lead to a game form that represents their strategic interaction in the dispute. In their attempts to secure their ends, the competing coalitions are capable of recruiting additional members, escalating the means of coercion, or compromising with each other. The model with two nations was analyzed in detail to discover the core of the resulting game and to demonstrate the links between the primitive terms of the theory—that is, the nations' utilities and the probability distributions attached to the challenges—and the decisions that each side will make during the course of the dispute. Conclusions drawn from the model are that nations will generally exaggerate their positions in a dispute for strategic reasons, that core outcomes always exist for this game, and that compromise is facilitated by trading among the different issues in the dispute.

Because the assumptions of the model are quite general, so are its conclusions. Conclusions about specific disputes would, of course, depend upon the specific issues, utility functions, and probability distributions of that dispute. The functions and distributions are the formal representations of how each party values the issues in the dispute and of what is likely to occur if the dispute cannot be resolved peacefully. Empirically, these functions and distributions could be estimated, leading to a direct test of the theory. However, given the paucity of existing data sets focusing on the role of

international issues in international conflict, such a direct test seems a distant prospect. Another approach to testing the theory would examine evidence about the conclusions of the theory in historical cases and existing statistical data sets.

Beyond just testing the model, other avenues for further research present themselves. Further deductions from the model are an obvious goal; for example, the three-nation model is informally discussed in Morrow (1985b). There are other directions as well. One advantage of a truly general theory of international conflict is its ability to serve as a building block for testing the logical consistency of traditional theories of international relations. For example, there is currently a great deal of interest in macrohistorical theories of war (e.g., Levy, 1985)—theories that examine the broad scope of international history as it is punctuated by general wars. Such theories attempt to explain both the occurrence and consequences of general wars from a variety of perspectives. The model presented here, if it captures a general theory, must give an account of how certain conflicts flare into general wars and why those general wars appear to occur in cycles over time. The assumptions of the different cyclic theories can be rendered in the terms of the model, and then each macrohistorical theory can be logically tested to see if cycles of general wars can be deduced from its assumptions using the model. Furthermore, the model presented here suggests other intermediate variables, such as the role of issues in cycles of general war, that the literature for the most part has ignored. (Mansbach and Vasquez [1981] discuss issue cycles, but they do not explicitly tie them into the idea of the periodicity of general war.) Only the development of truly general theories of international conflict opens up these possibilities, and while the model presented here is not drawn from the only general theory possible, it is one pre-

liminary path towards such a general theory.

Appendix

This appendix contains the proofs of several statements in the body of the paper. The statements of theorems used in these proofs are drawn from Apostol (1974).

Proof of the Existence of Equivalent Points

A nation's utility for a given challenge, CH , is

$$u_i(CH) = \int_Q p_{CH}(x) u_i(x) dx - c_i(CH), \quad (6)$$

where Q is just the line segment connecting the positions of the two contending coalitions; if CH is unopposed, then Q is just the position of the coalition, and $u_i(CH) = u_i(x_{CI})$. By the mean-value theorem for integrals, there exists a point x_{il} such that

$$u_i(x_{il}) = \int_Q p_{CH}(x) u_i(x) dx, \quad (7)$$

Then, because $u_i(x)$ is unbounded on the lower side along the extension of Q , there exists a point, x , with $u_i(x) = u_i(x_{il}) - c_i(CH)$ by the intermediate value theorem.

Proof That Equilibrium Positions Exist for Any Challenge between Two Actors

We first establish that such equilibrium positions must be found on the line through the two nations' ideal points and then that equilibrium positions do exist along that line. First, consider the case when $A_i = I$; this produces circular indifference curves. For any position off this line—say x' —project the line segment from x' to y , the other side's position, onto the line through x_i and y . We will show that i will prefer to adopt the projec-

tion of x' , which we will call $x'P$, as its position over x' by comparing the utility of the challenge that results from i 's adopting x' as its position, CH , to the utility of the challenge in which i adopts $x'P$ as its position, CH^* .

First, for every point on the line segment from x' to y , its projection has a higher utility than it does because the line of projection will be tangent to the indifference curve through the projection of the point. Then we have the following:

$$\begin{aligned} u_i(CH^*) &= \int_y^{x'P} p_{CH^*}(x) u_i(x) dx - c_i(CH^*) \\ &> \int_y^{x'} p_{CH^*}(xP) u_i(x) dx - c_i(CH^*). \quad (8a) \end{aligned}$$

Second, the projection of x' , $x'P$, is closer to x_i than is x' ; consequently, the capabilities of i rise, which in turn increases the probability of outcomes close to x_i . Formally, we compare the expected utility given in the second line of equation (8) to the expected utility with $p_{CH^*}(x)$ substituted for $p_{CH^*}(xP)$:

$$\begin{aligned} &\int_y^{x'} p_{CH^*}(xP) u_i(x) dx - \int_y^{x'} p_{CH^*}(x) u_i(x) dx \\ &= \int_y^{x'} [p_{CH^*}(xP) - p_{CH^*}(x)] u_i(x) dx \\ &= \int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)] u_i(x) dx \\ &\quad + \int_y^z [p_{CH^*}(xP) - p_{CH}(x)] u_i(x) dx \quad (9) \end{aligned}$$

for some $z \in [x', y]$ with $u_i(z) < u_i(x')$. By equation (2), then,

$$\int_z^{x'} [p_{CH^*}(xP) - p_{CH}(x)] dx > 0, \quad (10)$$

and because $\int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)] dx = 0$,

$$\int_y^z [p_{CH^*}(xP) - p_{CH}(x)] dx < 0. \quad (11)$$

Returning to the last line of equation (9), we have the following:

$$\begin{aligned} &\int_z^{x'} [p_{CH^*}(xP) - p_{CH}(x)] u_i(x) dx \\ &\quad + \int_y^z [p_{CH^*}(xP) - p_{CH}(x)] u_i(x) dx \end{aligned}$$

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$$\begin{aligned}
 &> u_i(z) \int_z^{x'} [p_{CH^*}(xP) - p_{CH}(x)] dx \\
 &+ u_i(z) \int_y^z [p_{CH^*}(xP) - p_{CH}(x)] dx \\
 &= u_i(z) \int_y^{x'} [p_{CH^*}(xP) - p_{CH}(x)] dx = 0. \quad (12)
 \end{aligned}$$

Returning to equation (8a), we have the following:

$$\begin{aligned}
 u_i(CH^*) &> \int_y^{x'} p_{CH^*}(xP) u_i(x) dx - c_i(CH^*) \\
 &> \int_y^{x'} p_{CH}(x) u_i(x) dx - c_i(CH^*). \quad (8b)
 \end{aligned}$$

Finally, because the costs each actor suffers from a challenge depend only upon the means of coercion and the composition of the two sides, $c_i(CH^*) = c_i(CH)$.

$$\begin{aligned}
 u_i(CH^*) &> \int_y^{x'} p_{CH}(x) u_i(x) dx - c_i(CH^*) \\
 &= \int_y^{x'} p_{CH}(x) u_i(x) dx - c_i(CH) \\
 &= u_i(CH) \quad (8c)
 \end{aligned}$$

Therefore, any actor with circular indifference curves will prefer $x'P$ to x' . With a general spatial utility function, the linear transformation $A_iPA_i^{-1}$, where P is the projection transformation onto the line through $A_i^{-1}y$ and x_i , produces a preferable position because A_i^{-1} transforms the issue space so that i 's indifference curves are circular, at which point the above reasoning holds.

Actors will, then, only adopt positions along the line through x_i and x_j because the above logic holds for both of them. To demonstrate that equilibrium positions along this line always exist, consider one actor's choice of position. For actor i , fix actor j 's position at x_{C2} and choose x to maximize i 's expected utility:

$$\begin{aligned}
 u_i(CH) &= \int_Q p_{CH}(x) u_i(x) dx - c_i(CH) \\
 &= \int_{y_0}^{x_0} p(x,t) u_i(t) dt - c_i(CH), \quad (13)
 \end{aligned}$$

where x_0 and y_0 are the extreme values of the line through the actors' ideal points

(recall that Q is compact, so x_0 and y_0 exist). The function $p(x,t)$ is the probability that point t will occur if x is i 's chosen position. Differentiate equation (13) and set it equal to 0 to maximize expected utility:

$$0 = \int_{y_0}^{x_0} \frac{\partial p(x,t)}{\partial x} u_i(t) dt. \quad (14)$$

Differentiating $p(x,t)$ with respect to x using the chain rule,

$$\begin{aligned}
 \frac{\partial p(x,t)}{\partial x} &= \left[\frac{\partial p}{\partial cap_i} \right] \left[\frac{\partial cap_i}{\partial x} \right] \\
 &= (x - x_{C2})^{-2} p(x,t). \quad (15)
 \end{aligned}$$

The second term of the above derivative arises to account for the stretching of the available alternatives by moving to a more extreme position. Substituting equation (15) into equation (14), along with some simple rearrangement of terms, produces the following:

$$\begin{aligned}
 (x - x_{C2})^{-2} \int_{y_0}^{x_0} p(x,t) u_i(x) dt \\
 = \left[\frac{\partial cap_i}{\partial x} \right] \int_{y_0}^{x_0} \left[\frac{\partial p}{\partial cap_i} \right] u_i(t) dt. \quad (16)
 \end{aligned}$$

Equation (16) produces the following implicit relationship:

$$\begin{aligned}
 0 &= (x - x_{C2})^2 \left[\frac{\partial cap_i}{\partial x} \right] \\
 &\left[\int_{y_0}^{x_0} \left[\frac{\partial p}{\partial cap_i} \right] u_i(t) dt \right] \\
 &- \int_{y_0}^{x_0} p(x,t) u_i(x) dt. \quad (17)
 \end{aligned}$$

A solution exists to this equation because for $x = x_{C2}$, the right-hand side is positive; for $x = x_0$, it is negative, and because it is continuous, it must have a zero between those two points. Then, by the implicit function theorem, the function that takes x_{C2} into the zero of equation (17) is a continuous function. Then,

by the Brouwer fixed-point theorem, a fixed point in the game of choosing positions exists because each side's choice of position is continuous and the space is compact and convex. Therefore, equilibrium positions exist on the line through the actors' ideal points.

Discussion of the Location of Equilibrium Positions

The left-hand side of equation (16) is negative because u_i is always negative; the second expression on the right-hand side of equation (16) is positive by assumption. Then $\partial \text{cap}_i / \partial x$ will be negative. This can only occur when x is moving away from x_i and x_j , which occurs only when i 's position is on the far side of x_i .

Definition of the Pareto Optimal Set

The Pareto optimal set can be found by maximizing one actor's utility while holding the other's constant. Mathematically, this reduces to the solution of the following constrained maximization problem:

$$\begin{aligned} \text{maximize } F(x, \lambda) = & -(x - x_j)A_j(x - x_j)^T \\ & + \lambda[(x - x_i)A_i(x - x_i)^T - C], \end{aligned} \quad (18)$$

which produces the following Lagrangian:

$$\begin{aligned} A_j(x - x_j)^T - \lambda A_i(x - x_i)^T &= 0 \\ (x - x_i)A_i(x - x_i)^T &= C. \end{aligned} \quad (19)$$

A solution of the above system of equations is the following (to see this, set $\lambda = t/(1 - t)$ for $0 < t < 1$:

$$x = [(1 - t)x_i A_i + t x_j A_j] [(1 - t)A_i + t A_j]^{-1}. \quad (20)$$

Notes

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comments. I would also like to thank the Department of Political Science at Michigan State University for intellectual and material support during the writing of the early drafts of this paper.

1. The type of restriction depends upon the process the group uses to reach decisions and on the set of choices. The best-known example of such restrictions on preferences is the concept of single-peaked preferences in a one-dimensional policy space, which leads to rational decisions under majority rule (Black, 1958). The recent literature on formal group decision theory suggests that coherent group decisions may be induced by structural rules (Shaples, 1979) or be likely to fall within a range of values reflecting the consensus of the group (McKelvey, 1983).

2. The question of the limits of rational theories as a tool to explain decisions in international policies is essentially empirical. Clearly, there are some policies that are not consistent (Allison, 1972). However, there is also good evidence to suggest that broad strategic decisions that are critical to national security are consistent with previous decisions (Bueno de Mesquita, 1981, 1985; Newman, 1983; Petersen, 1983). The question is reduced to the empirical question of which decisions exhibit rational consistency and which do not, and in the absence of a theory that predicts rational decisions in different situations (such as the theory presented here), a test of the rationality of different decisions is impossible.

3. Although there are many similarities between this model and the arguments of Mansbach and Vasquez (1981), such as a shared focus on issues, what I am defining as an issue corresponds to their notion of a stake. I define an *issue* as one specific policy in contention, while they see issues as broad composites of many different *stakes*, which are "objects that are seen as possessing or representing values" (Mansbach and Vasquez, 1981, p. 58).

4. The determination of the relative importance of issues cannot be directly derived from the salience matrix because the scales of the different issues are not known (Davis, Hinich, and Ordeshook, 1970). It may be that 1 unit on one issue is equivalent to 10 units on another issue.

This utility function differs from the spatial utility functions employed in studies of electoral competition in assuming that actors have consistent attitudes towards risk across the range of outcomes. In most spatial models, risk attitudes are represented by an increasing function of the issue distance, where the function induces a reaction to risk across different ranges. Although the typical approach is more general, allowing actors to be risk-acceptant over some ranges of values and risk-averse over others, it is less tractable empirically.

5. Some will complain that I have emphasized the stick of coercion at the expense of the carrot of inducements in international politics. This is not so. In the complete model, enticements to reach agree-

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ments are included in the structure of the negotiation between contending coalitions.

6. Coalitions should not be confused with alliances. An *alliance* is a formal, written agreement between two or more nations to coordinate their policies (generally military) over a long period of time, while a coalition is simply a temporary agreement between nations to coordinate their strategies and some of their policies for the duration of a dispute.

7. Wittman (1979) presents a rational model of war termination that examines the implications of different coercive strategies on the final agreement reached between two parties. He argues that the type of agreement reached depends upon each side's ability to impose costs above the cost of a settlement on the other side. However, some wars (e.g., the 1967 Arab-Israeli war) do not end with a final agreement but instead grind to a halt because one party has achieved its objectives and the other party is no longer able to impose appreciable costs on it to force it to the bargaining table.

8. In general, it will not be the case that coercion is equally effective over all issues. When this is the case, the set of possible outcomes without compromise will be a path between the two coalitions' positions rather than a straight line between them.

9. A promising solution concept for *n*-person games may be found within the concepts of *perfect* or *sequential equilibria* (Kreps and Wilson, 1982; Selten, 1975). Those equilibria are defined for extensive form games and, consequently, are not perfectly matched to traditional *n*-person game theory solutions, which examine only the characteristic function form and not the extensive form of the game.

10. One reader wondered whether some discussion of how rational actors form expectations under limited information (e.g., Kreps and Wilson, 1982) is needed to establish this point about exaggeration of position. Given that the game is played under perfect and complete information, the actors have no problem forming expectations of their opponent's future moves. Regardless of what the other actor does, each actor is better off adopting an exaggerated position; consequently, the question of expectations is moot. The "game" of position selection resembles the classic game of the prisoners' dilemma because each actor's dominant strategy is to exaggerate its position, and such exaggeration creates the possibility of Pareto suboptimal outcomes, such as a final resolution outside both actors' ideal points.

11. This result is an artifact of the two-nation model; with additional nations, adding issues to a dispute may make resolution of the dispute more difficult because it increases the likelihood that uninvolved third parties will intervene in the dispute. It may be the case that some acceptable compromises are not reached in actual disputes because the two sides are unwilling to add the issues needed to com-

plete a compromise, out of fear of provoking the intervention of an uninvolved party.

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LIBERALISM AND WORLD POLITICS

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Building on a growing literature in international political science, I reexamine the traditional liberal claim that governments founded on a respect for individual liberty exercise "restraint" and "peaceful intentions" in their foreign policy. I look at three distinct theoretical traditions of liberalism, attributable to three theorists: Schumpeter, a democratic capitalist whose explanation of liberal pacifism we often invoke; Machiavelli, a classical republican whose glory is an imperialism we often practice; and Kant, a liberal republican whose theory of internationalism best accounts for what we are. Despite the contradictions of liberal pacifism and liberal imperialism, I find, with Kant and other democratic republicans, that liberalism does leave a coherent legacy on foreign affairs. Liberal states are different. They are indeed peaceful. They are also prone to make war. Liberal states have created a separate peace, as Kant argued they would, and have also discovered liberal reasons for aggression, as he feared they might. I conclude by arguing that the differences among liberal pacifism, liberal imperialism, and Kant's internationalism are not arbitrary. They are rooted in differing conceptions of the citizen and the state.

Promoting freedom will produce peace, we have often been told. In a speech before the British Parliament in June of 1982, President Reagan proclaimed that governments founded on a respect for individual liberty exercise "restraint" and "peaceful intentions" in their foreign policy. He then announced a "crusade for freedom" and a "campaign for democratic development" (Reagan, June 9, 1982).

In making these claims the president joined a long list of liberal theorists (and propagandists) and echoed an old argument: the aggressive instincts of authoritarian leaders and totalitarian ruling parties make for war. Liberal states, founded on such individual rights as equality before the law, free speech and other civil liberties, private property, and elected representation are fundamentally against war this argument asserts. When the citizens who bear the burdens of war

elect their governments, wars become impossible. Furthermore, citizens appreciate that the benefits of trade can be enjoyed only under conditions of peace. Thus the very existence of liberal states, such as the U.S., Japan, and our European allies, makes for peace.

Building on a growing literature in international political science, I reexamine the liberal claim President Reagan reiterated for us. I look at three distinct theoretical traditions of liberalism, attributable to three theorists: Schumpeter, a brilliant explicator of the liberal pacifism the president invoked; Machiavelli, a classical republican whose glory is an imperialism we often practice; and Kant.

Despite the contradictions of liberal pacifism and liberal imperialism, I find, with Kant and other liberal republicans, that liberalism does leave a coherent legacy on foreign affairs. Liberal states are

different. They are indeed peaceful, yet they are also prone to make war, as the U.S. and our "freedom fighters" are now doing, not so covertly, against Nicaragua. Liberal states have created a separate peace, as Kant argued they would, and have also discovered liberal reasons for aggression, as he feared they might. I conclude by arguing that the differences among liberal pacifism, liberal imperialism, and Kant's liberal internationalism are not arbitrary but rooted in differing conceptions of the citizen and the state.

Liberal Pacifism

There is no canonical description of liberalism. What we tend to call *liberal* resembles a family portrait of principles and institutions, recognizable by certain characteristics—for example, individual freedom, political participation, private property, and equality of opportunity—that most liberal states share, although none has perfected them all. Joseph Schumpeter clearly fits within this family when he considers the international effects of capitalism and democracy.

Schumpeter's "Sociology of Imperialisms," published in 1919, made a coherent and sustained argument concerning the pacifying (in the sense of nonaggressive) effects of liberal institutions and principles (Schumpeter, 1955; see also Doyle, 1986, pp. 155–59). Unlike some of the earlier liberal theorists who focused on a single feature such as trade (Montesquieu, 1949, vol. 1, bk. 20, chap. 1) or failed to examine critically the arguments they were advancing, Schumpeter saw the interaction of capitalism and democracy as the foundation of liberal pacifism, and he tested his arguments in a sociology of historical imperialisms.

He defines *imperialism* as "an objectless disposition on the part of a state to unlimited forcible expansion"

(Schumpeter, 1955, p. 6). Excluding imperialisms that were mere "catchwords" and those that were "object-ful" (e.g., defensive imperialism), he traces the roots of objectless imperialism to three sources, each an atavism. Modern imperialism, according to Schumpeter, resulted from the combined impact of a "war machine," warlike instincts, and export monopolism.

Once necessary, the war machine later developed a life of its own and took control of a state's foreign policy: "Created by the wars that required it, the machine now created the wars it required" (Schumpeter, 1955, p. 25). Thus, Schumpeter tells us that the army of ancient Egypt, created to drive the Hyksos out of Egypt, took over the state and pursued militaristic imperialism. Like the later armies of the courts of absolutist Europe, it fought wars for the sake of glory and booty, for the sake of warriors and monarchs—wars *gratia* warriors.

A warlike disposition, elsewhere called "instinctual elements of bloody primitivism," is the natural ideology of a war machine. It also exists independently; the Persians, says Schumpeter (1955, pp. 25–32), were a warrior nation from the outset.

Under modern capitalism, export monopolists, the third source of modern imperialism, push for imperialist expansion as a way to expand their closed markets. The absolute monarchies were the last clear-cut imperialisms. Nineteenth-century imperialisms merely represent the vestiges of the imperialisms created by Louis XIV and Catherine the Great. Thus, the export monopolists are an atavism of the absolute monarchies, for they depend completely on the tariffs imposed by the monarchs and their militaristic successors for revenue (Schumpeter, 1955, p. 82–83). Without tariffs, monopolies would be eliminated by foreign competition.

Modern (nineteenth century) imperi-

alism, therefore, rests on an atavistic war machine, militaristic attitudes left over from the days of monarchical wars, and export monopolism, which is nothing more than the economic residue of monarchical finance. In the modern era, imperialists gratify their private interests. From the national perspective, their imperialistic wars are objectless.

Schumpeter's theme now emerges. Capitalism and democracy are forces for peace. Indeed, they are antithetical to imperialism. For Schumpeter, the further development of capitalism and democracy means that imperialism will inevitably disappear. He maintains that capitalism produces an unwarlike disposition; its populace is "democratized, individualized, rationalized" (Schumpeter, 1955, p. 68). The people's energies are daily absorbed in production. The disciplines of industry and the market train people in "economic rationalism"; the instability of industrial life necessitates calculation. Capitalism also "individualizes"; "subjective opportunities" replace the "immutable factors" of traditional, hierarchical society. Rational individuals demand democratic governance.

Democratic capitalism leads to peace. As evidence, Schumpeter claims that throughout the capitalist world an opposition has arisen to "war, expansion, cabinet diplomacy"; that contemporary capitalism is associated with peace parties; and that the industrial worker of capitalism is "vigorously anti-imperialist." In addition, he points out that the capitalist world has developed means of preventing war, such as the Hague Court and that the least feudal, most capitalist society—the United States—has demonstrated the least imperialistic tendencies (Schumpeter, 1955, pp. 95–96). An example of the lack of imperialistic tendencies in the U.S., Schumpeter thought, was our leaving over half of Mexico unconquered in the war of 1846–48.

Schumpeter's explanation for liberal pacifism is quite simple: Only war profiteers and military aristocrats gain from wars. No democracy would pursue a minority interest and tolerate the high costs of imperialism. When free trade prevails, "no class" gains from forcible expansion because

foreign raw materials and food stuffs are as accessible to each nation as though they were in its own territory. Where the cultural backwardness of a region makes normal economic intercourse dependent on colonization it does not matter, assuming free trade, which of the "civilized" nations undertakes the task of colonization. (Schumpeter, 1955, pp. 75–76)

Schumpeter's arguments are difficult to evaluate. In partial tests of quasi-Schumpeterian propositions, Michael Haas (1974, pp. 464–65) discovered a cluster that associates democracy, development, and sustained modernization with peaceful conditions. However, M. Small and J. D. Singer (1976) have discovered that there is no clearly negative correlation between democracy and war in the period 1816–1965—the period that would be central to Schumpeter's argument (see also Wilkenfeld, 1968, Wright, 1942, p. 841).

Later in his career, in *Capitalism, Socialism, and Democracy*, Schumpeter, (1950, pp. 127–28) acknowledged that "almost purely bourgeois commonwealths were often aggressive when it seemed to pay—like the Athenian or the Venetian commonwealths." Yet he stuck to his pacifistic guns, restating the view that capitalist democracy "steadily tells . . . against the use of military force and for peaceful arrangements, even when the balance of pecuniary advantage is clearly on the side of war which, under modern circumstances, is not in general very likely" (Schumpeter, 1950, p. 128).¹ A recent study by R. J. Rummel (1983) of "libertarianism" and international violence is the closest test Schumpeterian pacifism has received. "Free" states (those enjoying

political and economic freedom) were shown to have considerably less conflict at or above the level of economic sanctions than "nonfree" states. The free states, the partly free states (including the democratic socialist countries such as Sweden), and the nonfree states accounted for 24%, 26%, and 61%, respectively, of the international violence during the period examined.

These effects are impressive but not conclusive for the Schumpeterian thesis. The data are limited, in this test, to the period 1976 to 1980. It includes, for example, the Russo-Afghan War, the Vietnamese invasion of Cambodia, China's invasion of Vietnam, and Tanzania's invasion of Uganda but just misses the U.S., quasi-covert intervention in Angola (1975) and our not so covert war against Nicaragua (1981-). More importantly, it excludes the cold war period, with its numerous interventions, and the long history of colonial wars (the Boer War, the Spanish-American War, the Mexican Intervention, etc.) that marked the history of liberal, including democratic capitalist, states (Doyle, 1983b; Chan, 1984; Weede, 1984).

The discrepancy between the warlike history of liberal states and Schumpeter's pacifistic expectations highlights three extreme assumptions. First, his "materialistic monism" leaves little room for noneconomic objectives, whether espoused by states or individuals. Neither glory, nor prestige, nor ideological justification, nor the pure power of ruling shapes policy. These nonmaterial goals leave little room for positive-sum gains, such as the comparative advantages of trade. Second, and relatedly, the same is true for his states. The political life of individuals seems to have been homogenized at the same time as the individuals were "rationalized, individualized, and democratized." Citizens—capitalists and workers, rural and urban—seek material welfare. Schumpeter seems to presume

that ruling makes no difference. He also presumes that no one is prepared to take those measures (such as stirring up foreign quarrels to preserve a domestic ruling coalition) that enhance one's political power, despite detrimental effects on mass welfare. Third, like domestic politics, world politics are homogenized. Materially monistic and democratically capitalist, all states evolve toward free trade and liberty together. Countries differently constituted seem to disappear from Schumpeter's analysis. "Civilized" nations govern "culturally backward" regions. These assumptions are not shared by Machiavelli's theory of liberalism.

Liberal Imperialism

Machiavelli argues, not only that republics are not pacifistic, but that they are the best form of state for imperial expansion. Establishing a republic fit for imperial expansion is, moreover, the best way to guarantee the survival of a state.

Machiavelli's republic is a classical mixed republic. It is not a democracy—which he thought would quickly degenerate into a tyranny—but is characterized by social equality, popular liberty, and political participation (Machiavelli, 1950, bk. 1, chap. 2, p. 112; see also Huliung, 1983, chap. 2; Mansfield, 1970; Pocock, 1975, pp. 198-99; Skinner, 1981, chap. 3). The consuls serve as "kings," the senate as an aristocracy managing the state, and the people in the assembly as the source of strength.

Liberty results from "disunion"—the competition and necessity for compromise required by the division of powers among senate, consuls, and tribunes (the last representing the common people). Liberty also results from the popular veto. The powerful few threaten the rest with tyranny, Machiavelli says, because they seek to dominate. The mass demands not to be dominated, and their

veto thus preserves the liberties of the state (Machiavelli, 1950, bk. 1, chap. 5, p. 122). However, since the people and the rulers have different social characters, the people need to be "managed" by the few to avoid having their recklessness overturn or their fecklessness undermine the ability of the state to expand (Machiavelli, 1950, bk. 1, chap. 53, pp. 249-50). Thus the senate and the consuls plan expansion, consult oracles, and employ religion to manage the resources that the energy of the people supplies.

Strength, and then imperial expansion, results from the way liberty encourages increased population and property, which grow when the citizens know their lives and goods are secure from arbitrary seizure. Free citizens equip large armies and provide soldiers who fight for public glory and the common good because these are, in fact, their own (Machiavelli, 1950, bk. 2, chap. 2, pp. 287-90). If you seek the honor of having your state expand, Machiavelli advises, you should organize it as a free and popular republic like Rome, rather than as an aristocratic republic like Sparta or Venice. Expansion thus calls for a free republic.

"Necessity"—political survival—calls for expansion. If a stable aristocratic republic is forced by foreign conflict "to extend her territory, in such a case we shall see her foundations give way and herself quickly brought to ruin"; if, on the other hand, domestic security prevails, "the continued tranquility would enervate her, or provoke internal dissensions, which together, or either of them separately, will apt to prove her ruin" (Machiavelli, 1950, bk. 1, chap. 6, p. 129). Machiavelli therefore believes it is necessary to take the constitution of Rome, rather than that of Sparta or Venice, as our model.

Hence, this belief leads to liberal imperialism. We are lovers of glory, Machiavelli announces. We seek to rule or, at least, to avoid being oppressed. In

either case, we want more for ourselves and our states than just material welfare (materialistic monism). Because other states with similar aims thereby threaten us, we prepare ourselves for expansion. Because our fellow citizens threaten us if we do not allow them either to satisfy their ambition or to release their political energies through imperial expansion, we expand.

There is considerable historical evidence for liberal imperialism. Machiavelli's (Polybius's) Rome and Thucydides' Athens both were imperial republics in the Machiavellian sense (Thucydides, 1954, bk. 6). The historical record of numerous U.S. interventions in the postwar period supports Machiavelli's argument (Aron, 1973, chaps. 3-4; Barnett, 1968, chap. 11), but the current record of liberal pacifism, weak as it is, calls some of his insights into question. To the extent that the modern populace actually controls (and thus unbalances) the mixed republic, its diffidence may outweigh elite ("senatorial") aggressiveness.

We can conclude either that (1) liberal pacifism has at least taken over with the further development of capitalist democracy, as Schumpeter predicted it would or that (2) the mixed record of liberalism—pacifism and imperialism—indicates that some liberal states are Schumpeterian democracies while others are Machiavellian republics. Before we accept either conclusion, however, we must consider a third apparent regularity of modern world politics.

Liberal Internationalism

Modern liberalism carries with it two legacies. They do not affect liberal states separately, according to whether they are pacifistic or imperialistic, but simultaneously.

The first of these legacies is the pacification of foreign relations among liberal

states.² During the nineteenth century, the United States and Great Britain engaged in nearly continual strife; however, after the Reform Act of 1832 defined actual representation as the formal source of the sovereignty of the British parliament, Britain and the United States negotiated their disputes. They negotiated despite, for example, British grievances during the Civil War against the North's blockade of the South, with which Britain had close economic ties. Despite severe Anglo-French colonial rivalry, liberal France and liberal Britain formed an entente against illiberal Germany before World War I. And from 1914 to 1915, Italy, the liberal member of the Triple Alliance with Germany and Austria, chose not to fulfill its obligations under that treaty to support its allies. Instead, Italy joined in an alliance with Britain and France, which prevented it from having to fight other liberal states and then declared war on Germany and Austria. Despite generations of Anglo-American tension and Britain's wartime restrictions on American trade with Germany, the United States leaned toward Britain and France from 1914 to 1917 before entering World War I on their side.

Beginning in the eighteenth century and slowly growing since then, a zone of peace, which Kant called the "pacific federation" or "pacific union," has begun to be established among liberal societies. More than 40 liberal states currently make up the union. Most are in Europe and North America, but they can be found on every continent, as Appendix 1 indicates.

Here the predictions of liberal pacifists (and President Reagan) are borne out: liberal states do exercise peaceful restraint, and a separate peace exists among them. This separate peace provides a solid foundation for the United States' crucial alliances with the liberal powers, e.g., the North Atlantic Treaty Organization and our Japanese alliance. This foundation appears to be impervious

to the quarrels with our allies that bedeviled the Carter and Reagan administrations. It also offers the promise of a continuing peace among liberal states, and as the number of liberal states increases, it announces the possibility of global peace this side of the grave or world conquest.

Of course, the probability of the outbreak of war in any given year between any two given states is low. The occurrence of a war between any two adjacent states, considered over a long period of time, would be more probable. The apparent absence of war between liberal states, whether adjacent or not, for almost 200 years thus may have significance. Similar claims cannot be made for feudal, fascist, communist, authoritarian, or totalitarian forms of rule (Doyle, 1983a, pp. 222), nor for pluralistic or merely similar societies. More significant perhaps is that when states are forced to decide on which side of an impending world war they will fight, liberal states all wind up on the same side despite the complexity of the paths that take them there. These characteristics do not prove that the peace among liberals is statistically significant nor that liberalism is the sole valid explanation for the peace.³ They do suggest that we consider the possibility that liberals have indeed established a separate peace—but only among themselves.

Liberalism also carries with it a second legacy: international "imprudence" (Hume, 1963, pp. 346–47). Peaceful restraint only seems to work in liberals' relations with other liberals. Liberal states have fought numerous wars with non-liberal states. (For a list of international wars since 1816 see Appendix 2.)

Many of these wars have been defensive and thus prudent by necessity. Liberal states have been attacked and threatened by nonliberal states that do not exercise any special restraint in their dealings with the liberal states.

Authoritarian rulers both stimulate and respond to an international political environment in which conflicts of prestige, interest, and pure fear of what other states might do all lead states toward war. War and conquest have thus characterized the careers of many authoritarian rulers and ruling parties, from Louis XIV and Napoleon to Mussolini's fascists, Hitler's Nazis, and Stalin's communists.

Yet we cannot simply blame warfare on the authoritarians or totalitarians, as many of our more enthusiastic politicians would have us do.⁴ Most wars arise out of calculations and miscalculations of interest, misunderstandings, and mutual suspicions, such as those that characterized the origins of World War I. However, aggression by the liberal state has also characterized a large number of wars. Both France and Britain fought expansionist colonial wars throughout the nineteenth century. The United States fought a similar war with Mexico from 1846 to 1848, waged a war of annihilation against the American Indians, and intervened militarily against sovereign states many times before and after World War II. Liberal states invade weak nonliberal states and display striking distrust in dealings with powerful nonliberal states (Doyle, 1983b).

Neither realist (statist) nor Marxist theory accounts well for these two legacies. While they can account for aspects of certain periods of international stability (Aron, 1968, pp. 151-54; Russett, 1985), neither the logic of the balance of power nor the logic of international hegemony explains the separate peace maintained for more than 150 years among states sharing one particular form of governance—liberal principles and institutions. Balance-of-power theory expects—indeed is premised upon—flexible arrangements of geostrategic rivalry that include preventive war. Hegemonies wax and wane, but the liberal peace holds. Marxist "ultra-imperialists" expect a form

of peaceful rivalry among capitalists, but only liberal capitalists maintain peace. Leninists expect liberal capitalists to be aggressive toward nonliberal states, but they also (and especially) expect them to be imperialistic toward fellow liberal capitalists.

Kant's theory of liberal internationalism helps us understand these two legacies. The importance of Immanuel Kant as a theorist of international ethics has been well appreciated (Armstrong, 1931; Friedrich, 1948; Gallie, 1978, chap. 1; Galston, 1975; Hassner, 1972; Hinsley, 1967, chap. 4; Hoffmann, 1965; Waltz, 1962; Williams, 1983), but Kant also has an important analytical theory of international politics. *Perpetual Peace*, written in 1795 (Kant, 1970, pp. 93-130), helps us understand the interactive nature of international relations. Kant tries to teach us methodologically that we can study neither the systemic relations of states nor the varieties of state behavior in isolation from each other. Substantively, he anticipates for us the ever-widening pacification of a liberal pacific union, explains this pacification, and at the same time suggests why liberal states are not pacific in their relations with nonliberal states. Kant argues that perpetual peace will be guaranteed by the ever-widening acceptance of three "definitive articles" of peace. When all nations have accepted the definitive articles in a metaphorical "treaty" of perpetual peace he asks them to sign, perpetual peace will have been established.

The First Definitive Article requires the civil constitution of the state to be republican. By *republican* Kant means a political society that has solved the problem of combining moral autonomy, individualism, and social order. A private property and market-oriented economy partially addressed that dilemma in the private sphere. The public, or political, sphere was more troubling. His answer was a republic that preserved juridical

freedom—the legal equality of citizens as subjects—on the basis of a representative government with a separation of powers. Juridical freedom is preserved because the morally autonomous individual is by means of representation a self-legislator making laws that apply to all citizens equally, including himself or herself. Tyranny is avoided because the individual is subject to laws he or she does not also administer (Kant, *PP*, pp. 99–102; Riley, 1985, chap. 5).⁵

Liberal republics will progressively establish peace among themselves by means of the pacific federation, or union (*foedus pacificum*), described in Kant's Second Definitive Article. The pacific union will establish peace within a federation of free states and securely maintain the rights of each state. The world will not have achieved the "perpetual peace" that provides the ultimate guarantor of republican freedom until "a late stage and after many unsuccessful attempts" (Kant, *UH*, p. 47). At that time, all nations will have learned the lessons of peace through right conceptions of the appropriate constitution, great and sad experience, and good will. Only then will individuals enjoy perfect republican rights or the full guarantee of a global and just peace. In the meantime, the "pacific federation" of liberal republics—"an enduring and gradually expanding federation likely to prevent war"—brings within it more and more republics—despite republican collapses, backsliding, and disastrous wars—creating an ever-expanding separate peace (Kant, *PP*, p. 105).⁶ Kant emphasizes that

it can be shown that this idea of federalism, extending gradually to encompass all states and thus leading to perpetual peace, is practicable and has objective reality. For if by good fortune one powerful and enlightened nation can form a republic (which is by nature inclined to seek peace), this will provide a focal point for federal association among other states. These will join up with the first one, thus securing the freedom of each state in accordance with the idea of international right, and the whole will gradually

spread further and further by a series of alliances of this kind. (Kant, *PP* p. 104)

The pacific union is not a single peace treaty ending one war, a world state, nor a state of nations. Kant finds the first insufficient. The second and third are impossible or potentially tyrannical. National sovereignty precludes reliable subservience to a state of nations; a world state destroys the civic freedom on which the development of human capacities rests (Kant, *UH*, p. 50). Although Kant obliquely refers to various classical interstate confederations and modern diplomatic congresses, he develops no systematic organizational embodiment of this treaty and presumably does not find institutionalization necessary (Riley, 1983, chap. 5; Schwarz, 1962, p. 77). He appears to have in mind a mutual non-aggression pact, perhaps a collective security agreement, and the cosmopolitan law set forth in the Third Definitive Article.⁷

The Third Definitive Article establishes a cosmopolitan law to operate in conjunction with the pacific union. The cosmopolitan law "shall be limited to conditions of universal hospitality." In this Kant calls for the recognition of the "right of a foreigner not to be treated with hostility when he arrives on someone else's territory." This "does not extend beyond those conditions which make it possible for them [foreigners] to attempt to enter into relations [commerce] with the native inhabitants" (Kant, *PP*, p. 106). Hospitality does not require extending to foreigners either the right to citizenship or the right to settlement, unless the foreign visitors would perish if they were expelled. Foreign conquest and plunder also find no justification under this right. Hospitality does appear to include the right of access and the obligation of maintaining the opportunity for citizens to exchange goods and ideas without imposing the obligation to trade (a voluntary act in all cases under liberal constitutions).

Perpetual peace, for Kant, is an epistemology, a condition for ethical action, and, most importantly, an explanation of how the "mechanical process of nature visibly exhibits the purposive plan of producing concord among men, even against their will and indeed by means of their very discord" (Kant, *PP*, p. 108; *UH*, pp. 44-45). Understanding history requires an epistemological foundation, for without a teleology, such as the promise of perpetual peace, the complexity of history would overwhelm human understanding (Kant, *UH*, pp. 51-53). Perpetual peace, however, is not merely a heuristic device with which to interpret history. It is guaranteed, Kant explains in the "First Addition" to *Perpetual Peace* ("On the Guarantee of Perpetual Peace"), to result from men fulfilling their ethical duty or, failing that, from a hidden plan.⁸ Peace is an ethical duty because it is only under conditions of peace that all men can treat each other as ends, rather than means to an end (Kant, *UH*, p. 50; Murphy, 1970, chap. 3). In order for this duty to be practical, Kant needs, of course, to show that peace is in fact possible. The widespread sentiment of approbation that he saw aroused by the early success of the French revolutionaries showed him that we can indeed be moved by ethical sentiments with a cosmopolitan reach (Kant, *CF*, pp. 181-82; Yovel, 1980, pp. 153-54). This does not mean, however, that perpetual peace is certain ("prophesiable"). Even the scientifically regular course of the planets could be changed by a wayward comet striking them out of orbit. Human freedom requires that we allow for much greater reversals in the course of history. We must, in fact, anticipate the possibility of backsliding and destructive wars—though these will serve to educate nations to the importance of peace (Kant, *UH*, pp. 47-48).

In the end, however, our guarantee of perpetual peace does not rest on ethical conduct. As Kant emphasizes,

we now come to the essential question regarding the prospect of perpetual peace. What does nature do in relation to the end which man's own reason prescribes to him as a duty, i.e. how does nature help to promote his *moral purpose*? And how does nature guarantee that what man *ought* to do by the laws of his freedom (but does not do) will in fact be done through nature's compulsion, without prejudice to the free agency of man? . . . This does not mean that nature imposes on us a *duty* to do it, for duties can only be imposed by practical reason. On the contrary, nature does it herself, whether we are willing or not: *facta volentem ducunt, nolentem tradunt*. (*PP*, p. 112)

The guarantee thus rests, Kant argues, not on the probable behavior of moral angels, but on that of "devils, so long as they possess understanding" (*PP*, p. 112). In explaining the sources of each of the three definitive articles of the perpetual peace, Kant then tells us how we (as free and intelligent devils) could be motivated by fear, force, and calculated advantage to undertake a course of action whose outcome we could reasonably anticipate to be perpetual peace. Yet while it is possible to conceive of the Kantian road to peace in these terms, Kant himself recognizes and argues that social evolution also makes the conditions of moral behavior less onerous and hence more likely (*CF*, pp. 187-89; Kelly, 1969, pp. 106-13). In tracing the effects of both political and moral development, he builds an account of why liberal states do maintain peace among themselves and of how it will (by implication, has) come about that the pacific union will expand. He also explains how these republics would engage in wars with nonrepublics and therefore suffer the "sad experience" of wars that an ethical policy might have avoided.

The first source of the three definitive articles derives from a political evolution—from a constitutional law. Nature (providence) has seen to it that human beings can live in all the regions where they have been driven to settle by wars. (Kant, who once taught geography, reports on the Lapps, the Samoyeds, the Pescheras.)

"Asocial sociability" draws men together to fulfill needs for security and material welfare as it drives them into conflicts over the distribution and control of social products (Kant, *UH*, p. 44-45; *PP*, pp. 110-11). This violent natural evolution tends towards the liberal peace because "asocial sociability" inevitably leads toward republican governments, and republican governments are a source of the liberal peace.

Republican representation and separation of powers are produced because they are the means by which the state is "organized well" to prepare for and meet foreign threats (by unity) and to tame the ambitions of selfish and aggressive individuals (by authority derived from representation, by general laws, and by nondespotic administration) (Kant, *PP*, pp. 112-13). States that are not organized in this fashion fail. Monarchs thus encourage commerce and private property in order to increase national wealth. They cede rights of representation to their subjects in order to strengthen their political support or to obtain willing grants of tax revenue (Hassner, 1972, pp. 583-86).

Kant shows how republics, once established, lead to peaceful relations. He argues that once the aggressive interests of absolutist monarchies are tamed and the habit of respect for individual rights engrained by republican government, wars would appear as the disaster to the people's welfare that he and the other liberals thought them to be. The fundamental reason is this:

If, as is inevitably the case under this constitution, the consent of the citizens is required to decide whether or not war should be declared, it is very natural that they will have a great hesitation in embarking on so dangerous an enterprise. For this would mean calling down on themselves all the miseries of war, such as doing the fighting themselves, supplying the costs of the war from their own resources, painfully making good the ensuing devastation, and, as the crowning evil, having to take upon themselves a burden of debts which will embitter peace itself and which can never be paid off on account of the constant

threat of new wars. But under a constitution where the subject is not a citizen, and which is therefore not republican, it is the simplest thing in the world to go to war. For the head of state is not a fellow citizen, but the owner of the state, and war will not force him to make the slightest sacrifice so far as his banquets, hunts, pleasure palaces and court festivals are concerned. He can thus decide on war, without any significant reason, as a kind of amusement, and unconcernedly leave it to the diplomatic corps (who are always ready for such purposes) to justify the war for the sake of propriety. (Kant, *PP*, p. 100)

Yet these domestic republican restraints do not end war. If they did, liberal states would not be warlike, which is far from the case. They do introduce republican caution—Kant's "hesitation"—in place of monarchical caprice. Liberal wars are only fought for popular, liberal purposes. The historical liberal legacy is laden with popular wars fought to promote freedom, to protect private property, or to support liberal allies against nonliberal enemies. Kant's position is ambiguous. He regards these wars as unjust and warns liberals of their susceptibility to them (Kant, *PP*, p. 106). At the same time, Kant argues that each nation "can and ought to" demand that its neighboring nations enter into the pacific union of liberal states (*PP*, p. 102). Thus to see how the pacific union removes the occasion of wars among liberal states and not wars between liberal and nonliberal states, we need to shift our attention from constitutional law to international law, Kant's second source.

Complementing the constitutional guarantee of caution, international law adds a second source for the definitive articles: a guarantee of respect. The separation of nations that asocial sociability encourages is reinforced by the development of separate languages and religions. These further guarantee a world of separate states—an essential condition needed to avoid a "global, soul-less despotism." Yet, at the same time, they also morally integrate liberal states: "as culture grows and men gradually move towards greater agreement over their

principles, they lead to mutual understanding and peace" (Kant, *PP*, p. 114). As republics emerge (the first source) and as culture progresses, an understanding of the legitimate rights of all citizens and of all republics comes into play; and this, now that caution characterizes policy, sets up the moral foundations for the liberal peace. Correspondingly, international law highlights the importance of Kantian publicity. Domestically, publicity helps ensure that the officials of republics act according to the principles they profess to hold just and according to the interests of the electors they claim to represent. Internationally, free speech and the effective communication of accurate conceptions of the political life of foreign peoples is essential to establishing and preserving the understanding on which the guarantee of respect depends. Domestically just republics, which rest on consent, then presume foreign republics also to be consensual, just, and therefore deserving of accommodation. The experience of cooperation helps engender further cooperative behavior when the consequences of state policy are unclear but (potentially) mutually beneficial. At the same time, liberal states assume that nonliberal states, which do not rest on free consent, are not just. Because nonliberal governments are in a state of aggression with their own people, their foreign relations become for liberal governments deeply suspect. In short, fellow liberals benefit from a presumption of amity; nonliberals suffer from a presumption of enmity. Both presumptions may be accurate; each, however, may also be self-confirming.

Lastly, cosmopolitan law adds material incentives to moral commitments. The cosmopolitan right to hospitality permits the "spirit of commerce" sooner or later to take hold of every nation, thus impelling states to promote peace and to try to avert war. Liberal economic theory holds that these cosmopolitan ties derive from a

cooperative international division of labor and free trade according to comparative advantage. Each economy is said to be better off than it would have been under autarky; each thus acquires an incentive to avoid policies that would lead the other to break these economic ties. Because keeping open markets rests upon the assumption that the next set of transactions will also be determined by prices rather than coercion, a sense of mutual security is vital to avoid security-motivated searches for economic autarky. Thus, avoiding a challenge to another liberal state's security or even enhancing each other's security by means of alliance naturally follows economic interdependence.

A further cosmopolitan source of liberal peace is the international market's removal of difficult decisions of production and distribution from the direct sphere of state policy. A foreign state thus does not appear directly responsible for these outcomes, and states can stand aside from, and to some degree above, these contentious market rivalries and be ready to step in to resolve crises. The interdependence of commerce and the international contacts of state officials help create crosscutting transnational ties that serve as lobbies for mutual accommodation. According to modern liberal scholars, international financiers and transnational and transgovernmental organizations create interests in favor of accommodation. Moreover, their variety has ensured that no single conflict sours an entire relationship by setting off a spiral of reciprocated retaliation (Brzezinski and Huntington, 1963, chap. 9; Keohane and Nye, 1977, chap. 7; Neustadt, 1970; Polanyi, 1944, chaps. 1-2). Conversely, a sense of suspicion, such as that characterizing relations between liberal and nonliberal governments, can lead to restrictions on the range of contacts between societies, and this can increase the prospect that a single conflict will deter-

mine an entire relationship.

No single constitutional, international, or cosmopolitan source is alone sufficient, but together (and only together) they plausibly connect the characteristics of liberal polities and economies with sustained liberal peace. Alliances founded on mutual strategic interest among liberal and nonliberal states have been broken; economic ties between liberal and nonliberal states have proven fragile; but the political bonds of liberal rights and interests have proven a remarkably firm foundation for mutual nonaggression. A separate peace exists among liberal states.

In their relations with nonliberal states, however, liberal states have not escaped from the insecurity caused by anarchy in the world political system considered as a whole. Moreover, the very constitutional restraint, international respect for individual rights, and shared commercial interests that establish grounds for peace among liberal states establish grounds for additional conflict in relations between liberal and nonliberal societies.

Conclusion

Kant's liberal internationalism, Machiavelli's liberal imperialism, and Schumpeter's liberal pacifism rest on fundamentally different views of the nature of the human being, the state, and international relations.⁹ Schumpeter's humans are rationalized, individualized, and democratized. They are also homogenized, pursuing material interests "monistically." Because their material interests lie in peaceful trade, they and the democratic state that these fellow citizens control are pacifistic. Machiavelli's citizens are splendidly diverse in their goals but fundamentally unequal in them as well, seeking to rule or fearing being dominated. Extending the rule of the dominant elite or avoiding the political collapse of their state, each calls for imperial expansion.

Kant's citizens, too, are diverse in their goals and individualized and rationalized, but most importantly, they are capable of appreciating the moral equality of all individuals and of treating other individuals as ends rather than as means. The Kantian state thus is governed publicly according to law, as a republic. Kant's is the state that solves the problem of governing individualized equals, whether they are the "rational devils" he says we often find ourselves to be or the ethical agents we can and should become. Republics tell us that

in order to organize a group of rational beings who together require universal laws for their survival, but of whom each separate individual is secretly inclined to exempt himself from them, the constitution must be so designed so that, although the citizens are opposed to one another in their private attitudes, these opposing views may inhibit one another in such a way that the public conduct of the citizens will be the same as if they did not have such evil attitudes. (Kant, *PP*, p. 113)

Unlike Machiavelli's republics, Kant's republics are capable of achieving peace among themselves because they exercise democratic caution and are capable of appreciating the international rights of foreign republics. These international rights of republics derive from the representation of foreign individuals, who are our moral equals. Unlike Schumpeter's capitalist democracies, Kant's republics—including our own—remain in a state of war with nonrepublics. Liberal republics see themselves as threatened by aggression from nonrepublics that are not constrained by representation. Even though wars often cost more than the economic return they generate, liberal republics also are prepared to protect and promote—sometimes forcibly—democracy, private property, and the rights of individuals overseas against nonrepublics, which, because they do not authentically represent the rights of individuals, have no rights to noninterference. These wars may liberate oppressed individuals

overseas; they also can generate enormous suffering.

Preserving the legacy of the liberal peace without succumbing to the legacy of liberal imprudence is both a moral and a strategic challenge. The bipolar stability of the international system, and the near certainty of mutual devastation resulting from a nuclear war between the superpowers, have created a "crystal ball effect" that has helped to constrain the tendency toward miscalculation present at the outbreak of so many wars in the past (Carnesale, Doty, Hoffmann, Huntington, Nye, and Sagan, 1983, p. 44; Waltz, 1964). However, this "nuclear peace" appears to be limited to the superpowers. It has not curbed military interventions in the Third World. Moreover, it is subject to a desperate technological race designed to overcome its constraints and to crises that have pushed even the superpowers to the brink of war. We must still reckon with the war fevers and moods of appeasement that have almost alternately swept liberal democracies.

Yet restraining liberal imprudence, whether aggressive or passive, may not be possible without threatening liberal pacification. Improving the strategic acumen of our foreign policy calls for in-

roducing steadier strategic calculations of the national interest in the long run and more flexible responses to changes in the international political environment. Constraining the indiscriminate meddling of our foreign interventions calls for a deeper appreciation of the "particularism of history, culture, and membership" (Walzer, 1983, p. 5), but both the improvement in strategy and the constraint on intervention seem, in turn, to require an executive freed from the restraints of a representative legislature in the management of foreign policy and a political culture indifferent to the universal rights of individuals. These conditions, in their turn, could break the chain of constitutional guarantees, the respect for representative government, and the web of transnational contact that have sustained the pacific union of liberal states.

Perpetual peace, Kant says, is the end point of the hard journey his republics will take. The promise of perpetual peace, the violent lessons of war, and the experience of a partial peace are proof of the need for and the possibility of world peace. They are also the grounds for moral citizens and statesmen to assume the duty of striving for peace.

Appendix 1. Liberal Regimes and the Pacific Union, 1700-1982

Period	Period	Period
18th Century	1900-1945 (cont.)	1945- (cont.)
Swiss Cantons ^a	Italy, -1922	Costa Rica, -1948; 1953-
French Republic, 1790-1795	Belgium, -1940	Iceland, 1944-
United States, ^a 1776-	Netherlands, -1940	France, 1945-
Total = 3	Argentina, -1943	Denmark, 1945
	France, -1940	Norway, 1945
1800-1850	Chile, -1924, 1932-	Austria, 1945-
Swiss Confederation	Australia, 1901	Brazil, 1945-1954; 1955-1964
United States	Norway, 1905-1940	Belgium, 1946-
France, 1830-1849	New Zealand, 1907-	Luxemburg, 1946-
Belgium, 1830-	Colombia, 1910-1949	Netherlands, 1946-
Great Britain, 1832-	Denmark, 1914-1940	Italy, 1946-
Netherlands, 1848-	Poland, 1917-1935	Philippines, 1946-1972
Piedmont, 1848-	Latvia, 1922-1934	India, 1947-1975, 1977-
Denmark, 1849-	Germany, 1918-1932	Sri Lanka, 1948-1961; 1963-1971;
Total = 8	Austria, 1918-1934	1978-
	Estonia, 1919-1934	Ecuador, 1948-1963; 1979-
1850-1900	Finland, 1919-	Israel, 1949-
Switzerland	Uruguay, 1919-	West Germany, 1949-
United States	Costa Rica, 1919-	Greece, 1950-1967; 1975-
Belgium	Czechoslovakia, 1920-1939	Peru, 1950-1962; 1963-1968; 1980-
Great Britain	Ireland, 1920-	El Salvador, 1950-1961
Netherlands	Mexico, 1928-	Turkey, 1950-1960; 1966-1971
Piedmont, -1861	Lebanon, 1944-	Japan, 1951-
Italy, 1861-	Total = 29	Bolivia, 1956-1969; 1982-
Denmark, -1866		Colombia, 1958-
Sweden, 1864-	1945- ^b	Venezuela, 1959-
Greece, 1864-	Switzerland	Nigeria, 1961-1964; 1979-1984
Canada, 1867-	United States	Jamaica, 1962-
France, 1871-	Great Britain	Trinidad and Tobago, 1962-
Argentina, 1880-	Sweden	Senegal, 1963-
Chile, 1891-	Canada	Malaysia, 1963-
Total = 13	Australia	Botswana, 1966-
	New Zealand	Singapore, 1965-
1900-1945	Finland	Portugal, 1976-
Switzerland	Ireland	Spain, 1978-
United States	Mexico	Dominican Republic, 1978-
Great Britain	Uruguay, -1973	Honduras, 1981-
Sweden	Chile, -1973	Papua New Guinea, 1982-
Canada	Lebanon, -1975	Total = 50
Greece, -1911; 1928-1936		

Note: I have drawn up this approximate list of "Liberal Regimes" according to the four institutions Kant described as essential: market and private property economies; polities that are externally sovereign; citizens who possess juridical rights; and "republican" (whether republican or parliamentary monarchy), representative government. This latter includes the requirement that the legislative branch have an effective role in public policy and be formally and competitively (either inter- or intra-party) elected. Furthermore, I have taken into account whether male suffrage is wide (i.e., 30%) or, as Kant (*MM*, p. 139) would have had it, open by "achievement" to inhabitants of the national or metropolitan territory (e.g., to poll-tax payers or householders). This list of liberal regimes is thus more inclusive than a list of democratic regimes, or polyarchies (Powell, 1982, p. 5). Other conditions taken into account here are that female suffrage is granted within a generation of its being demanded by an extensive female suffrage movement and that representative government is internally sovereign (e.g., including, and especially over military and foreign affairs) as well as stable (in existence for at least three years). Sources for these data are Banks and Overstreet (1983), Gastil (1985), *The Europa Yearbook*, 1985 (1985), Langer (1968), U.K. Foreign and Commonwealth Office (1980), and U.S.

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Department of State (1981). Finally, these lists exclude ancient and medieval "republics," since none appears to fit Kant's commitment to liberal individualism (Holmes, 1979).

^aThere are domestic variations within these liberal regimes: Switzerland was liberal only in certain cantons; the United States was liberal only north of the Mason-Dixon line until 1865, when it became liberal throughout.

^bSelected list, excludes liberal regimes with populations less than one million. These include all states categorized as "free" by Gastil and those "partly free" (four-fifths or more free) states with a more pronounced capitalist orientation.

Appendix 2. International Wars Listed Chronologically

British-Maharattan (1817-1818)	Pacific (1879-1883)
Greek (1821-1828)	British-Zulu (1879)
Franco-Spanish (1823)	Franco-Indochinese (1882-1884)
First Anglo-Burmese (1823-1826)	Mahdist (1882-1885)
Javanese (1825-1830)	Sino-French (1884-1885)
Russo-Persian (1826-1828)	Central American (1885)
Russo-Turkish (1828-1829)	Serbo-Bulgarian (1885)
First Polish (1831)	Sino-Japanese (1894-1895)
First Syrian (1831-1832)	Franco-Madagascan (1894-1895)
Texas (1835-1836)	Cuban (1895-1898)
First British-Afghan (1838-1842)	Italo-Ethipian (1895-1896)
Second Syrian (1839-1940)	First Philippine (1896-1898)
Franco-Algerian (1839-1847)	Greco-Turkish (1897)
Peruvian-Bolivian (1841)	Spanish-American (1898)
First British-Sikh (1845-1846)	Second Philippine (1899-1902)
Mexican-American (1846-1848)	Boer (1899-1902)
Austro-Sardinian (1848-1849)	Boxer Rebellion (1900)
First Schleswig-Holstein (1848-1849)	Ilinden (1903)
Hungarian (1848-1849)	Russo-Japanese (1904-1905)
Second British-Sikh (1848-1849)	Central American (1906)
Roman Republic (1849)	Central American (1907)
La Plata (1851-1852)	Spanish-Moroccan (1909-1910)
First Turco-Montenegrin (1852-1853)	Italo-Turkish (1911-1912)
Crimean (1853-1856)	First Balkan (1912-1913)
Anglo-Persian (1856-1857)	Second Balkan (1913)
Sepoy (1857-1859)	World War I (1914-1918)
Second Turco-Montenegrin (1858-1859)	Russian Nationalities (1917-1921)
Italian Unification (1859)	Russo-Polish (1919-1920)
Spanish-Moroccan (1859-1860)	Hungarian-Allies (1919)
Italo-Roman (1860)	Greco-Turkish (1919-1922)
Italo-Sicilian (1860-1861)	Riffian (1921-1926)
Franco-Mexican (1862-1867)	Druze (1925-1927)
Ecuadorian-Colombian (1863)	Sino-Soviet (1929)
Second Polish (1863-1864)	Manchurian (1931-1933)
Spanish-Santo Dominican (1863-1865)	Chaco (1932-1935)
Second Schleswig-Holstein (1864)	Italo-Ethiopian (1935-1936)
Lopez (1864-1870)	Sino-Japanese (1937-1941)
Spanish-Chilean (1865-1866)	Russo-Hungarian (1956)
Seven Weeks (1866)	Sinai (1956)
Ten Years (1868-1878)	Tibetan (1956-1959)
Franco-Prussian (1870-1871)	Sino-Indian (1962)
Dutch-Achinese (1873-1878)	Vietnamese (1965-1975)
Balkan (1875-1877)	Second Kashmir (1965)
Russo-Turkish (1877-1878)	Six Day (1967)
Bosnian (1878)	Israeli-Egyptian (1969-1970)
Second British-Afghan (1878-1880)	Football (1969)

Changkufeng (1938)	Bangladesh (1971)
Nomohan (1939)	Philippine-MNLF (1972-)
World War II (1939-1945)	Yom Kippur (1973)
Russo-Finnish (1939-1940)	Turco-Cypriot (1974)
Franco-Thai (1940-1941)	Ethiopian-Eritrean (1974-)
Indonesian (1945-1946)	Vietnamese-Cambodian (1975-)
Indochinese (1945-1954)	Timor (1975-)
Madagascar (1947-1948)	Saharan (1975-)
First Kashmir (1947-1949)	Ogaden (1976-)
Palestine (1948-1949)	Ugandan-Tanzanian (1978-1979)
Hyderabad (1948)	Sino-Vietnamese (1979)
Korean (1950-1953)	Russo-Afghan (1979-)
Algerian (1954-1962)	Iran-Iraqi (1980-)

Note: This table is taken from Melvin Small and J. David Singer (1982, pp. 79-80). This is a partial list of international wars fought between 1816 and 1980. In Appendices A and B, Small and Singer identify a total of 575 wars during this period, but approximately 159 of them appear to be largely domestic, or civil wars.

This list excludes covert interventions, some of which have been directed by liberal regimes against other liberal regimes—for example, the United States' effort to destabilize the Chilean election and Allende's government. Nonetheless, it is significant that such interventions are not pursued publicly as acknowledged policy. The covert destabilization campaign against Chile is recounted by the Senate Select Committee to Study Governmental Operations with Respect to Intelligence Activities (1975, *Covert Action in Chile, 1963-73*).

Following the argument of this article, this list also excludes civil wars. Civil wars differ from international wars, not in the ferocity of combat, but in the issues that engender them. Two nations that could abide one another as independent neighbors separated by a border might well be the fiercest of enemies if forced to live together in one state, jointly deciding how to raise and spend taxes, choose leaders, and legislate fundamental questions of value. Notwithstanding these differences, no civil wars that I recall upset the argument of liberal pacification.

Notes

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1. He notes that testing this proposition is likely to be very difficult, requiring "detailed historical analysis." However, the bourgeois attitude toward the military, the spirit and manner by which bourgeois societies wage war, and the readiness with which they submit to military rule during a prolonged war are "conclusive in themselves" (Schumpeter, 1950, p. 129).

2. Clarence Streit (1938, pp. 88, 90-92) seems to have been the first to point out (in contemporary

foreign relations) the empirical tendency of democracies to maintain peace among themselves, and he made this the foundation of his proposal for a (non-Kantian) federal union of the 15 leading democracies of the 1930s. In a very interesting book, Ferdinand Hermens (1944) explored some of the policy implications of Streit's analysis. D. V. Babst (1972, pp. 55-58) performed a quantitative study of this phenomenon of "democratic peace," and R. J. Rummel (1983) did a similar study of "libertarianism" (in the sense of *laissez faire*) focusing on the postwar period that drew on an unpublished study (Project No. 48) noted in Appendix 1 of his *Understanding Conflict and War* (1979, p. 386). I use the term *liberal* in a wider, Kantian sense in my discussion of this issue (Doyle, 1983a). In that essay, I survey the period from 1790 to the present and find no war among liberal states.

3. Babst (1972) did make a preliminary test of the significance of the distribution of alliance partners in World War I. He found that the possibility that the actual distribution of alliance partners could have occurred by chance was less than 1% (Babst, 1972, p. 56). However, this assumes that there was an equal possibility that any two nations could have gone to war with each other, and this is a strong assumption. Rummel (1983) has a further discussion

of the issue of statistical significance as it applies to his libertarian thesis.

4. There are serious studies showing that Marxist regimes have higher military spending per capita than non-Marxist regimes (Payne, n.d.), but this should not be interpreted as a sign of the inherent aggressiveness of authoritarian or totalitarian governments or of the inherent and global peacefulness of liberal regimes. Marxist regimes, in particular, represent a minority in the current international system; they are strategically encircled, and due to their lack of domestic legitimacy, they might be said to "suffer" the twin burden of needing defenses against both external and internal enemies. Andreski (1980), moreover, argues that (purely) military dictatorships, due to their domestic fragility, have little incentive to engage in foreign military adventures. According to Walter Clemens (1982, pp. 117-18), the United States intervened in the Third World more than twice as often during the period 1946-1976 as the Soviet Union did in 1946-79. Relatedly, Posen and VanEvera (1980, p. 105; 1983, pp. 86-89) found that the United States devoted one quarter and the Soviet Union one tenth of their defense budgets to forces designed for Third World interventions (where responding to perceived threats would presumably have a less than purely defensive character).

5. All citations from Kant are from *Kant's Political Writings* (Kant, 1970), the H. B. Nisbet translation edited by Hans Reiss. The works discussed and the abbreviations by which they are identified in the text are as follows:

- PP *Perpetual Peace* (1795)
- UH *The Idea for a Universal History with a Cosmopolitan Purpose* (1784)
- CF *The Contest of Faculties* (1798)
- MM *The Metaphysics of Morals* (1797)

6. I think Kant meant that the peace would be established among liberal regimes and would expand by ordinary political and legal means as new liberal regimes appeared. By a process of gradual extension the peace would become global and then perpetual; the occasion for wars with nonliberals would disappear as nonliberal regimes disappeared.

7. Kant's *foedus pacificum* is thus neither a *pactum pacis* (a single peace treaty) nor a *civitas gentium* (a world state). He appears to have anticipated something like a less formally institutionalized League of Nations or United Nations. One could argue that in practice, these two institutions worked for liberal states and only for liberal states, but no specifically liberal "pacific union" was institutionalized. Instead, liberal states have behaved for the past 180 years as if such a Kantian pacific union and treaty of perpetual peace had been signed.

8. In the *Metaphysics of Morals* (the *Rechtslehre*) Kant seems to write as if perpetual peace is only an epistemological device and, while an ethical duty, is

empirically merely a "pious hope" (MM, pp. 164-75)—though even here he finds that the pacific union is not "impracticable" (MM, p. 171). In the *Universal History* (UH), Kant writes as if the brute force of physical nature drives men toward inevitable peace. Yovel (1980, pp. 168 ff.) argues that from a post-critical (post-*Critique of Judgment*) perspective, *Perpetual Peace* reconciles the two views of history. "Nature" is human-created nature (culture or civilization). Perpetual peace is the "*a priori* of the *a posteriori*"—a critical perspective that then enables us to discern causal, probabilistic patterns in history. Law and the "political technology" of republican constitutionalism are separate from ethical development, but both interdependently lead to perpetual peace—the first through force, fear, and self-interest; the second through progressive enlightenment—and both together lead to perpetual peace through the widening of the circumstances in which engaging in right conduct poses smaller and smaller burdens.

9. For a comparative discussion of the political foundations of Kant's ideas, see Shklar (1984, pp. 232-38).

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ORGANIZING GROUPS FOR COLLECTIVE ACTION

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How can the beneficiaries of collective action be persuaded to contribute the resources (time, energy, money) necessary for the effort to succeed? Rational and selfish players will recognize they can free ride on the successful contributions of others. If the effort is not successful, they will lose a contribution—and be “suckered.” Other than relying on altruism, organizers of the group effort can modify incentives so that players are more prepared to contribute. Laboratory experiments offer one way of assessing the effectiveness of various such modifications; we conducted such tests to see how well contributing is promoted by (1) assuring contributors that they will not lose if the group effort fails (a “money-back guarantee”) and (2) enforcing contributions if it succeeds (“fair share”). We expect the latter to be more successful because it is “stable,” unlike the former, whose success can be undermined by expectations of that success. Three experimental replications demonstrate that the money-back guarantee is no more successful than a standard dilemma, but fair-share requirements increase contributing significantly over that base. Analysis of subjects’ expectations about others’ behavior offers some support to the hypothesized process undermining the money-back guarantee, but motivational factors must also be taken into account for a full explanation.

The problem of organizing groups for collective action (whether political or otherwise) has, since Olson (1965), been well understood: at least when the group is large, there is a clear disincentive for the potential beneficiaries to contribute the time, money, or other resources necessary for the group effort to be successful, even if all want that effort to be successful. Why should I contribute when there is only a trivial chance that my contribution will make a

critical difference and when the only other possibilities are that the group effort will be realized without my contribution (in which case I can “free ride”) and that it will not be realized if I do contribute (in which case I will be “suckered”)?

In more formal terms, the problem is a *social dilemma* (Dawes, 1980): it involves a dominant incentive (do not contribute) associated with a suboptimal equilibrium (the group effort fails). If individuals follow their self-interest, groups confront-

ing such incentives will not attain objectives that all members want.

In natural situations, of course, individuals do not always appear to follow their self-interest, and this impression is supported by experimental studies of social dilemma behavior in which it is possible to specify payoffs with confidence; some incidence of cooperation is normal in such experiments, much more if a period of group discussion is permitted (see, for example, Caldwell, 1976; Dawes, McTavish, and Shaklee, 1977; Edney and Harper, 1978; Jerdee and Rosen, 1974; Orbell, Schwartz-Shea, and Simmons, 1984; Rapoport, 1974; van de Kragt, Orbell, and Dawes, with Braver and Wilson, 1986). To the extent people do not respond to self-interest in these games, the pessimistic prediction from a priori analysis is not borne out, and the side payments Olson discusses (whether "threats" or "bribes") are not required.

In spite of these data—indeed, because the incidence of cooperation (in one-shot games) in the absence of discussion is, characteristically, low—good sense dictates that we search for institutional ways of solving social dilemmas; that is, that we restructure incentives so that selfish individuals are led by consideration of their private interests to contribute to the common interest.¹ Such a restructuring is in the spirit of Adam Smith's (1976) "unseen hand" and of the derived modern arguments in favor of fully specified private property rights and the market as a mechanism for making social decisions. It is also in the spirit of America's founding fathers and of contemporary efforts to design institutions for collective decision making so that selfish individuals playing political games are led to act in the common interest.

Restructuring incentives is what we investigate in the present article. We offer an empirical study of the relative effectiveness of two ways of modifying incentives in step-level dilemmas: (1) ensuring

players that they will not lose a contribution if the group effort fails and (2) enforcing a contribution (requiring a "fair share") in the event the group effort succeeds.

The *money back guarantee* device was used by a group of state system faculty members (The Association of Oregon Faculties) wishing to raise money in 1979 to hire a lobbyist at the state legislature. The desired lobbyist—a public good for all faculty members because any pay increases he produced would go to all faculty in the system, not just to those who contributed—required an annual retainer of \$30,000 for his work.² The Association asked all faculty in the state for contributions, suggesting \$36, \$60, or \$84, depending on salary. The request contained an explicit promise that all money would be returned if less than the \$30,000 was raised. The solicitation was successful.

The logic of *enforced contribution* is often present in efforts by apartment dwellers to resist developers who wish to convert their apartment building into a condominium. The developers offer to sell the units at a reduced rate to anyone wishing to vote for conversion prior to a specified deadline. One "contributes" to the apartment dwellers' effort, then, by withstanding the offer. If the effort fails and the conversion proceeds, those who withstood the offer are out of pocket to the extent of the reduced offer; they have to pay the higher rate. However, it is not possible for an apartment dweller to free ride on the restraint of others because if a sufficient number withstand the offer, the conversion won't occur, and the people who voted "for" won't get the private benefit from the sale. Similar logic can exist in union elections in which the choice is between supporting or not supporting an organizing effort and where, if the vote succeeds, "fair share" provisions exist and all members of a bargaining unit must pay their fees.

In natural circumstances, a wide range of uncontrolled factors can influence outcomes one way or the other, and we cannot say—short of the most detailed study—whether success of the group effort hinges on the presence (or absence) of one or the other of these devices. However, the comparative usefulness of the respective organizational devices is an issue susceptible to theoretical and experimental investigation, and the first purpose of the present study is to compare the operation of the two in an experimental context in which factors that might influence performance in natural circumstances are random. We address this purpose in Experiments 1 and 2.

Our design involves a step-level game (Frohlich and Oppenheimer, 1970; Rapoport, 1985; van de Kragt et al., 1983) in which a "bonus," or public good, is provided to all group members in the event some specified number of them make a fixed contribution. In the *full dilemma*—the standard version of this game—individuals have a positive incentive for not contributing if enough others do contribute or if too few others contribute to make a contribution useful. In the former case, the individual can "free ride," and in the latter he or she can avoid wasting a contribution. Our institutional modifications involve *half dilemmas* in which one of these incentives is removed. These are the *money-back guarantee half dilemma* and the *enforced contribution* ("fair share") *half dilemma*, respectively. With the money-back guarantee, individuals receive higher payoffs for not contributing if the public good is provided and no more than contributors if it is not. With the enforced contribution, they receive higher payoffs for not contributing if the public good is not provided and no more than contributors if it is.

There is a third possible outcome of others' choices in any step-level dilemma, whether full or half: that the individual's contribution will be "critical," or neces-

sary for provision of the public good; that is, when k contributions are required, $(k - 1)$ others will contribute and the individual's contribution will make the difference.³ Setting aside this possibility, we note that players in the half dilemmas have a dominating incentive to withhold a contribution, just as do players in the standard game: they will make more by defecting if one outcome happens and no less by defecting if the other does.

At least in the large number case, it does seem reasonable to set aside the possibility of "criticalness." It is, to use our examples, highly unlikely that all (or, indeed, any) contributing faculty or apartment dwellers contributed because they believed their contribution would be critical. Yet this is possible, especially in smaller groups, and it is also possible that there are different beliefs about the probability of being critical in the three conditions. A second purpose of this set of experiments, therefore, is to examine the effects of the money-back guarantee and the enforced contribution on the subjective probability of (1) being critical, (2) wasting one's contribution, and (3) being redundant. We address this purpose in Experiment 3, in which we collect such probability judgments. In addition, we will use these probabilities to estimate the expected value of the payoffs for contribution or noncontribution in the standard dilemma and the two half dilemmas. These expected values are not the same in the three conditions, and we will attempt to relate actual choice between contributing and not contributing to these values both within and across conditions.

Predictions

As Coombs (1973) has pointed out, the prisoner's dilemma offers two incentives to defect: the desire to avoid being "suckered" and the desire to capture the "free rider's" payoff. In Coombs' terms, these

incentives are *fear* and *greed*, respectively, and they are redundant; either is sufficient to predict defection by itself. We note that our two half dilemmas each retain one of these incentives while eliminating the other. Providing a money-back guarantee offers the individual protection against loss through contributing, while enforcing a contribution should the good be provided means that nothing is to be gained from withholding a contribution—if the good is provided under enforced contribution, a player will end up paying one way or the other. In 1975, Brubaker proposed that defection was more a consequence of desire to avoid loss through contributing than of a desire for gain. If he is correct, we would expect greater group success from the half dilemma that offers a money-back guarantee than from the half dilemma that enforces contribution.

However, a structural difference between the two predicts the opposite. Let us suppose that people given a money-back guarantee believe that such a guarantee works—that is, it will increase the probability that others will contribute. This belief yields an increase in the subjective probability of the goods being obtained without the individual's contribution and, therefore, an increase in the probability that an attempt to free ride will be successful. For example, a faculty member who registered the money-back guarantee might have assumed that its presence would induce enough other faculty members to contribute to make his or her own contribution unnecessary. In short, the money-back guarantee may not "reduce" the dilemma at all. When such a guarantee is given, people may respond to it no differently from the way they respond to the standard dilemma and contribute for the same reasons they contribute in that standard situation (presumably conscience, fellow feelings, magnanimity, etc.) and in the same numbers. *The instability of the money-back guaran-*

*tee half dilemma may result in its collapse as a device for improving base rates of contributing.*⁴

In contrast, the enforced contribution dilemma is stable. Suppose that people who know they are operating under an enforced contribution rule believe that it will work—that is, it will increase the probability that others will contribute. This belief implies a decrease in the subjective probability that the good will not be provided by others' contributions—it makes group failure and the attendant reason for not contributing less likely. Why *not* contribute under such circumstances?

From this nonmotivational analysis, we predict that the enforced contribution half dilemma will be more effective in raising contributions than will the money-back guarantee half dilemma.

Methodological Overview

Predictions such as these can be readily tested in the laboratory where all other factors that might influence cooperation rates in natural situations are randomized. We do not claim that public goods games created in the laboratory are simulations of any particular naturally occurring situation; we claim only that the payoff matrices confronting subjects *are* public goods matrices. Similarly, we are not particularly interested in absolute levels of cooperation, which can be expected to vary with subject pools and (as we will see) historical periods. Our interest is in differences in cooperation that might exist between experimental conditions of theoretical interest, and we know of no good reasons for predicting an interaction between condition and population pool.

We created seven-person public goods problems under such laboratory circumstances. After they signed the necessary release form, subjects were seated around a large table and were read instructions.

Each was given a \$5 promissory note and was told that he or she could contribute that \$5 toward a "bonus" of \$70 to the group as a whole. This bonus would be distributed equally so that each group member would receive \$10. A specified minimal number of contributions was required for the bonus to be distributed; if that number of contributions was not forthcoming, there would be no bonus. Subjects made the decision between contributing and not contributing simultaneously, anonymously, and only once. The latter fact meant there was no possibility of influencing other subjects' behavior in the future, a consideration crucial to some proposed solutions to social dilemma games—notably Axelrod and Hamilton's (1981). Each subject was paid singly and was on the elevator before the next one was paid; this procedure was explained to the subjects in advance so that they knew their choices would be completely unknown to the other participants. If the subject had not contributed the \$5 and the bonus was produced, then the net payoff was \$15; if the subject had contributed and the bonus was produced, the net payoff was \$10; if the subject had not contributed and not enough other subjects had contributed to produce the bonus, the net payoff was \$5; and if the subject had contributed and not enough others had, the net payoff was zero. As was emphasized at the outset, there was no deception. Subjects' payoffs varied, as indicated, as a function of their own choices and of the simultaneous choices of the others in the experimental session.

The game described above is a standard step-level public goods game, one that involves no communication, no opportunity for persuasion or coercion, no possibility of side payments or reciprocity, and no social disclosure of individual choices (except to the experimenters). We modified it in two ways: the money-back guarantee half dilemma and the enforced contribution half dilemma.

Under the money-back guarantee half dilemma, subjects were told that if they contributed their \$5, but there were not enough other contributions to ensure provision of the bonus, their \$5 would be returned. Thus, there was no way in which subjects could lose their contributions. However, they could still free ride on the contributions of others. If a subject withheld his or her contribution and enough others contributed to provide the bonus, the subject would benefit from both the bonus and the original \$5, obtaining a total of \$15 for participating in the experiment.

Under the enforced contribution half dilemma, all payments were truncated at \$10. Should the bonus be provided, no subject could leave the experiment with more than \$10. (Truncating payments at \$10 is logically equivalent to forcing all to contribute \$5 if the bonus is provided.) Thus, there was no opportunity to free ride. When the bonus was provided by others' contributions, subjects who did not contribute left with the same amount of money—\$10—as those who had contributed. However, subjects could still lose their contributions and be "suckered." If enough other people did not contribute to produce the bonus, then individuals who did contribute would lose their \$5.

The main difference between our experimental dilemmas and the naturally occurring ones discussed earlier is size; with the smaller numbers demanded by experimental logistics, the probability of being critical may be seen as important. Such beliefs will be discussed, evaluated, and analyzed in Experiment 3. The purpose of these first two experiments is to compare the relative efficiency of the two half dilemmas in enhancing cooperation and to test the prediction that enforcing a contribution in the event the good is provided will elicit more voluntary contributing than will providing a money-back guarantee.

Table 1. Experiment 1: Percentage Contributing, Number of Contributors, and the Analysis of Variance and Scheffé Test Results (3 or 7 required)

		Standard Dilemma (1)	Money-Back Guarantee (2)	Enforced Contribution (3)
Percentage contributing		51	61	86
Number of contributors in each group		1, 2, 2, 3, 3, 3, 4, 5, 6, 7	3, 3, 4, 4, 5, 5, 6	4, 5, 5, 6, 6, 6, 7, 7, 7, 7
Analysis of Variance				
	<i>df</i>	<i>MS</i>	<i>F</i>	<i>p</i>
Between conditions	2	14.05	7.41	.003
Within conditions	24	1.90		
Total	26	2.83		
Post hoc Scheffé tests ^a				
	Average difference	Range for $p = .10$	Range for $p = .05$	Range for $p = .01$
(1) vs. (2)	.59	1.52	1.76	2.32
(1) vs. (3)	2.30	1.38	1.59	2.04
(2) vs. (3)	1.71	1.52	1.76	2.32

^aAny difference between means larger than the number specified is significant at the level indicated.

Experiment 1

Subjects were recruited by advertising in the Eugene, Oregon, *Register-Guard* and in student newspapers at the University of Oregon and Utah State University. Subjects participated in the standard dilemma game, the money-back guarantee half dilemma, or the enforced contribution half dilemma. As much care as possible was taken to ensure that subjects in a particular session did not know each other prior to the experiment. In each condition, contributions from any three or more subjects in seven-subject groups were required for the bonus to be provided. The standard dilemma game and the money-back guarantee half dilemma were run in 1979 (forming the core of Simmons's doctoral dissertation; see Simmons, 1980), while the enforced contribution half dilemma was conducted two years later.⁵

All subjects were given the \$5 promissory note, after which the appropriate experimental condition was explained in

detail. The explanation covered both the logic of the game and the explicit set of monetary payoffs under each condition. Subjects were "quizzed" about the payoffs, and if there was any misunderstanding, the instructions were explained again. The individual payoffs and anonymity were emphasized. *Subjects were not permitted to communicate among themselves; any questions were directed to the experimenter.* In all conditions, subjects made their choices—either to withhold the \$5 promissory note or to contribute it—by checking the appropriate box on the "decision form" they had in front of them.

Results of Experiment 1

Table 1 presents the percentage of subjects contributing, the number of contributors, the analysis of variance results, and the results of a *post hoc* Scheffé test using groups as the unit of analysis.⁶ The results indicate that there is no significant difference between contribution rates in the standard dilemma and the money-

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Table 2. Experiment 2: Percentage Contributing, Number of Contributors, and the Analysis of Variance and Scheffé Test Results (5 of 7 required)

		Standard Dilemma (1)	Money-Back Guarantee (2)	Enforced Contribution (3)
Percentage contributing		64	65	93
Number of contributors in each group		3, 4, 4, 4, 4, 4, 5, 5, 6, 6	3, 4, 4, 5, 5, 5, 6	5, 6, 6, 6, 7, 7, 7, 7, 7, 7
Analysis of Variance				
	<i>df</i>	<i>MS</i>	<i>F</i>	<i>p</i>
Between conditions	2	12.24	15.69	.00
Within conditions	24	.78		
Total	26	1.66		
Post hoc Scheffé tests ^a				
	Average difference	Range for $p = .10$	Range for $p = .05$	Range for $p = .01$
(1) vs. (2)	.07	.99	1.14	1.46
(1) vs. (3)	2.00	.89	1.03	1.32
(2) vs. (3)	1.93	.99	1.14	1.46

^aAny difference between means larger than the number specified is significant at the level indicated.

back guarantee half dilemma (51% and 61%, respectively) while there is a significant difference between contribution rates in the standard dilemma and the enforced contribution half dilemma (51% vs. 86%).

Experiment 2

This experiment was identical in all respects to Experiment 1, except that five out of the seven subjects were required to make a contribution in order for the bonus to be provided. By requiring five contributions instead of three, questions of criticalness, deficiency, and redundancy were reversed. For example, whereas two of six others had to contribute for an individual's contribution to be critical in Experiment 1, four of six others in this experiment had to do so.⁷

Results of Experiment 2

The outcomes of Experiment 2 are given in Table 2. Once again, there is no

significant difference between contribution rates in the standard dilemma and the money-back guarantee half dilemma (64% and 65%, respectively) while there is a significant difference between contribution rates in the standard dilemma and the enforced contribution half dilemma (64% vs. 93%). Note that the contribution rates are somewhat higher in this experiment (where five contributions are required to activate the bonus) than in the first experiment (where three contributions are required). An analysis of variance in which the main effect is three vs. five required contributions shows that the number required does not produce a significant main effect ($F_{1,48} = 3.32$, $p > .07$) and that its interaction with the three conditions is not significant ($F_{2,48} = .196$, $p > .7$).

As predicted by the structural hypothesis, the enforced contribution half dilemma was superior to the money-back guarantee. In fact, the contribution rate in the money-back guarantee half dilemma

was not significantly higher than the rate in the standard dilemma in either experiment while the differences between the enforced contribution half dilemma and the standard dilemma were large and highly reliable. Whether the money-back guarantee works at all is a moot point; across both experiments it yields a cooperation rate of 63%, as opposed to 57% in the standard dilemma. (A rough median split indicates that if these percentages remained constant, the results would be significant if the sample were multiplied by a factor of about 10.)

Experiment 3

What accounts for the 60% cooperation rate in our standard dilemma without any structural modification? One possible explanation is that such base-line contributors were influenced by motivations unrelated to their own monetary payoff—for example, conscience, altruism, and self-esteem. Another explanation, however, derives from the step-level character of our basic design. These contributors might have believed the probability that they were critical to the provision of the bonus (to themselves, as well as to the group) was greater than .5, in which case they would receive a net gain of \$5 by contributing, not a net loss of \$5, as they would under any other circumstance. The purpose of our third experiment was to distinguish between these two hypotheses by investigating the relationship between subjective probability estimates about others' contributions and a person's own choice, both within and between groups. This approach was suggested by Rapoport (1985), who begins by postulating that in each of these experimental goods situations, each participant estimates three probabilities:⁸

1. p = the probability of being futile—that is, the probability that whatever one does, an insufficient number of

- others will contribute for the public good to be provided (less than $[m - 1]$ others when m or more are required);
2. p^* = the probability of being critical—that is, the number of others contributing is exactly the number such that the bonus is provided if and only if the individual contributes (exactly $[m - 1]$ others when m or more are required);
3. $(1 - p - p^*)$ = the probability of being redundant—that is, a sufficient number of others will contribute to ensure that the public good will be provided regardless of what the individual chooses (m or more others when m or more are required).

Rapoport works out the expected values for contributing and not contributing. For example, consider the value of cooperation to be $V(c)$ and that of the bonus to be $V(g)$. Letting the subscript 1 refer to the full dilemma condition, we compute the expected value of contributing to be

$$0 \times p_1 + V(g) \times (1 - p_1) = V(g) \times (1 - p_1). \quad (1)$$

That of not contributing is

$$(p_1 + p^*_1) \times V(c) + (1 - p_1 - p^*_1) \times [V(g) + V(c)] = V(c) + (1 - p_1 - p^*_1) \times V(g). \quad (2)$$

The difference is $[p^*_1 \times V(g) - V(c)]$, which is greater than zero only if

$$p^*_1 > V(c)/V(g) = 1/c, \quad (3)$$

where $[c = V(g)/V(c)]$.

Hence, an individual maximizing expected value would choose to contribute if and only if inequality (3) were satisfied. Similar algebra demonstrates that in the money-back guarantee half dilemma, the expected value of contributing is greater than that of not contributing if and only if

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$$p_2^* > (1 - p_2)/c, \quad (4)$$

where the subscript 2 indicates probability estimates for this second situation. Finally, it can be demonstrated that for the third situation—the enforced contribution half dilemma—the expected value of contributing is greater than that of not contributing if and only if

$$p_3^* > p_3/(c - 1). \quad (5)$$

Rapoport derives the same three conditions. Note first that all three involve an inequality implying that the maximizer of expected value should contribute if and only if p^* is greater than some value. The inequality involves p^* because the situation in which the potential contributor is critical is the only one in which he or she experiences a net gain of \$5 by contributing, rather than a net loss of \$5 or indifference. Note also that all three inequalities involve only probabilities and the ratio of $V(c)$ and $V(g)$, that is, c , which in our experiments is equal to two. These equations allow qualitative predictions, even though the actual probabilities p , p^* , and $(1 - p - p^*)$ may vary by condition. That is, within each condition we can compare the choice of each individual to his or her estimate of p^* and in conditions 2 and 3, of p .

Method

We replicated Experiment 2 (5 or more contributions out of 7 required to elicit the bonus) with subjects recruited by advertisements in the student newspaper at Utah State University, Logan, Utah, in late November and early December 1984. In addition to asking subjects to make a choice, we asked them each to estimate the probabilities p , p^* and $(1 - p - p^*)$. As before, each subject was assigned to only one condition on a quasi-random basis, and care was taken that friends or acquaintances were not assigned to the same group. The instructions were identical to those of Experiment 2. The prob-

ability estimates were elicited by asking subjects to distribute 100 points, as follows:

For the following questions you must enter a number between 100 and zero. The number indicates how likely you think it is that the specified outcome will occur. These numbers must total to 100. If you are certain that a specific outcome will occur, put 100 in that box and 0 in the others. If you think that each outcome is equally likely, put 33.3 in each box. Any combination is possible, of course, but remember that all numbers must add to 100.

We then asked for answers to the following questions:

1. What is the likelihood of fewer than four others choosing to invest, that is to say, one, two or three other members of the experiment?
2. What is the likelihood of exactly four others choosing to invest?
3. What is the likelihood of more than four others choosing to invest, that is to say, five or all six of the other members of the experiment?

Five groups were run in each of the conditions: standard dilemma, money-back guarantee half dilemma, and enforced contribution half dilemma.

Results of Experiment 3

Table 3 presents the percentage contributing, the number of contributors in each group, and the analysis of variance and a *post hoc* Scheffé test result between conditions. Note that the overall contribution rate is less in 1984 than in 1979–81, significantly so ($F_{1,40} = 15.14$, $p < .0005$). Nevertheless, the pattern is the same: there is no significant difference between the standard dilemma and the money-back guarantee half dilemma, and there is a significant difference between

Table 3. Experiment 3: Percentage Contributing, Number of Contributors, and the Analysis of Variance and Scheffé Test Results (5 of 7 required)

		Standard Dilemma (1)	Money-Back Guarantee (2)	Enforced Contribution (3)
Percentage contributing		23	43	77
Number of contributors in each group		1, 1, 2, 2, 3	5, 2, 1, 3, 4	4, 5, 6, 6, 6
Analysis of Variance				
	<i>df</i>	<i>MS</i>	<i>F</i>	<i>p</i>
Between conditions	2	18.47	15.39	.0005
Within conditions	12	1.20		
Total	14	3.67		
Post hoc Scheffé tests^a				
	Average difference	Range for $p = .10$	Range for $p = .05$	Range for $p = .01$
(1) vs. (2)	1.40	1.64	1.93	2.58
(1) vs. (3)	3.80	1.64	1.93	2.58
(2) vs. (3)	2.40	1.64	1.93	2.58

^aAny difference between means larger than the number specified is significant at the level indicated.

the full dilemma and the enforced contribution half dilemma.

As we pointed out earlier, our interest is in behavioral differences between theoretically relevant conditions in experiments such as ours not in absolute levels of cooperation, which can be expected to reflect cultural and other parametric differences. It is easy—no doubt too easy—to speculate on the causes, as well as the consequences, of what here seems to be a behavioral difference between historical periods.

Table 4 gives the average probability estimates, along with their standard deviations, of being futile, being critical, and being redundant in the three conditions. The estimates are broken down into cooperators vs. defectors. A three-way repeated measures analysis of variance on the data of the second part of Table 4 (treating the estimated probabilities of being futile and of being redundant for each individual nested within conditions as the repeated variable) reveals that

probability estimates vary by condition ($F_{2,102} = 5.35, p = .01$).¹⁰

The pattern is clear. The probability of redundancy is enhanced and the probability of deficiency is diminished in both half dilemmas by a ratio of 2:1. The judged probability of being critical is unaffected. This pattern of average estimates is consistent with the expectations model of cooperation; it follows that *on the average*, a subject should cooperate in the three conditions if p^* is greater than .50 for equation (3), .37 for equation (4), and .27 for equation (5). These inequalities are progressively easier to satisfy—but note empirically that, on the average again, none is itself easy to satisfy. Note also that, since the p^* s are inherently incomparable, this prediction is “weak.”

Moreover, the analysis shows that there is a main effect for cooperation ($F_{1,102} = 26.36, p < .001$) with cooperators having higher estimates of being critical or redundant than defectors. More importantly, there is an interaction effect

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Table 4. Experiment 3: Average Probability Estimates, with Standard Deviations, of Being Futile, Critical, and Redundant, and Analysis of Variance for Repeated Measures

Experimental Condition and Choice	Probability of Being Futile ($M < 4$)		Probability of Being Critical ($M = 4$)		Probability of Being Redundant ($M > 4$)	
	Average Probability Estimate	Standard Deviation	Average Probability Estimate	Standard Deviation	Average Probability Estimate	Standard Deviation
By condition						
Standard dilemma	.47	.27	.21	.20	.32	.25
Money-back guarantee	.27	.17	.25	.18	.43	.23
Enforced contribution	.27	.22	.24	.15	.50	.26
By choice and condition						
Standard dilemma						
Cooperators ($N = 9$)	.24	.20	.29	.31	.46	.32
Defectors ($N = 26$)	.55	.25	.18	.13	.26	.20
Money-back guarantee						
Cooperators ($N = 15$)	.25	.15	.29	.19	.46	.18
Defectors ($N = 20$)	.28	.19	.21	.16	.50	.28
Enforced contribution						
Cooperators ($N = 27$)	.20	.16	.23	.15	.57	.25
Defectors ($N = 8$)	.51	.23	.25	.16	.24	.08

for cooperation by condition ($F_{2,101} = 5.91$, $p < .001$). Thus, the estimates of cooperators and defectors are differentially affected by the changes from the standard condition to the two half dilemmas. Are these changes compatible, however, with the hypothesis that cooperation or defection are due to differential expectations aroused by the different conditions?

First, the actual numbers do not support the hypothesis. It is specifically the defectors whose expectations should be compatible with not contributing, but under the standard condition, the defectors' average probability estimates of either deficiency or redundancy is .81, as opposed to .70 for the cooperators (who

—according to the model—should not cooperate in such circumstances). Under the money-back guarantee condition, the defectors' average probability estimate of being redundant (which makes free riding possible) is virtually identical to that of the cooperators: .50 vs. .46. Only in the enforced contribution condition are the differential directional predictions for cooperators as a group vs. defectors as a group supported. The average probability estimate of the defectors (there were only eight) of being deficient is .51, as opposed to .20 for the cooperators.

When the predicted behavior of each separate individual is compared with his or her actual behavior, directional results are more compatible. Here we computed

the phi value of the 2×2 contingency between whether the subject did or did not contribute and whether, for that subject, inequality (3), (4), or (5) is satisfied in the appropriate condition. In the standard dilemma, 7 of the 9 contributors are predicted defectors while none of the defectors is a predicted contributor; phi is .42. Under the money-back guarantee condition, 12 of the 15 contributors are predicted defectors and, of the 20 defectors, 3 are predicted cooperators; phi is .09. Under the enforced contribution condition, 13 of the 27 contributors are predicted defectors while 2 of the 8 defectors are predicted contributors; phi is .23. Pooling across the three conditions, only 19 of the 51 contributors "should have" contributed, while 5 of the 54 defectors "should have" done so too; phi is .33 (chi-square = 11.52, $df = 3$, $p < .01$). Thus, the direction of contingency supports the predictions of expectations, but note that a full 63% of the contributors had a negative expected value for contributing. The phi coefficient is, however, much larger in the standard and enforced contribution conditions than in the money-back guarantee, a finding that could follow from the hypothesized instability of the money-back guarantee condition.

A final individual analysis involved the contingency between contributing or not and estimating that such a contribution would result in a net loss of \$5 relative to potential payoff. In the standard condition, such a \$5 loss occurs if an individual's contribution would be either deficient or redundant. Of the 9 cooperators, 7 believed that they had a better than .50 probability of sustaining such a loss—that is, the sum of each person's estimated probabilities of deficiency and redundancy was greater than .50. In contrast, 25 of the 26 defectors believed that they had a probability of over .50 of sustaining such a loss. Again, the absolute numbers do not support the expectations model, but the direction does, although not sig-

nificantly so; phi = .29.

Under the money-back guarantee condition, such a \$5 net loss occurs only if the individual is redundant—if more than four others also contribute. Of the 15 cooperators, 5 believed that they had a better than .50 chance of sustaining such a loss while 8 of the 20 defectors believed that they did. Here, even the directional contingency is virtually nonexistent; phi = .07.

Under the enforced contribution condition, such a \$5 net loss to a contributing individual occurs only if that individual is deficient—if fewer than four others also contribute. Only 1 of 27 cooperators believed that he or she had a better than .50 probability of sustaining such a loss, while 3 of 8 defectors believed that they did. Once more, the absolute numbers do not support the expectations model, but the direction does; phi = .45. We also compared the last two contingencies by means of a Goodman z test for analyzing the interaction term in a $2 \times 2 \times 2$ table.¹¹ The resulting value was 1.71 ($p < .05$, one-tailed), but this value must be treated as merely suggestive, due to the *post hoc* nature of the test and because two cells had expected values of less than 5.0 (1.93 and 3.63).

Finally, pooling across all conditions, of 49 subjects who expected a \$5 relative loss with probability greater than .50 were they to contribute, only 13 did. In contrast, of 56 who did not estimate the probability of such a relative loss to be greater than .50, 38 cooperated. This contingency is not only strong (phi = .41), but highly significant; chi-square = 17.87 with 3 degrees of freedom, $p < .001$.

We must point out that the analysis based on potential net loss ignores the outcome in which contributing or not leads to the same outcome.¹² Nevertheless, the results of the analysis constitute a clear comparison of the money-back guarantee with the enforced contribution. The enforced contribution results are

more compatible with the expectations model than are the money-back guarantee results (again supporting the hypothesis of instability of expectations under the money-back guarantee). Moreover, in the enforced contribution, the contingency between estimation and choice is stronger when the possibility that leads to the same result for contributing and not contributing is ignored.

Discussion

There is no ambiguity whatever about the success of the money-back guarantee device for eliciting contributions compared with the success of the enforced contribution device: the enforced contribution is superior. The pattern of results is stable over variations in the rate of contribution between regions and over a five-year time span. In all three replications it was superior to the standard dilemma at a high level of statistical significance. In two of three, it was significantly superior to the money-back guarantee and was marginally better in the third. In contrast, in no experiment did the money-back guarantee yield results that were superior to those in the standard dilemma at standard levels of significance.

Much of what social science can demonstrate is already "known" by evolved social systems, and we speculate that the frequent use of the enforced contribution or "fair share" device—most notably, of course, in labor unions—is a consequence of long experience with alternative ways of trying to persuade the beneficiaries of group efforts to contribute to those efforts. Institutions that work or that add nothing beyond some base tend to be found out, as do institutions that do not work.

Yet if frequent use suggests that a mechanism has been found to work, it does not, in itself, explain why that mechanism works. We proposed that "fair share," or

enforcing a contribution in the event the group effort succeeds, is successful because expectations of its success do not undermine that success—in contrast to the money-back guarantee. However, while the behavioral data supported the prediction, the expectations data (Experiment 3) lend only directional support. There is clearly a contingency between choice and expectation based on probability estimates, but the actual behavior must be influenced by other factors as well. We speculate that one such factor is the relative motivational strengths of Coombs's (1973) *fear* and *greed*.

Earlier, we mentioned Brubaker's (1975) hypothesis that fear of being "suckered" underlays defection, not desire to capture the "free rider's" payoff (*greed*, in Coombs's terms). This hypothesis predicts that a money-back guarantee, which offers the individual protection against losing a contribution, will work better than enforcing a contribution. This means that opportunity for gain through defecting is removed. The failure of the money-back guarantee to increase contributing beyond the level of the standard dilemma suggests that Brubaker is wrong. Fear of loss through contributing is not the critical motivation underlying defection.

On the other hand, the relative success of the enforced contribution is consistent with the hypothesis that desire for gain through defecting is the motivation underlying defection. By hypothesis, enforcing a contribution if a public good is provided works to promote contributing by convincing people that the good will be provided and by removing the opportunity for free riding if it is provided. Free from the fear that the good will not be provided and that their contribution will be lost, people contribute under this rule because they see nothing to be gained by not contributing and, presumably, because they recognize the personal benefits available from the public good. Enforcing contribution should a

public good be provided is, by this hypothesis, an institutional modification that is appropriately attuned to widespread, perhaps characteristic, human motivations.

Notes

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1. In our experiments with a dominant incentive to defect of \$5, about 30% of the subjects characteristically cooperate. A pessimist, of course, might see such cooperation rates as high.

2. It was, we maintain, a public good for the state, the country, and the world at large.

3. Thus, these step-level games are not strictly social dilemmas, which require a dominant incentive to defect. Note that this absence of a dominant incentive does not preclude their classification in standard terms as "public goods" games. See Frohlich and Oppenheimer (1970) and van de Kragt, Orbell, and Dawes (1985).

4. Of course, there is the possibility of an endless regress here: if an individual is tempted to free ride due to believing the guarantee will work, he or she may infer that the others will also be tempted, which means that the contribution rate will be lower, which makes the guarantee more important, and so on.

5. The distribution of sessions over locations was as follows:

	Utah	Oregon
Standard dilemma	2	8
Money-back guarantee	3	4
Enforced contribution	5	5

6. The test can be found in Scheffé (1960). The computations were executed through the SPSSX one-way module and its Scheffé test option (SPSSX, 1983, p. 458).

7. The distribution of sessions over locations was as follows:

	Utah	Oregon
Standard dilemma	7	3
Money-back guarantee	4	3
Enforced contribution	5	5

8. We wish to acknowledge our debt to Amnon Rapoport for developing this approach and our

pleasure that he found these step-level games of sufficient interest to develop his interpretation of the results. Palfrey and Rosenthal (1984) have developed an alternative interpretation based on mixed strategies. While their approach contains some elements identical to Rapoport's (e.g., solving for indifference between contributing and not contributing), the predictions following from it allow so many possible pure and mixed equilibria that we do not elaborate on it here.

9. The typographical error of omitting zero from this statement apparently had no effect; almost all subjects gave estimates for the three possibilities adding to one.

10. That the estimates of being deficient, critical, and redundant are not equal is a trivial finding ($F_{2,102} = 8.60, p < .01$). What we report as effects are all, technically, interactions with these three estimates.

11. See Goodman (1964).

12. In fact, this analysis is confounded with the earlier analysis of variance of repeated measures, which treated choice as the independent variable and estimates as the dependent one, rather than vice versa. We present both analyses, complete with significance levels, not because we believe in redundant analyses as a general desideratum—quite the opposite—but because each alone is flawed. In this context, we know of no overall best analysis. Certainly, an overall goodness-of-fit statistic, given directional support but striking discrepancy from predictions, would be inappropriate.

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AGENDA CONTROL, COMMITTEE CAPTURE, AND THE DYNAMICS OF INSTITUTIONAL POLITICS

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Most models of agenda control examine dyadic relations—for example, those between a committee and the floor of a legislature. Such relations, however, are always embedded in a larger context, namely, a political environment composed of voters and interest groups. In this paper we model agenda setters (a legislative committee) as decision makers with limited cognitive abilities who adjust over time to their larger political environment. The legislators' policy positions are endogenous, reflecting the relative strengths of voters wielding the district-specific resource of votes and of interest groups wielding the transferable resource of money. The resulting outcomes indicate that neoclassical models of voting and pluralist models of group influence have each told part of the story. When only votes matter, our boundedly rational agents grope toward equilibria close to those of neoclassical models; however, when mobile resources matter as well, the outcomes depart systematically from those of previous models. In particular, interest groups can make themselves worse off by capturing the committee. The results suggest that agenda control is less powerful than conventionally believed and point toward conditions shaping its effectiveness—conditions highlighting the distinctive contributions of pluralist and neoclassical thinking to a broader theory of political institutions.

Recent years have witnessed important developments in the formal analysis of political institutions. Formal theorists, long centrally concerned with voting and markets, have taken an active interest in questions of structure, hierarchy, and control. Students of organization and public administration, traditionally skeptical of mathematical approaches, have increasingly recognized the relevance of these efforts for their own areas of substantive concern. Encouraged by new perspectives within both groups, formal theories of legislative and bureaucratic behavior

have developed rapidly and assumed a larger, more influential role in the institutional literature.

The take-off point was a long time in coming. For decades, institutional analysis was almost entirely sociological and historical in method, with little attention to formal theories or economic modes of analysis. There was one important exception: the Simon-March behavioral tradition, with its emphasis on bounded rationality, adaptation, and dynamic models of organizational decision making. Yet, while this tradition had, and continues to have, substantial

influence in the realm of ideas, its formal models did little to bring about a methodological reorientation or a wider appreciation of the promise of formal approaches. These changes were set in motion only recently through significant developments in social choice theory and the economics of organization. In social choice theory, new research has moved away from a focus on pure majority rule and its associated disequilibria toward the investigation of institutional mechanisms—agenda control most prominent among them—that shape and stabilize voting outcomes (Denzau and Mackay, 1983; Enelow and Hinich, 1984a, Ferejohn et al, 1984; McKelvey, 1976; Romer and Rosenthal, 1978; Shepsle, 1979; Shepsle and Weingast, 1981). In the economics of organization, economists have moved to provide an organizational foundation for neoclassicism, putting rational choice theory to use in explaining the emergence and behavior of complex organizations, the nature of hierarchic control, and its function as a productive alternative to markets (Alchian and Demsetz, 1972; Coase, 1937; Hess, 1983; Moe, 1984; Williamson, 1975).

These research programs may well transform our understanding of institutions. In particular, we agree with Riker (1982) that the new understanding of agenda control is a major insight that highlights the political significance of institutional structure. The existence of structure-induced equilibria (Shepsle, 1979)—of policies that are selected because well-positioned decision makers manipulate the choice sets of other agents—indicates that political institutions are not merely transmission belts for environmental forces. Instead, they shape outcomes in patterned ways.

The idea that manipulated choice sets can stabilize policy outcomes is a powerful and fertile concept, one that has created a research program within the larger stream of formal analyses of

political organizations. An indicator that an idea has created a research program is the number of its progeny. By this criterion, the concept of institutionally based agenda control is well on its way.¹ Because many of these works draw inspiration from the large and growing literature on the economics of information and organization, it is safe to say that the neoclassical analysis of agenda control—and, more generally, of political institutions—will be a fruitful research program for many years to come.

Yet much remains to be done. As part of a pioneering effort to gain an analytical foothold, current models are usually kept quite simple, abstracting from much of the substantive and theoretical features that the institutional literature has long emphasized and, in fact, that formal modelers themselves often recognize as integral components of future, more comprehensive models (see, e.g., Shepsle, 1983). It is impossible at this point to know with any certainty which features are essential and which can safely be overlooked, but it does seem clear that the theoretical progress hinges on systematic efforts to explore these avenues and thus to elaborate existing models by tentatively incorporating what now appear to be important aspects of institutional detail and behavior. Several dimensions stand out as particularly promising candidates.

1. *Institutional context.* Most models focus on relationships that are essentially two sided: chairman-committee, committee-legislature, bureau-committee, principal-agent. Yet as the empirical literature has long emphasized, it is difficult to understand these relationships without placing them in larger context. In the United States especially, bureaus, the legislature, committees, and constituents are all bound up with one another. Their incentives and interactions are mutually structured, and they should presumably be modeled as such.

2. *Endogenous policy positions.* Institu-

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tional models of agenda setting often take the policy positions of political actors as exogenously fixed when, in fact, these positions often derive from dynamic relations among actors and change in systematic ways over time. Legislators motivated by reelection, for example, have induced preferences over policy alternatives as a reflection of constituency influences, and their induced preferences change with changes in the constituency. In a larger institutional model, legislators' policy positions would not be taken as given but would become endogenous components of the system.

3. *Political resources.* Most models assume that the only political resource influencing politicians is voting, a resource that cannot flow across districts. Yet in electoral campaigns the significance of money—the quintessentially mobile resource—is evident to scholars and journalists alike (Drew, 1983; Jacobson, 1980). A more elaborate model would recognize that legislators' policy positions may respond in varying degrees to both district-specific and transferable resources.

4. *Cognitive limitations.* While the most common modeling assumption is that people are computationally flawless, the thrust of the empirical literature is that the patterns of behavior we observe in the policy process are explained in large part by the cognitive limitations of decision makers. These behavioral patterns, moreover, can play an important role in compensating for the imperfections of individuals, allowing them to generate collective outcomes that reflect the operation of a higher level of "intelligence." None of this can be investigated or explained if cognitive limitations are assumed away at the outset.

5. *Process.* Current models use comparative statics as the main method of analysis. Comparative static predictions are invaluable, but they do not address the process of interaction, how it evolves

over time, or how equilibria are determined. Particularly in a world of cognitive limitations and endogenous policy positions, insight into this process would appear to be crucial to an understanding of institutions and their outcomes.

In this paper, we attempt to move along all five of these dimensions in offering a new perspective on agenda control and its consequences for institutional outcomes. We begin with a model of institutional politics that (1) incorporates a bureau, a majority-rule legislature, and constituency groups, (2) makes legislative policy positions endogenous, (3) assumes political resources can flow freely throughout the system, (4) views all actors as having limited information and cognitive abilities, and (5) generates an institutional process of mutually adaptive adjustment among these actors over time. In an earlier article, we developed this model in some detail and explored its basic properties (Bendor and Moe, 1985). Its hallmark is that, under quite general conditions, the system moves toward what we have called a *pluralist equilibrium*—a steady state characterized by a balance of interest group resources.

Here, we use this earlier institutional model as a foundation, and we explore agenda control by introducing two new components: (1) a legislative committee with monopoly power over the proposal of policy alternatives and (2) a system of electoral districts whose boundaries constrain the flow of political resources. The general question then becomes, how does the known behavior of the institutional system change in response to these new components, and how do these impacts vary with changes in the fundamental structure of the system itself?

More concretely, we will be investigating ideas that have long held sway in the institutional literature and, to this point, have generally been reinforced by formal analysis. The conventional wisdom is that oversight committees tend

also to be "high-demand" committees—that is, committees in some sense "captured" by interest groups benefitting from the policies in question—and that the committees are in fact successful in using their agenda control powers to bring about the policy outcomes preferred by the capturing groups. However accurate this may be as a description of politics, its analytical foundations remain unclear. Are agenda powers in themselves sufficient to guarantee committee success in engineering policy outcomes, or is capture somehow integral or necessary to their efficacious use? Does capture of an agenda-setting committee in fact work to the advantage of the capturing group? How are agenda control, committee capture, and the relationship between them conditioned by the mobility of political resources and the constraining role of electoral districts?

Most generally, our analyses will show that agenda control and committee capture are more problematic than the literature tends to suggest. The mobility of political resources plays a crucial role in this regard. When resources are district specific, for example, successful committee capture and agenda control are promoted in much the same fashion that now-familiar spatial models lead us to expect, and this occurs despite the cognitive limitations of our actors. When all political resources can flow across districts, however, very different patterns emerge, patterns consistent with pluralist images of balance of power policy making. With mobile resources, agenda control is often rendered impotent as committee members move to the legislative center, and efforts at capture not only may fail in the long run but may even leave the intended beneficiary worse off than if these mechanisms of institutional power had never been used in the first place. Because in the real world of American politics some resources are district specific and some are mobile, it

seems clear that theories of agenda control and institutional politics must incorporate both pluralist and spatial notions of political influence.

This work is an effort to shed new light on agenda control and its implications for institutional politics, but we also want to encourage some rethinking on the methodological front. As formal models go, ours is a bit unorthodox—it is a computer simulation anchored in the Simon-March behavioral tradition and, to our knowledge, is the only analysis of agenda control to derive from this line of work. Our hope is that while neoclassical methods have recently been instrumental in promoting the formal analysis of institutions, efforts to build more comprehensive models will recognize the value of this tradition and put its ideas and methods to effective use.

An Overview of the Model

Our model of institutional politics consists of three basic types of participants—an agency, legislators, and interest groups—and the interactions among them. The detailed assumptions governing their motivation, information, and adaptive adjustments have been described elsewhere (Bendor and Moe, 1985), and, for economy of presentation, we will not cover the same ground here. Readers unfamiliar with the model are provided with a summary of assumptions in the Appendix.

At a general level, the model is quite simple. The electorate is comprised of two groups of citizens, one that benefits from agency programs and one that is hurt by them. These citizens express their preferences about agency activity through the electoral connection: they evaluate legislators based on the legislators' positions on agency budgets. Generally speaking, citizens who benefit from higher levels of agency activity support higher

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agency budgets and thus legislators who vote accordingly; citizens who shoulder costs as a result of agency activism do the reverse. Legislators adjust their budgetary votes over time in an effort to enhance their electoral security. These votes, aggregated through the legislature's rules and procedures, yield budgets that are passed along to the agency, enabling it to generate some level of programmatic activity.² How much activity a given budget generates is largely up to the agency, however, and depends on what the agency values—budgets, slack, policy, or some combination of these—along with its assessment of whether it is being rewarded or sanctioned for its past choices of activity level. Seeking to enhance its own utility, the agency decides how much activity to produce (through its choice of how "efficiently" to translate its budget into output), and this new level of activity is then experienced in the form of new levels of benefits and costs for citizens. This alters the stakes of political activity for citizens, prompting them to adjust the amount of resources they devote to politics. Having done so, they turn to a fresh evaluation of their legislators, noting how the legislators' votes changed over the period, and adjust their support and opposition accordingly among them. This sends the cycle of influence into another iteration—stimulating new changes in legislative votes, agency activity, political resources, and so on until an equilibrium is eventually reached at which budgets, activity levels, and other systemic outcomes take on stable values.

The model is not specialized as to types of policies or agencies, but because it is often helpful to think in more concrete terms, we will develop the analysis with reference to an agency engaged in consumer protection regulation. We take the relevant groups of citizens to be "consumers" and "business firms." The agency's output is the level of regulatory

enforcement, and stronger enforcement is assumed to benefit consumers and to impose costs on firms.

The analysis is organized by the presence or absence of the two components we ultimately want to graft onto the basic institutional model: an agenda-controlling committee and electoral districts. First we assume that all relevant resources are mobile throughout the system—that is, there are no districts—and explore the consequences of introducing a committee with agenda-setting powers. In the next section we go to the other extreme, exploring the role of such a committee when the political world exhibits electoral districts and all relevant resources are constrained from flowing across district boundaries.³ Finally, we combine the two approaches by investigating agenda control when both mobile and district-specific resources come into play.⁴

Mobile Resources

In this section, all political resources are assumed to flow freely throughout the system, generated by firms and consumers in response to the prevailing level of regulatory enforcement. As enforcement rises, the political stakes—benefits for consumers, costs for business—rise as well, and both sectors devote more resources to the electoral process. These political "warchests" are then divided among legislators in accordance with their positions on agency budgets. The larger the budgetary increase advocated by a given legislator, the more support he will attract from consumers and the more opposition from business; the converse obtains, of course, for a decrease in the budget.

From Majority Rule to Agenda Control

Suppose we take as our null model—call it Version 1-1 (model 1, version 1)—a context in which the bureau is a budget

Table 1. Computer Runs of Model 1: Mobile Resources

	Simulation Version					
	1	2	3	4	5	6
Parameters						
Committee has agenda authority?	no	yes	no	yes	yes	yes
Interest groups allocate extra resources to committee?	neither	neither	consumers	consumers	consumers	consumers and firms
Initial budget and enforcement	low	low	low	low	high	high
Outcomes						
Enforcement	169	172	145	151	242	177
Efficiency	.53	.60	.52	.76	.81	.75
Type of equilibrium ^a	PE	PE	MPE	SE	SE	PE

- ^a PE = pluralist equilibrium
MPE = modified pluralist equilibrium
SE = structural equilibrium

maximizer overseen by the pro-consumer contingent within the legislature. This context would certainly appear to be favorable from the standpoint of consumer interests. While the bureau does not care at all about pursuing consumer enforcement objectives, it does desire bigger budgets and is presumably willing to do whatever it takes to get them. Consumerist legislators are supportive of budgetary increases for the agency because, other things being equal, these increases translate into higher levels of enforcement. If the bureau should behave perversely with its larger budgets, moreover, legislators have oversight mechanisms to bring the bureau back into line.

Now suppose we initialize this model—which still has no committee—by assuming that the bureau begins with a budget of 100 units and an efficiency of .8, which yield an initial enforcement level of 80. This level of enforcement is well below the social optimum of 150. Can consumers and their legislative agents succeed in increasing bureaucratic enforcement?

The results, set out in Table 1, indicate that enforcement does go up. The system reaches equilibrium beyond the social optimum at an enforcement level of 169. This is a *pluralist equilibrium*—that is, the point at which the resources contributed to politics by business and consumers are equal and thus at which a balance of forces exists in the constituency. The reason the system arrives at such an equilibrium, however, is not that consumers and their supporters in the legislature are “powerful,” for this result (which we could easily demonstrate with additional runs of the model) is in fact independent of the suboptimal starting point. Had we started the system out at an enforcement level well beyond the social optimum, it would then have moved down to virtually the identical pluralist equilibrium—consumers would have lost ground rather than gained it. This model of a budget-maximizing bureau constrained by oversight, then, is hardly a setup for consumer power. Its dynamics “naturally” move it to a pluralist equilibrium around an enforce-

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ment level of 169, regardless of where the system starts out.

Now let's introduce a committee into the legislature. Specifically, let's assume the committee has the authority each period to pose one-time, take-it-or-leave-it budgets to the full legislature under a closed rule, with failure of the committee's proposal resulting in automatic adoption of the prevailing status quo budget. Legislative decision making proceeds as follows. The committee's most desired budget is determined by a vote of its members, with the median member being decisive. The committee takes note of where the median legislator (in the full legislature) stands relative to both the status quo and the committee's ideal point. If the ideal budget is destined to win against the status quo, the committee simply goes ahead and proposes it. If, however, the median legislator prefers the status quo to the committee's ideal point, the committee adjusts by proposing the "closest" budget to its ideal that can win—which may often mean proposing the status quo itself. Thus, regardless of the preferences of the median legislator, the committee is always able to use its agenda powers to guarantee budgetary outcomes somewhere between the status quo and its own ideal point. (For further details concerning the committee, see the Appendix.)

Suppose the committee consists of the five most proconsumer members of the legislature. Agenda control allows such a committee to block any decreases in the budget that may be desired by the full legislature, and, should the full legislature favor increases in the budget, the committee can impose budgets that are still larger than the median legislator would have adopted had his agenda not been controlled. Over time, then, it would seem that a proconsumer committee with agenda-setting powers should be able to engineer much higher levels of regulatory enforcement for consumers.

In fact, things do not work out this way. Version 1-2 introduces this proconsumer committee into a model world that is otherwise identical to the one described by Version 1-1. As Table 1 shows, the committee fails to impose an equilibrium outcome more beneficial to consumers. The system simply moves to the same pluralist equilibrium as before—and again, this result is independent of the starting point.

What happened to the presumed power of agenda control? Why did the committee fail to impose a *structural equilibrium*?⁵ The answer is that the committee did indeed have substantial power over outcomes but only in the short run. Like other legislators, the committee members move toward moderation over time in response to constituency forces and, in their oscillatory adjustments, even occupy probusiness territory for a while. In the end, the median committee voter is virtually indistinguishable from the floor median, and the system's equilibrium properties are essentially unaffected by what would otherwise seem to be a major structural change.

The disappearance of what was initially a substantial degree of committee power testifies in dramatic fashion to the importance of recognizing the endogeneity of legislative policy preferences. Were legislators rooted to their initial policy positions, the committee's agenda powers would have been overwhelming and permanent, which is precisely what most models of agenda control lead us to expect. Once we allow legislators to respond over time to constituency influences, as they obviously do in the real world of politics, we gain a very different perspective indeed.

Agenda Control and Committee Capture

Clearly, if agenda control is to work for them, consumers must do something to

see to it that the committee does not abdicate its initial proconsumer position. Thus far, the model has been based on an assumption of symmetry: business and consumer groups implicitly follow exactly the same rules in allocating their resources to legislators. In particular, they both contribute for or against a legislator in proportion to the legislator's position along the budgetary scale (that is, according to the size of the budget increase or decrease he or she favors). The way for consumers to capture the committee, it seems, would be to single out committee members for special treatment—to direct more resources their way than their policy positions would otherwise dictate; if they are proconsumer, they should be disproportionately rewarded; if they are pro-business, they should be disproportionately punished.

To understand the consequences of asymmetric group behavior, it is better to begin by temporarily factoring out the element of agenda control. Thus, in Version 1-3 we assume that consumers alter their allocation rule in an effort to capture the committee but that the committee, in fact, has no power to set the legislature's agenda. Under this scenario, consumers are obviously making a mistake by concentrating their resources disproportionately on legislators who have no more power than their colleagues; and we would expect this to affect consumer policy success.

As outlined in Table 1, this is just what happens. On the assumption that consumers set aside 20% of their political resources for expenditure on committee members (over and above what they would normally receive), the system no longer arrives at the former pluralist equilibrium but rather comes to rest at a lower enforcement level of 145. This is not simply a lower-level pluralist equilibrium since consumers are in fact out-contributing business at that point. Nonetheless, it does represent a balance of

group forces in a very real sense. Because consumers are literally wasting some of their resources on powerless committee members, it takes more consumer resource units (dollars, if you prefer) than business resource units to achieve the same political impact. Thus, consumer and business resources can only come into balance, in a political sense, when consumers out-contribute business by precisely the "right" amount. When this occurs, the system comes to rest at what we might call a *modified pluralist equilibrium*, which the enforcement level of 145 in this case represents.

One way to look at this is in terms of opportunity costs. When consumers choose to contribute disproportionately to committee members, the opportunity cost is that they have less left over to contribute to all the other legislators, who will therefore be more susceptible to influence by business. The big question is, What do consumers get in return for paying this cost? The answer is that committee members have nothing extra to offer here since they are no more powerful than anyone else. Thus, consumers are forced to accept the consequences of an inefficient allocation rule—a lower, less desirable level of enforcement.

Now suppose, in Version 1-4, that we reendow the committee with agenda-setting powers. Here we have what would appear to be the ideal match: consumers allocate 20% of their resources to committee members who, in turn, have monopoly power over the legislature's agenda. The most likely scenario, it would seem, is that consumers will be successful in the legislature, enforcement will soar, and the modified pluralist equilibrium will be replaced by a structural equilibrium at this higher level.

Some of these expectations are borne out, as we illustrate in Table 1. Consumers do succeed in capturing the committee, which remains resolutely pro-consumer in equilibrium. The committee

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uses its power relentlessly in imposing budgetary outcomes more favorable to consumers than the median legislator desires, and the modified pluralist equilibrium is indeed replaced by a structural equilibrium: a perpetual standoff in which the median legislator prefers to reduce the budget and the committee proposes and achieves the status quo of no change at all.

But there is a fly in the ointment. While consumers achieve a somewhat higher level of enforcement here than they did in the absence of agenda control, moving the system from 145 to 151, this new level is still below the original pluralist equilibrium attainable when consumers ignore the committee altogether. Consumers do in fact receive a bit of political benefit in return for concentrating their contributions on the committee, but it turns out that these benefits of agenda control to consumers are "small" and not worth their price. The price, once again, is the opportunity cost of diverting resources away from legislators who are not on the committee. Though the committee is more proconsumer than it was in Version 1-2, when consumers did not give it special attention, the rest of the legislature is more probusiness than it had been. The net effect of these two opposed shifts is less enforcement than was achieved in the pluralist equilibrium in Version 1-2. Consumers are better off if they do not attempt to capture the committee at all.

These results contradict conventional wisdom on the efficacy of agenda control and the consequences of committee capture. What the usual accounts, whether theoretical or empirical, tend to overlook are the dynamic adjustments that these factors set off among all the other political participants—adjustments that, over time, undercut the initial advantages that agenda control and committee capture bestow. These other participants, non-committee legislators in particular, have a kind of power that is less obtrusive and

less quickly imposed than the committee's but that ultimately asserts itself with real force.⁶

Any model of agenda control contains these opposing powers, and the general issue is, How do their relative strengths vary as basic properties of the system are allowed to change? The results of Version 1-4 are important in their direct challenge to conventional wisdom, but there is no reason to think there may not be other conditions under which agenda control and committee capture do indeed work as well as we have been led to expect. The most prominent candidate is the initial status quo, which, by assumption, is the reversion level for committee agenda control. Given the demonstrated significance of the reversion level in other models of agenda control (see, e.g., Romer and Rosenthal, 1978), there is clearly reason to suspect that our results may change with the initial status quo.

Our suspicions have been wrong before in this regard. As we saw with Version 1-2, a committee with agenda-setting power cannot prevent movement of the system to its "natural" pluralist equilibrium, and this occurs regardless of the starting point. However, this came about because the committee abandoned its original proconsumer position and went to the center. If consumers took steps to capture the committee, as they did in Version 1-4, the committee would presumably remain proconsumer—and therefore be in a position to impose budgetary increases whenever opportunities arise. In Version 1-4, the committee did succeed in raising the initial level of enforcement through the imposition of higher budgets, but the initial level was so low (80) and its opportunities for budgetary largesse so few that the structural equilibrium (151) fell short of the pluralist equilibrium (169). This suggests that if the initial status quo had been above the pluralist equilibrium, a proconsumer committee could "lock in" these

high levels of enforcement and perhaps increase them.

We can test this by creating Version 1-5, which is identical to Version 1-4 except that it begins in the first period with a budget of 300 and a corresponding enforcement level of 240. As the figures in Table 1 indicate, our suspicions are correct: the result is a structural equilibrium at an enforcement level of 242, far beyond the pluralist equilibrium and even farther beyond the structural equilibrium of Version 1-4. Here, therefore, committee agenda power is indeed harnessed to serve consumer interests—but only due to the combination of asymmetric group contribution rules and a high reversion level. Both appear to play necessary roles. The contribution asymmetry produces a captured committee, while the high reversion level gives the committee a crucial tool with which to work: it can preserve and protect consumer interests by doing nothing more than blocking floor action to change the status quo. This it can do quite effectively. Clearly, then, the consumer victory amounted to a preservation of the status quo. As is consistent with the neoclassical models, agenda control and committee capture mounted a good defense, but little offense.

Defense, however, sufficed; the committee members had a major impact on the system, imposing a structural equilibrium very different from the original pluralist equilibrium and leaving consumers much better off than they would have been in the absence of committee power. So, while the consequences of agenda control are less dramatic and more highly qualified than some of the literature leads us to expect, it can obviously play a central role in shaping systemic outcomes, and it represents a resource of potentially great value for political participants.

Agenda Control and Group Allocation Rules

A potential resource for consumers is

likely to be a potential resource for business as well. If consumers can concentrate their resources on committee members in attempting to capture the committee and its agenda control powers, business can surely attempt to do the same. This brings up the general question of group strategies and their implications for systemic outcomes.

In this article, we do not model interest groups as unitary rational actors with motives and strategies in the usual sense. We think of them as large, decentralized sectors whose political resources are simply increasing functions of the sectoral benefits or costs imposed by bureaucratic enforcement, and whose allocations among legislators are governed by one or more rules that seem empirically plausible. The rationality of decision makers within sectors suggests there is presumably some relationship between what "works" in politics and how resources are allocated: if concentrating resources on committee members works, then this should tend to be reflected in resource decisions and allocations. Yet the cognitive limitations on decision makers and their distance from politics (in national legislative campaigns, for instance, most contributions come from ordinary individuals, not from Washington-based interest organizations) suggest that such relationships are likely to be imperfect. Thus, while the nature and extent of interest group support is clearly crucial to systemic outcomes in our model, we think it would be a mistake to require that sectoral resources are always generated and allocated for maximum political efficiency, that sectors truly have strategies, or that they engage in gamesmanship. At this early stage of theory construction, we prefer to think in terms of reasonable generation functions and allocation rules and to put them to use in exploring the systemic consequences of alternative patterns of interest group support.

To this point, we have concentrated on consumer allocations and have found that consumers can push the system

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above the pluralist equilibrium when three factors come together: committee agenda control, disproportionate consumer contributions to the committee, and a "high" initial status quo. If any one of these is lacking, the result is either the pluralist equilibrium or a still lower structural equilibrium. Had we concerned ourselves with business instead, the results would have followed identical lines. Business is only able to achieve enforcement levels below the pluralist equilibrium when the committee has agenda control; business funnels additional resources to the committee, and the initial status quo is "low." When any one element is absent, the result is either the pluralist equilibrium or a still higher structural equilibrium. Whether the committee is initially proconsumer or probusiness is of little importance; due to the endogeneity of legislative policy preferences, the committee quickly moves to the capturing group's side.

In this section, we want to explore the impact of group allocation rules further by taking up the basic question of the symmetry or asymmetry of group contributions. We know that when the committee has agenda control, asymmetric allocation rules—one group tries to capture the committee, the other does not—shape systemic outcomes. What happens, however, when both groups try to capture the committee by allocating equal portions (20%) of their resources to committee members? An answer is provided with the help of Version 1-6, which is identical to Version 1-5 (proconsumer committee with agenda control and initial budget of 300) but for the assumption of symmetric group allocation rules oriented toward capture. The result reported in Table 1 is striking: the structural equilibrium of Version 1-5 is shattered, giving way to the pluralist equilibrium. Thus, even though the committee is initially proconsumer and even though the status quo budget (300) and enforcement levels (240) overwhelmingly favor consumers,

symmetric group allocation rules bring the system back close to its "natural" equilibrium. Had we started out at a much lower status quo very unfavorable to consumers, moreover, the result would have been the same: the pluralist equilibrium.

Why does this happen? Why, in Version 1-6, does a proconsumer committee fail to use its agenda control to "lock in" the initially high status quo? It fails to do so because at a status quo well beyond the pluralist equilibrium, business is outcontributing consumers in the aggregate. With symmetric allocation rules, this means that all legislators, whether on or off the committee, tend to be more responsive to business than consumer interests. As legislators subsequently shift in a probusiness direction, it does not take long for consumers to lose all of their initial advantages. The committee's agenda control is exercised throughout, but it has no impact on the final result.

Asymmetry is therefore a fundamental determinant of whether agenda control will be of real consequence, for one group must capture the committee if agenda control is to alter the system's equilibrium. However, this only works as long as the other sector does not follow suit. If there is a tendency within the interest group system for relevant information to be disseminated, if groups and individuals tend to imitate one another in their allocations, if common rules of thumb tend to arise regarding contribution strategies, or if any other mechanisms are at work in the world that encourage boundedly rational contributors to behave according to the same basic rules, agenda control may be a fairly unimportant determinant of equilibrium outcomes.

The Spatial Model and District-Specific Resources

Empirically, we know there is an im-

portant institutional constraint on interest group support and opposition that tends to guarantee some, and perhaps a very substantial, measure of asymmetry. This constraint derives from the election of legislators from geographic districts.

In a system of electoral districts, political resources are not perfectly fungible. Some, like votes, are necessarily district specific. Many others, including money, are in principle free to flow across district boundaries, though they are also generated and allocated within districts. The degree of nonfungibility becomes important when political interests and resources are unevenly distributed across districts, as they typically are, of course, in American politics: farmers, blacks, blue-collar workers, Catholics, and other groups of political relevance tend to be concentrated in some districts and areas of the country and underrepresented in others. Nonfungibility and concentration combine to guarantee that candidates for office are subjected to differing patterns of support and opposition depending on the types of interests that are concentrated in their districts and on the relative tendencies of these groups to mobilize resources.⁷

Conventional analyses of legislative agenda control assume a political world of electoral districts and thus, implicitly, one in which interest groups are constrained to behave asymmetrically. In addition, they generally take as their paradigmatic case the "high demand" committee—a committee whose members come from districts that, due to concentrated interests and resources, generate pressure for high levels of the policy output controlled by the committee. While these models of agenda control are static, they are supported by a familiar and certainly reasonable story about system dynamics: constituency concentration within districts promotes committee capture through the electoral connection; capture then guarantees that the committee's

preferred level of output will depart (substantially) from that of the median legislator, and committee agenda powers are then put to use to generate disproportionate benefits for the concentrated group.

From this perspective, our mobile resources model may appear quite inadequate for investigating agenda control. By ignoring electoral districts and allowing resources to flow freely throughout the system, we undercut a crucial foundation for both constituency capture of the committee and the committee's exercise of agenda powers, and it is hardly surprising that we have been led to emphasize the weaknesses of agenda control. It would seem that an alternative institutional model—one founded on electoral districts—should lead us in just the opposite direction.

Is this in fact the case? To investigate, let's assume each legislator is elected from a single-member district and that all political resources are constrained from crossing district boundaries. As in familiar spatial models of voting (Downs, 1957), what now determines each legislator's electoral security is the difference between the legislator's own policy position and the bliss point of his or her district's median voter. Electoral security is a single-peaked function of this difference, reaching a maximum when the legislator's position equals his or her pivotal voter's ideal point.

Each district's median voter is endowed in period one with a permanent bliss point defined in terms of enforcement. In the analysis to follow, we adopt bliss points ranging from 100 for the most probusiness district to 200 for the most proconsumer, with the median district having a bliss point equal to the social optimum of 150.⁸

Because only votes matter, politicians are solely concerned with finding the political center of gravity in their districts—their median voters. Further-

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Table 2. Computer Runs of Model 2: District-Specific Resources

	Simulation Version				
	A	B	C	D	E
Parameters					
Committee has agenda authority?	no	yes	yes	yes	yes
Initial budget and enforcement	very low	very low	very high	bit low	bit high
Outcomes					
Enforcement	150	197	197	170	171
Efficiency	.62	.81	.81	.82	.82
Type of equilibrium ^a	FM	CM	CM	SE	SE

- ^a FM = floor median
 CM = committee median
 SE = structural equilibrium

more, because each district's center of gravity is constant, the incentives facing each politician are unchanging: a proconsumer district remains proconsumer, a probusiness district remains probusiness. Consequently, unlike the model of the first part of this article, there are no forces here that would move a proconsumer committee to the center of the policy spectrum. The committee is captured by proconsumer voters in the first period and remains so throughout. Thus, policy differences between the committee and the floor median will persist. As in neoclassical models of agenda control, the essence of this game is continual struggle between floor and committee.

Let's begin by considering the null case in which the committee has no agenda control. We already know that when resources are perfectly mobile, enforcement moves to the pluralist equilibrium regardless of the initial status quo. What happens when resources are constrained by district boundaries? As the figures for Version 2-A in Table 2 illustrate, the system behaves in a thoroughly neoclassical manner: after hunting about, it eventually stabilizes at the enforcement level desired by the median voter in the median district.⁹ Further experimentation would

easily show that this general result applies regardless of the location of the ideal point and regardless of the initial status quo.¹⁰ In these fundamental respects, Model 2's behavioral dynamics—while anchored in the adaptive adjustments of myopic, imperfectly informed participants—are completely consistent with the basic median voter results of static neoclassical models.

Now suppose the committee is given the authority to control the legislature's agenda under a closed rule. Neoclassical models of perfect information yield straightforward predictions in such cases. With a proconsumer committee whose ideal point is above the status quo, for instance, the committee would (1) impose the status quo if the floor median is less than the status quo, (2) impose its own ideal point if the floor median is somewhere beyond halfway between the status quo and committee median, and (3) impose a compromise position whose distance from the status quo is twice that of the floor median if the floor median is less than halfway between the status quo and the committee median (Krehbiel, n.d.).

Our model is not based on perfect information. Legislators are handicapped by not knowing their districts' ideal points; they must group their way toward

them in trial-and-error fashion based on electoral feedback. Naturally, because legislators do not know the exact location of their own districts' ideal points, they do not know those of their colleagues either. This means that committee members do not know where the floor median would like to wind up, for even the floor median does not know that. We do assume, however, that in every period the committee knows the current budgetary position of the floor median and that budgetary preferences are symmetric.

Despite the uncertain complications that incomplete information and trial-and-error behavior would seem to involve, the model turns out to behave very much in the manner of conventional neoclassical models. In Version 2-B, in which the floor median (150) is closer to the committee median (198) than it is to the initial status quo (80), the system ultimately moves to an equilibrium (197) virtually identical to the committee's ideal. In Version 2-C, the initial status quo is shifted to 240, and the committee once again achieves an equilibrium nearly equal to its ideal. In both cases, this is just what static neoclassical models of complete information would have predicted.

In Versions 2-D and 2-E, the initial status quo is much closer to the floor median, giving the committee far less opportunity for agenda manipulation. In each case, the nature of the result is again what the neoclassical model would predict: a compromise solution representing a standoff between a committee median that is in favor of more regulation and a floor median that is in favor of less. Numerically, these equilibria diverge a bit more from precise neoclassical predictions than did those of 2-B and 2-C, but because the committee's uncertainty about legislative preferences leads to a trial-and-error search that is not precisely on target, some divergence is to be expected. The significance of the results is that the pattern is quite clearly neoclassical.

Mobile and District-Specific Resources

We can combine the two models by allowing for the relevance of both kinds of resources. Each legislator comes from an electoral district characterized by a particular distribution of voter preferences, and, other things being equal, the legislator's electoral security is enhanced the closer his or her own policy positions are to the district median. But now, other things need not be equal, for the legislator's electoral security is also affected by the allocation of mobile resources across all legislators by business and consumer interest groups, and this source of benefits could lead him or her to adopt positions that depart substantially from the district's ideal point.

A legislator's relative responsiveness to district and "national" forces depends on how he or she weights the two in his or her calculus. For purposes of illustration, we present results for versions in which the two sources of utility receive roughly equal weight, in the sense that a unit percentage change in either has the same impact on a legislator's subjective index of electoral security. The model accommodates, of course, any set of weights we might want to explore.

The versions in Table 3, which simply parallel the versions set out in the first part of this article and in Table 1, provide a summary of results for this combined resource model. Because neoclassical models of district-specific resources are the conventional foundation for analyses of agenda control, two points merit emphasis. First, even though the political world is organized by districts, some resources are now mobile, thus reintroducing the influential role of opportunity costs. When consumers set aside some portion of their mobile resources for the purpose of capturing the committee, as in Version 3-4, they do indeed help create a proconsumer committee—but

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Table 3. Computer Runs of Model 3: Mobile and District-Specific Resources

	Simulation Version					
	1	2	3	4	5	6
Parameters						
Committee has agenda authority?	no	yes	no	yes	yes	yes
Interest groups allocate extra resources to committee?	neither	neither	consumers	consumers	consumers	consumers and firms
Initial budget and enforcement	low	low	low	low	high	high
Outcomes						
Enforcement	157	163	146	149	218	177
Efficiency	.83	.78	.77	.81	.81	.80
Type of equilibrium ^a	PE/FM	PE/SE	MPE/FM	PE/SE	SE	PE/SE

- ^a PE = pluralist equilibrium
MPE = modified pluralist equilibrium
SE = structural equilibrium
FM = floor median

resources allocated to committee members are resources not allocated to other legislators on the floor, who respond by shifting against consumers. The net effect is less enforcement than consumers could have achieved had they not made any special efforts to enhance their control over the committee (Version 3-2).

Second, consumerists' efforts to capture the committee do combine with a high initial status quo to yield enforcement levels beyond those predicted by the neoclassical model (Version 3-5), but symmetrical behavior by business does more than simply destroy the consumer advantage. While the neoclassical model would predict an equilibrium at the committee median, the actual equilibrium under conditions of symmetry is a compromise between the committee median and the lower pluralist equilibrium (Version 3-6). Thus, symmetry does not "cancel out" the consumer advantage and restore the committee median. Instead, it imposes the pluralist equilibrium as a competing point of attraction that pulls the system toward

a more probusiness solution unanticipated by the neoclassical model of district-specific resources.

Empirically, of course, we do not know the relative weights of mobile and district-specific resources. Strictly speaking, they doubtless vary across legislators and districts, and historical developments—for example, the rise of political action committees (PACs)—have certainly influenced their values over time. What our model can do is explore the systemic implications of the various possibilities. In this regard, as we could show with further experimentation, it exhibits an important continuity property. When mobile resources are increasingly important in the legislator's calculus, the equilibria are increasingly close to pure pluralist equilibria. When district-specific resources are increasingly important, equilibria approach those of the pure spatial model. In general, there is a tension between the pluralist and neoclassical equilibria whose resolution depends upon the relative salience of the two types of resources.

Conclusion

Our purpose here is to encourage new perspectives in the study of agenda control, committee capture, and institutional politics more generally. Neoclassical approaches have shed important light on these matters and doubtless will continue to do so. Indeed, our own analysis of district-specific resources testifies to the scope and power of neoclassicism, for its simple, static models of complete information are already capable of generating conclusions about systemic equilibria, agenda control, and committee capture virtually identical to those produced by our far more complicated model of adaptive adjustment.

When resources are mobile, however, a static focus on the dyadic core of agenda control—in this case, the relationship between a legislature and a captured, agenda-setting committee—can be misleading. When this relationship is placed within its larger institutional context, when legislators' policy positions are seen as endogenous functions of constituency influences, and when institutional dynamics are allowed to play themselves out over time, we begin to see agenda control and capture in a different light: as part of a system of political behavior in which action provokes counteraction, power begets adaptation, and all political actors are ultimately bound up with one another.

Viewing these issues from a broader institutional perspective suggests several basic conclusions:

(1) Agenda control works quite effectively under some conditions and not at all well under others. Because of the very nature of politics, a committee endowed with formidable powers may find that the system's internal dynamics tend to drain these powers away, to the point where its exercise of agenda control has little or no effect on political outcomes. With a few exceptions (Enelow and Hinich, 1984b;

Krehbiel, n.d.; Miller, 1984), the literature tends to overemphasize its political impact.

(2) The wisdom and efficacy of the capture of committees by interest groups are also overemphasized. Groups can indeed capture legislative committees, but their corresponding influence over policy may be short-lived and misleading; over the long haul, institutional dynamics may well promote policy outcomes that are less desirable than those that could have been obtained by ignoring the committee entirely.

(3) For agenda control to work effectively when mobile resources are dominant, it appears that the institutional rules granting a committee agenda-setting powers must be accompanied by two facilitating conditions: asymmetric group behavior that allows one group to capture the committee and, an initial status quo sufficiently high or low relative to the pluralist equilibrium.

(4) When district-specific resources are dominant, agenda control seems to operate just as neoclassical models have claimed, even in a world of incomplete information and trial-and-error adaptation.

(5) When both mobile and district-specific resources are important motivators of legislative behavior, institutional dynamics—and, with them, the efficacy of agenda control—reflect an underlying tension between pluralist and neoclassical equilibria.

These conclusions are derived from an adaptive modeling perspective that is broadly anchored in the Simon-March tradition of organizational analysis. While this tradition has so far been put to little use in the formal analysis of agenda control, we believe that its ideas and methods have much to contribute.¹¹ Whatever orientation is adopted in future work, however, it seems clear that we must ultimately move toward broader models of institutional politics—models that recognize the agenda-setter as but

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one component of a larger, dynamic whole.

Appendix

Description of the Model

The model has been described in detail elsewhere (Bendor and Moe, 1985); here we sketch out the period-by-period iterations. Readers interested in the finer details may write to the authors for a copy of the computer program.

Every period the legislature gives the bureau a new budget. Each legislator must vote on the budget. Legislators' decisions are determined by how their previous choices affected their electoral security. If they became more proagency last period and their electoral security subsequently increased, then they will take another step in this direction, with the size of the step determined by the size of their utility gain. The same adaptive logic applies to the other permutations.

Once all the legislators have determined their positions, the five committee members vote on a committee bill, with the median of the five being decisive. (We also use a benchmark model without a committee, wherein the budget is determined by the median legislator.) Because the committee has the authority to pose take-it-or-leave-it alternatives to the floor, the committee's budget is paired against the status quo appropriation.

Once the floor is presented with the committee's alternative, the median legislator decides the final outcome. For example, if the committee wants to increase the budget and the median legislator wants to decrease it, the committee's bill is voted down and the agency receives the previous period's appropriation. If the median representative wants a 1% increase and the median committee member wants a 5% boost, then we

assume that the committee sophisticatedly proposes an increase just under 2%, which the floor passes.

The committee members behave adaptively, in that their votes are trial-and-error responses to changes in their electoral fortunes. In this sense, they behave just as their colleagues do. However, because committee members are specialists, they not only know more about the substance of an issue than their colleagues do but also have a better grasp of the political lay of the land. Therefore, our committee is assumed to know the "win set" (Shepsle and Weingast, 1981) against the status quo. (This implicitly means knowing the median legislator's ideal point and that he or she has symmetric preferences.) We also assume that the committee acts sophisticatedly within the win set. These assumptions about the committee's knowledge, its sophisticated strategy, and closed rule authority are strong ones. (See Enelow and Hinich [1984a] for a criticism of the complete information assumptions of many agenda models; see Krehbiel [1986] for some experimental evidence on sophistication.) We have tilted the balance of advantages in favor of the committee. We believe, however, that doing so makes the model's central results that much more interesting.

In addition to passing a budget, the legislature conducts rudimentary oversight, as described in the text.

The legislature's work ends once it sets the new levels of budget and oversight. These new quantities now become input to the agency's utility, telling it whether the environment approves of its past behavior. Its utility function contains four potential arguments: budget, slack, oversight, and policy (an ideal enforcement level). When relevant, budget and slack increase utility; oversight and departure from the agency's ideal point reduce it. Once the agency knows its utility change, it adaptively sets a new efficiency level. Thus, if it increased its efficiency last

period and its utility rose, it will increase efficiency again in the current period.

Efficiency is defined as the fraction of the budget spent on enforcement, and enforcement is a linear function of the money spent on it:

$$\text{enforcement} = c \cdot \text{efficiency} \cdot \text{budget}, \quad (1)$$

where c , a constant, denotes the units of enforcement purchased by a dollar of spending.

The new level of enforcement yields new levels of costs for business and benefits for consumers. The business costs imposed by regulation are convex (increasing marginal costs) while benefits to consumers are concave (decreasing marginal benefits). Specifically,

$$\begin{aligned} \text{benefits} &= k_1 \cdot \text{enforcement} \\ &- k_2 \cdot \text{enforcement}^2 - \text{budget} \end{aligned} \quad (2)$$

$$\text{costs} = k_3 \cdot \text{enforcement}^2, \quad (3)$$

where k_1 , k_2 , and k_3 are positive constants.

In the mobile resources model, these benefits and costs prompt consumers and firms to make political contributions to legislators. Consumers' total contributions increase linearly in benefits; firms' total contributions increase linearly in costs:

$$\begin{aligned} \text{consumers' political resources} &= k_4 \\ &+ k_5 \cdot \text{benefits} \end{aligned} \quad (4)$$

$$\begin{aligned} \text{firms' political resources} &= k_6 \\ &+ k_7 \cdot \text{costs}, \end{aligned} \quad (5)$$

where k_4 through k_7 are positive constants. To represent the organizational advantages of firms, we set $k_7 > k_5$.

Once business and consumer groups decide upon resource levels, they expend them for or against legislators in accord with the legislators' positions on the

budget. Firms contribute to the campaigns of legislators who vote for budgetary decreases and to opponents of legislators who vote for increases. Consumers typically do the opposite. (They will support legislators voting for smaller budgets if the taxes funding the agency outweigh the program's benefits). In each case, contributions to a legislator's campaign kitty increase in the absolute value of his or her vote.

In the pure district-specific resources model, each legislator's electoral security is a single-peaked function of the difference between his or her vote on the budget and the ideal point of his or her district's median voter. However, because politicians vote on the budget rather than on enforcement directly, the voters must estimate their ideal points defined over budgets. They use the following linear rule of thumb:

$$\begin{aligned} \text{budget bliss}_{i,n} &= \frac{\text{enforcement bliss}_i}{\text{enforcement}_n} \\ &\cdot \text{budget}_n, \end{aligned} \quad (6)$$

where subscript i denotes the i^{th} district and n denotes the period.

Whichever resource is active, legislators must adapt to altered patterns of electoral support and opposition, for as they change their votes on the budget, the interest groups and voters adjust their evaluations accordingly. Hence, changes in the legislators' utilities are anchored in these adjusted evaluations of groups and voters. This new feedback prompts the politicians to adapt by choosing a new budgetary position for the coming period, following the decision rule outlined above. This completes the circle and sends the process into another iteration.

Initializations of the Model

In the pure mobile-resources model, each legislator's first vote on the budget

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equals the status quo ante budget plus a normally distributed random variable of mean zero and standard deviation of four. In the pure fixed-resources model, the legislators' initial positions equal their respective districts' budgetary bliss points plus a normally distributed disturbance of mean zero and standard deviation of five. In the combined resource model, the legislators' initial positions are weighted averages of their positions in the pure models. The weights reflect the relative importance of the two resources. For example, if the mobile resource is negligibly important, then the median legislator's initial position is negligibly different from his or her position in the pure spatial model.

Notes

An earlier version of this paper was presented at the Stanford Conference on Adaptive Institutions, November 1984. We would like to thank John Ferejohn, Morris Fiorina, Keith Krehbiel, Roger Noll, John Padgett, Ken Shepsle, Serge Taylor, Barry Weingast, and the other conference participants for their helpful comments.

1. For example, Shepsle and Weingast (1984) have recently examined the potency of agenda setting when all decision makers, not just the agenda setter, are sophisticated; Denzau and Mackay (1983) and Enelow and Hinich (1984a) have studied how uncertainty about preferences influences agenda-setting behavior.

2. In addition to passing a budget, the legislature engages in rudimentary oversight, which is activated whenever the agency produces less programmatic activity with a bigger budget. The intensity of oversight increases as the legislature becomes more pro-consumer.

3. For terminological convenience, when only mobile resources matter, we refer to firms and consumers as *interest groups*; when only the fixed resource counts, we refer to them as *voters*.

4. Most neoclassical theories assume that group resources are either completely transferable (see Becker, 1983) or completely district specific (see Shepsle, Weingast, and Johnsen, 1981). For an interesting and unusual combination of these two properties, see Denzau and Munger (1986).

5. Readers noting that an equilibrium exists in this unidimensional policy space, even when the committee is inactive, have suggested that we avoid the term *structure-induced equilibrium*. Discussions with Kenneth Shepsle indicate that he invented that term primarily to refer to equilibria created by institutions out of the potential chaos of majority rule cycles.

6. This opportunity cost of committee capture would also appear in models where the decision makers were cognitively perfect, as they typically are in neoclassical theories. Thus, the perverse effect of paying special attention to the committee follows from the structure of the model—that is, from mobile resources and the endogeneity of the legislators' policy positions—rather than from assumptions about individuals. Of course, completely informed and completely rational agents will, in the face of opportunity costs, always make optimal allocations to the committee and floor whereas boundedly rational agents may persist in a sub-optimal allocation because they do not understand the system.

7. It is unclear to what extent political resources are in fact nonfungible, for there is little evidence on the matter. However, the growth of well-organized, nationally based political action committees (PACs), which apparently pay close attention to voting records of congressmen (Poole and Romer, 1984), may indicate a trend towards greater fungibility.

8. Because the politicians vote on the budget rather than on enforcement directly, the voters must estimate their ideal points defined over budgets. They do so via a simple linear rule of thumb (see Appendix), by means of which, for example, if a district's median voter wants twice as much enforcement as the agency is currently generating, then he or she wants the legislature to appropriate twice as much money. This rule of thumb embodies relatively little knowledge of how the agency generates enforcement. It does, however, have several desirable qualitative properties. First, if enforcement equals a voter's bliss point, then that voter is perfectly satisfied with the existing budget. Second, if the voter prefers more (less) enforcement than exists, he or she wants a greater (lesser) budget.

9. Because the interest groups in the spatial model cannot allocate extra resources to the committee, this parameter is not in Table 2. To underscore this difference between the two models, the versions in Table 2 are labelled alphabetically rather than numerically.

10. Furthermore, when the median district stays constant but the other districts' bliss points become more dispersed, the equilibrium enforcement is unchanged. However, there is an effect on the individual legislators' equilibrium votes on the budget: as the districts' median voters spread out, so do the legislators' positions. This position-taking effect has no institutional consequences, but does enhance each legislator's electoral security in his or her own

district. All of this is just as neoclassical spatial models would predict.

11. In particular, we suspect that extending adaptive models into the context of a multidimensional policy space would sharpen the differences between behavioral and neoclassical theories, perhaps, in the process, undermining the convergence we observed here under conditions of district-specific resources. Briefly, because searching for alternatives in a multidimensional world is complex, boundedly rational agents would simplify the problem by using heuristics such as satisficing ("do not propose a new policy if the status quo is sufficiently good"). Given these cognitive constraints on the generation of alternatives, a legislature composed of adaptive politicians would probably be more stable than a neoclassical chamber, particularly when both operate under an open rule. It appears promising to investigate such "cognitively induced equilibria" (CIE). (See Riker, 1982, pp. 209-11, for some interesting remarks about a model of politicians' trial and error in a multidimensional space.)

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NEITHER THE PURSE NOR THE SWORD: DYNAMICS OF PUBLIC CONFIDENCE IN THE SUPREME COURT

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Systematic study of changes in support for the U.S. Supreme Court across time has not been undertaken. Armed with a time series of observations from 1966 through 1984, I provide a description of the ebb and flow of public esteem for the Court. Then I outline and test several plausible propositions about the dynamics of support. Statistical analyses compel the conclusion that apart from a relatively constant core of support, increases in judicial activism, inflation, and solicitude for the rights of the accused decreased confidence in the Court; the events surrounding Watergate and increases in presidential popularity and the public salience of the Court brought about increased popular esteem for the high bench. Previous scholars, based on cross-sections of individuals, have emphasized the public's ignorance of and disinterest in the Supreme Court and judicial policy making. The responsiveness of public support for the Court in the aggregate to political events and shifts in the behavior of the justices stands in stark contrast to the conventional image of United States citizenry as singularly out of touch with and unmoved by the Supreme Court.

In the 1960s and early 1970s, at the same time as and perhaps because of the political and social chaos and the apparent decline of faith in basic values and rules, a small but prolific body of political scientists focused attention on public support for a variety of political institutions. Naturally, because of the importance and visibility of the office and the ready availability of data, the president and presidency claimed the bulk of scholarly energies (Kernell, 1978; Mueller, 1973). Nonetheless a few hearty students of the courts (e.g., Dolbeare and Hammond, 1968; Murphy and Tanenhaus, 1968a, 1968b) have attempted to discover the levels, depth, and bases of public support for judicial institutions among publics in the United States (for work on other institutions, see Dennis,

1966, 1975a; Parker and Davidson, 1979; Patterson, Hedlund, and Boynton, 1975).

The lack of any formal connection to the electorate and its rather demonstrable vulnerability before the president and Congress mean that the United States Supreme Court must depend to an extraordinary extent on the confidence, or at least the acquiescence, of the public. Mr. Justice Frankfurter stated the quandary more eloquently: "The Court's authority—possessed of neither the purse nor the sword—ultimately rests on sustained public confidence in its moral sanction" (*Baker v. Carr* [1962]; see also *United States v. Lee* [1882]). Virtually all scholars agree that the Supreme Court plays a crucial role in the making of national policy (Casper, 1976; Dahl, 1957; Shapiro, 1979), but if the Court is the

"least dangerous branch," how do the justices manage to hold sway much of the time even in the face of substantial "law-making majorities" (cf. Barnum, 1985)? To account for this anomaly, some "commentators have attributed the Court's endurance as a national policymaker to its special status with the public" (Adamany and Grossman, 1983, p. 406).

For the most part, lamentably, empirical researchers have painted a dreary and unencouraging portrait of public attitudes toward the Court. Citizens know surprisingly little about the Court and the workings of the judicial branch, manifest scant concern about its personnel and about most decisions, and offer support contingent upon agreement with specific public policies (for reviews, see Adamany, 1973; Daniels, 1973). This brief set of generalizations, understandably enough, stems from data collected in cross-sections of the public. Yet, despite the widespread consensus these days about the importance and desirability of developing an understanding of the dynamics or temporal dimension of public opinion and other political phenomena, no one has undertaken a study of changes in public support for the Supreme Court across time (cf. Handberg, 1984). Fortunately, since 1966 Louis Harris Associates and the National Opinion Research Center have queried members of the American public on a relatively regular basis about confidence in a number of institutions, including the Court. In this article, armed with a time series of observations from 1966 through 1984, I provide a description of the ebb and flow of public esteem for the Court and then develop and test several plausible propositions about the dynamics of support.

I begin by reviewing some of the previous work on public evaluations of the Supreme Court and by briefly introducing the essentials of patterns of confidence in the Court from 1966 through 1984. I then present alternative explanations of confi-

dence in the Court and, finally, statistical results and interpretations. The statistical analyses lead me to conclude that increases in judicial activism, inflation, and solicitude for the rights of the accused decreased confidence in the Court; the events surrounding Watergate and increases in presidential popularity and in the public salience of the Court brought about increased popular esteem for the high bench. The responsiveness of public confidence in the Court to political events and to shifts in the behavior of the justices stands in stark contrast to the conventional image of U.S. citizenry as singularly out of touch with and unmoved by the Supreme Court.

Public Evaluations of the Court

Prior to the advent of modern political science, constitutional commentators made intriguing claims about the public's regard for the Supreme Court. Thus, for example, Lerner (1937) waxed eloquent in a classic essay about a deep-seated, psychological need for security among the public that was required in the United States by worship of the Constitution and transferred to the justices as interpreters of the basic law (see also Baas, 1979, 1980; Baas and Thomas, 1984; Jackson, 1954, p. 23; Levinson, 1980; Miller, 1965). This image of a revered and deified Supreme Court gathers scant support in the much more critical and human portrayals in the empirical research conducted since the 1950s. To be sure, in a study of the socialization of children, Easton and Dennis (1969, p. 278) lent some empirical credence to the views Lerner (1937) and others had articulated, but based on analysis of children's responses to open-ended questions, Caldeira (1977a, p. 864) found no evidence that children possess any detailed knowledge of the Court and its functions or see the

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justices as particularly omnipotent, omniscient, and beneficent; for the most part, children view the Court with indifference.

Research on the attitudes of adults reveals that there is only a relatively shallow reservoir of knowledge about or affect toward the Court in the mass public. Thus, for example, less than half of the public in 1964 and 1966 could name one or more specific decisions of the Court (Murphy and Tanenhaus, 1968a, p. 277; see also Casey, 1976; Dolbeare, 1967; Kessel, 1966). Many apparently clung to a belief in the myth of mechanical jurisprudence in the Court (Casey, 1974). Few members of the public, regardless of the place or time of the sample, fulfill the most minimal prerequisites of the role of a knowledgeable and competent citizen vis-à-vis the Court. Yet on more general evaluations of judicial performance, the public has, for the most part, given the Supreme Court more favorable than unfavorable marks (Dolbeare, 1967; Kessel, 1966; Murphy and Tanenhaus, 1968a). From the analysis of national surveys executed over a period of 30 years (1937-1966), Dolbeare and Hammond (1968) contend that the Court in fact enjoys widespread, though probably shallow, respect among the public (see also Dennis, 1975b; Dolbeare, 1967; Kessel, 1966). On the other hand, scholars report that favorable evaluations of the Court's performance have declined (Dolbeare, 1967; Murphy and Tanenhaus, 1968a; Tanenhaus and Murphy, 1981).

If the public refuses to offer unstinting praise of the Court's performance in general, citizens consistently cast an even more jaundiced eye on specific decisions of the justices. Asked about particular decisions of the Court, respondents to a national survey in 1966 volunteered three times as many negative as positive remarks (Murphy and Tanenhaus, 1968a, p. 287; cf. Adamany and Grossman, 1983, p. 411; Dennis, 1975b, p. 20). Over

the 10 years of Murphy and Tanenhaus's panel (1966-1975), the ratio of dislikes to likes declined to a slight extent, indicating perhaps a modest increase in support for specific policies of the Court (Tanenhaus and Murphy, 1981, p. 31). On the visible issues of the day, however one couches the issues, most people find fault with the choices the Court makes.

Scholars have offered a number of explanations for patterns of public support. For example, Murphy, Tanenhaus, and Kastner (1973, p. 50) identify "absence of concern about federal power, youthful idealization, political partisanship, a view of government as a monolith, sympathy for the underdog, liberalism, and agreement with particular decisions" as chief candidates in the search for an understanding of diffuse support. Yet, with all said and done, they conclude that attitudes toward public policy, regardless of the source, provide the best explanation of support. In effect, individuals classified on several items as "liberals" approved of the Court's policies, and the "conservatives" did not. Dolbeare and Hammond (1968), based on earlier studies, purported to show a relationship between partisan identification and association with the president on the one hand and support for the Court on the other. Democrats and adherents of the incumbent chief executive manifested significantly more support than did their counterparts (see Casey, 1976; Dolbeare, 1967, pp. 205-6). Still others suggest a connection between support for state courts and support for executives and legislatures (Lehne and Reynolds, 1978). At various times and places the following have emerged as significant determinants of support: political activism (Adamany and Grossman, 1983); membership in an elite (Beiser, 1972; Caldeira, 1977b; Murphy and Tanenhaus, 1970; Murphy et al., 1973, pp. 45-51; Schmidhauser and Berg, 1972); level of education (Casey, 1974; Murphy and Tanenhaus, 1968a); race (Hirsch and

Donohew, 1968); age (Kessel, 1966; Murphy and Tanenhaus, 1968a; Murphy et al., 1973); and social status (Casey, 1974). Subsequent analyses suggest, however, that beliefs about proper public policy tend to overwhelm demographic influences.

Measuring Support for the Supreme Court

During the storm over Franklin D. Roosevelt's "Court-Packing Plan" in 1937 and in the aftermath of *Brown v. Board of Education*, pollsters invested a fair amount of expense and energy in monitoring public attitudes toward the Supreme Court repeatedly over an extended period of time—in the former case, throughout 1937; in the latter, from 1954 through the early 1960s (Handberg, 1984). Because the interest of elite and mass publics inevitably moved on to new topics, polling operations soon dropped questions on the Court. The wording of items on the Court shifted with the focus of controversy. Thus, even though we now have data on public opinion and the Court from 1935 to the present, we have only recently begun to accumulate a lengthy string of comparable observations. Time-series regression, so often utilized with good effect in studies of presidential popularity, requires a substantial number of observations. Until recently, therefore, scholars of the Court could not study public support systematically across time.

In February of 1966, Louis Harris Associates, for the first time, asked a national sample of Americans, "As far as the people running the Supreme Court are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?" Nearly a year later, Harris posed the question again and then dropped the matter entirely until August of 1971. In

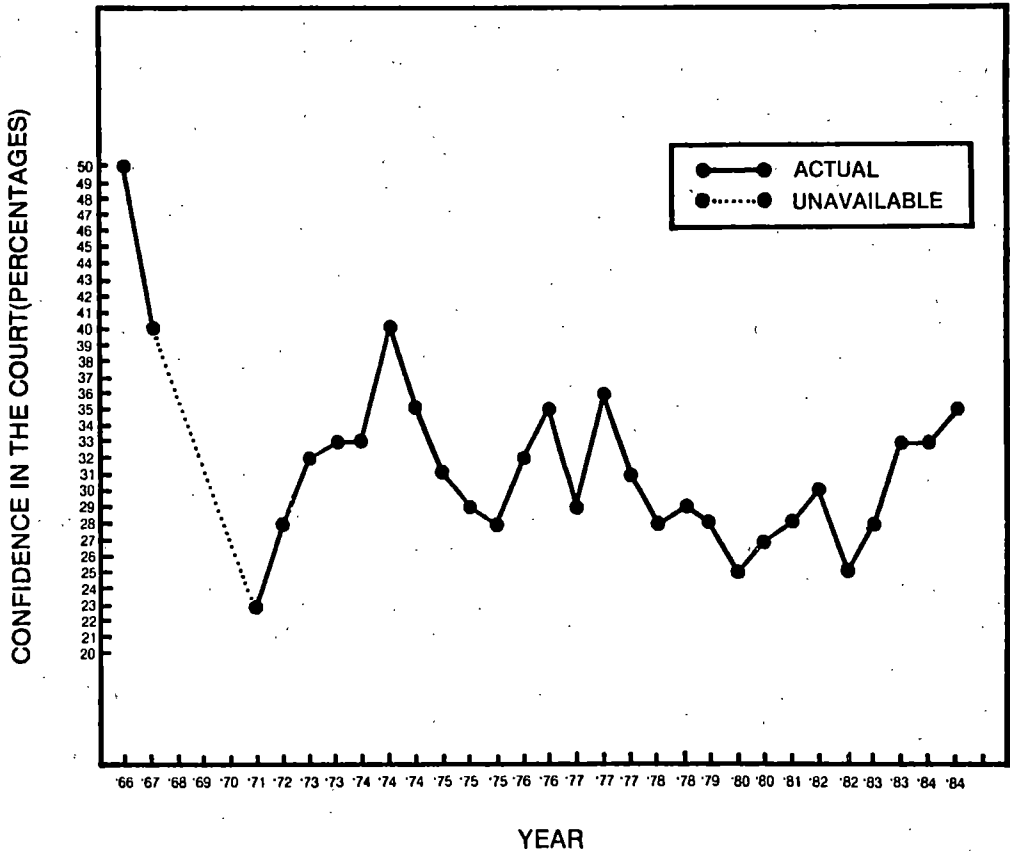
1973 the National Opinion Research Center (NORC) began to ask the question on an annual basis in the General Social Survey, missing only one year in the last decade. Over the period from 1966 through 1984, NORC and Harris have asked the same question in 29 separate surveys. For students of the Court, these 29 points constitute a rich lode of data. To be sure, one wishes for a larger number; after all, research on public evaluations of the president often includes hundreds of observations. Accordingly, I have taken particular aim at the parsimonious selection of independent variables.¹

To measure support for the Court, I have opted for the percentage of each sample who responded with "great confidence" in the justices. Figure 1, which displays this number from 1966 through 1984, illustrates the ebb and flow of public support for the Supreme Court. To place public attitudes toward the Court in a broader context, I shall also call attention to materials on Congress and the executive branch (Harris, various years; National Opinion Research Corporation, various years).²

Quite clearly, public confidence in the incumbents of the Court has taken a sharp secular decline from 1966 through 1984. More precisely, during this period support has on average dropped 2.2% each year ($Y = 35.5 - 2.2 \cdot \text{Time}$; $r^2 = .50$). Whereas about half of the public registered great confidence in the Court early in 1966, only one-third did so during 1984. Even if one considers 1967 as the base, support has dropped by a rather considerable amount. Public confidence in the Court hit rock bottom in 1971 and reached apogees in 1966 and 1974. Of course we have no way of knowing whether the degree of support recorded in 1966 typified public evaluations in the mid-1960s or whether it was entirely out of step. Confidence in the incumbents of the Court, unlike diffuse support (Handberg, 1984; Tanenhaus and Murphy,

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Figure 1. Patterns of Confidence in the Court: 1966-1984



1981), has varied a great deal, as the jagged pattern attests. For the entire period, support has a mean of 32%, a standard deviation of 5.5, and a range of 27%. Much of the variation, to be sure, stems from the rapid decline in the late 1960s. If, for example, we restrict our attention to the 1970s and 1980s, confidence in the Court actually ranges from 23% to 40%, for a mean of 30.5 and a standard deviation of 4.0.

Yet confidence does not bob around randomly; change comes gradually. In the late 1960s and the early 1970s, the decline and then rebound of confidence in the Supreme Court marched in step with patterns for other political and social institu-

tions. For the Court, "Watergate" translated into a major temporary gain of public support. Judges and courts, after all, had stood as bastions of the rule of law against the attempts by Nixon and associates to evade justice. Paradoxically, though Congress played a more central role in Watergate than did the Court, the public gave the incumbents of the national legislature much less credit. By 1978, confidence in the Court was again on the decline, perhaps reflecting a general discontent with institutions caused by economic instability. In the last four surveys, support for the Court, along with that for Congress and the president, has increased markedly, consonant with

the many reports of an increase in optimism since the midpoint of the first Reagan administration. Finally, across the entire period, the public has bestowed greater confidence in the incumbents of the Court than in those of Congress and usually by quite a large margin.

Explanations of Support for the Court

Thus far I have established that support for the Court varies considerably over time; of this there can be little doubt. Now, the more significant issue is, What, precisely, accounts for these perturbations in public confidence? It is conceivable, on the one hand, that support for the Court responds to changes in societal conditions: as the quality of life declines, so, too, does faith in the justices. On the other hand, perhaps members of the public react to events, politics, and judicial behavior. Large happenings on the political landscape could shape public perceptions of the high bench. The justices themselves, through their own actions, might increase or decrease esteem for the Court, or public feeling about the Court could simply follow sentiment toward other political institutions. These broad conceptions of the wellsprings of public support for the Court encompass each of the more specific explanations I shall develop in this section and test in the next.

Economic Conditions

Work on presidential popularity, support for Congress, and confidence in institutions in general often indicates a strong and positive relationship between economic prosperity and stability and favorable public evaluations. Chief executives who preside over serious inflation and unemployment normally suffer considerable losses of public support, and unstable prices apparently cause more damage

(Kernell, 1978; MacKuen, 1983). There is some evidence that the public holds Congress responsible for our economic well-being: as unemployment increases, so does congressional "unpopularity" (Parker, 1977), and Lipset and Schneider (1983) report strong and negative relationships between confidence in the Court and inflation and unemployment. Obviously the Court has no direct impact on economic performance, and no one would argue that it does. Nevertheless, in the bad times of the 1970s, discontent over economic maladies and lost faith in economic organizations spilled over onto noneconomic institutions. High unemployment, which naturally shakes public certitude about the structure of the economy, may well translate into less support for political institutions, including the Court. It is even more likely that inflation should decrease public support for the Court and other institutions because prices affect everyone in some measure. To measure inflation and unemployment, I have chosen the percentage of change in prices and the rate of unemployment in the quarter previous to the observation on public support (U.S. Department of Labor, various years).

Presidential Popularity

Several scholars have contended that people think of the president and the Court in the same vein (Casey, 1975; Dolbeare, 1967; Dolbeare and Hammond, 1968). The president, of course, nominates all members of the Court. Because the president usually has a number of opportunities to nominate new justices, the Court normally constitutes an integral part of the "ruling coalition" (Dahl, 1957; see Scigliano, 1971). From the results of polls, Dolbeare (1967) reports that respondents are more likely to see the Court in a favorable light when a member of their own party occupies the White House and that those who were inclined

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to defer to presidential decisions were similarly disposed toward the Court. In a later study, Casey (1975) affirmed the "presidential association" and demonstrated that opinions toward the chief executive have a greater impact on attitudes toward the Court among independents than among partisan identifiers. Furthermore, people who trust in the federal government as a whole evince greater support for the Court (Murphy et al., 1973). We therefore have good reason to expect support for the two institutions to move hand-in-hand, even if people do not connect the president and the Court. If support for institutions in general tends to move upward and downward in unison, then presidential popularity and confidence in the Court should show a significant and positive relationship (cf. Tanenhaus and Murphy, 1981). To measure presidential popularity, I have relied on Gallup's data and used the percentage of each sample who express approval of the incumbent chief executive in the quarter prior to the observation on attitudes toward the Court (Gallup, various issues).

Judicial Activism

For the last 30 years or so, commentator after commentator has taken the Supreme Court to task for getting itself too involved in the making of public policy and the lives of the citizens. Critics and even friends of the Court have often warned that over the long haul, "judicial activism" can and does carry severe penalties. These cautions come in several forms. First of all, some scholars have argued that the Court, by refraining from the use of judicial review, can legitimize the policies of other branches of government. If the Court in fact has the power to confer legitimacy, then excessive activity might devalue the judicial coin. Second, others point out that when the Court invalidates a state or federal law, it acts contrary to the preferences of a "law-making majority" (Dahl, 1957). To rebuff

popular wishes repeatedly or on controversial measures, however heroic and appropriate, is to run the risk of public disapprobation and perhaps active resistance. Historically, extended periods or particularly aggravating instances of judicial activism have culminated in congressional attempts to curb the powers of the justices; the Court-Packing Plan of 1937 and the Jenner Bill of 1957 spring immediately to mind (Murphy, 1962).

Though assertions of this kind have great currency, few have subjected them to empirical testing. Lehne and Reynolds (1978) show that during a period of great controversy over a significant judicial decision, the salience of the Supreme Court of New Jersey increased, and evaluations became more dependent on people's views of public policy. Yet in the aggregate, judicial activism seems not to have made much difference; through all of the brouhaha, ratings of the Court did not move consistently in one direction.

Few scholars, commentators, or judges agree on the meaning of the concept of judicial activism. In an ideal world, I would create indicators of judicial activism that reflect all of the facets of this complex notion; here I have chosen a simple measure: the number of times the Supreme Court has declared a federal law invalid (for data and further justification, see Caldeira and McCrone, 1982, p. 110).³

Salience of the Court

In the normal course of events, the Court does not rank high in public visibility. At certain junctures, however, the Court becomes a more salient object in the public mind. By taking on tough issues and deciding them in a dramatic fashion, the justices themselves play a major role in focusing attention on the Court. Various mass media are even more central in shaping the agenda for public discussion (Grey, 1972; Newland, 1964). In any event, regardless of the precise reasons, public attention to the Court

changes substantially over time. Under certain circumstances, heightened salience should lead to greater comprehension of the Court. Presumably, people who learn more about the complexities of judicial decision making will lend the Court greater support than will the less sophisticated. The gist of the argument is that "to know the Court is to respect it, if not to love it" (Kessel, 1966; Murphy et al., 1973, p. 41).

From another perspective, public visibility might actually contribute to negative attitudes toward the justices. Quite apart from stories at the beginning of the term or on new appointments, much of the attention to the Supreme Court in the mass media derives from particular decisions. Clearly, controversial decisions merit much more extensive coverage, usually of a highly divided court. Since the Court often surfaces in the mass media in divisive and politically-charged situations, members of the public might well associate the high bench with trouble. Under public scrutiny, the Court does not usually show off its best judicial face—a harmonious, nonpartisan collection of Solomons. Furthermore, these occasions of intensified interest could focus the citizenry on the reality that judges do make law, all the time—a fact that apparently escapes some people (Casey, 1974).

Cognizant of potential flaws, I have nonetheless employed the number of stories on the Court in the *New York Times* in a year as an indicator of public salience. That newspaper of course reaches only a fraction of the citizenry, but a large proportion of national opinion-leaders read the *Times*. One way or another, the *Times'* coverage of the Court filters down into the towns and villages of America. If anything, this indicator overestimates the salience of the Supreme Court.

Crime

During the era of the Warren Court and

even well into the Burger Court, members of the public blamed the Supreme Court for all manner of social maladies—permissiveness, communism, pornography, venereal disease, and the breakdown of the family. An ever-expanding rate of crime figured prominently among the ills ascribed to the Supreme Court. Certainly, an image of the Court as an all-powerful entity unloosing the fetters on social disorder and moral decay does not comport well with the quite circumscribed powers most scholars envision. Even if the Court did have a hand in encouraging the lawless, most academics agree that other influences (e.g., demographics) account for much of the increase in the rate of crime during the 1960s and 1970s. Still, the rate of crime did skyrocket in the midst of the Warren Court's heyday, a time when the accused and the convicted seemed to win a victory every week, and so it is not surprising that some should conclude that the Supreme Court was at fault.

At any rate, when crime goes up, the Court constitutes a logical and handy target for public displeasure. Indeed, of all of the problems that plague the nation, crime provides one of the best tests of the efficacy of the legal and political processes. Courts, after all, have the responsibility of trying and punishing the accused. Just as citizens hold the chief executive responsible for inflation and unemployment, so, too, might the public view the control of crime as peculiarly within the ambit of the Supreme Court. Fault, once placed at the doorstep of the Court, could result in a decline in the public's confidence in the justices. To test this notion, I have used the change in the rate of crime in the quarter previous to the collection of the data on confidence (U.S. Department of Justice, various years). Following the considerations I have outlined, I project that as the rate of crime increases, the level of public support for the Supreme Court will decline.

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Judicial Solicitude for the Rights of the Accused

Related to the previous hypothesis, if members of the public become concerned about crime and make a connection between it and the Court, we might reasonably expect some movement in public support as the Supreme Court shifts in one direction or the other concerning the rights of the accused. Statements of the justices both on and off the bench suggest a belief among them that judicial protection of criminal defendants normally goes against the tides of public opinion and often translates into public anger. The public need not monitor the subtleties of changes in the criminal law, and everything we know about citizens' knowledge of the law indicates that, for all but a select few, this would be impossible. Instead, the citizenry might sense the general tenor of the Court's attitude toward the rights of the accused and then reduce or increase confidence accordingly. Since only a small segment has direct contact with the *Supreme Court Reports*, the public would receive cues on the justices' current stance at second- and third-hand from such sources as newspapers, television, radio, and opinion-leaders. In light of the lack of public knowledge, confidence in the Court would probably respond only to relatively gross shifts in judicial policies. This means that we need not worry a great deal over the development of sophisticated and detailed indicators of the content of the Supreme Court's policies in the criminal law. Accordingly, I have relied on the percentage of the cases on criminal law decided against the government as a measure of the Court's solicitude toward the accused in any particular term. There is no apparent difference in the effects of shifts in the Court's state and federal decisions on public support, so I have treated the criminal decisions as one category. The foregoing ruminations suggest that as judicial support for the

accused increases, public support for the Supreme Court will decrease.

Political Events

In previous work on support for institutions and leaders, scholars have demonstrated the crucial effects of discrete political events and circumstances on the rise and decline of public confidence. For example, Mueller (1973) persuasively argues that crises in foreign affairs result in "rallying-around-the-flag" and a subsequent increase in the popularity of the incumbent chief executive (cf. Parker, 1977). Unfortunately for the purposes of analysis, events normally associated with the Court seldom cause a splash of the dimensions of the Mayaguez incident or the Cuban missile crisis. Particular decisions sometimes do gain a fair amount of attention in the elite media of communications, but few single cases—with the exception of a bombshell such as *Dred Scott*—have sufficient weight to shift public attitudes one way or the other. Even if we could isolate a number of crises or landmark decisions, the polling organizations have not gathered data on support for the Court often enough to permit a precise reading on the influence of salient events.

Quite apart from the momentous and controversial decisions of the last two decades, such as *Roe v. Wade* and cases on capital punishment, two easily identifiable political events rank far above others in potential effects on public support for the Supreme Court. Many have commented on the spectacular consequences—at least in the short run—of the Watergate scandal on the structure of government and people's perceptions of politicians. To be sure, the revelation of the innumerable "horribles" sent President Nixon's rating, as well as public confidence in politicians in general, down to the depths. Perhaps paradoxically, however, public stock in Congress and the Supreme Court rose dramatically. Hear-

ings in the Senate and, later, the House Judiciary Committee's work on articles of impeachment placed the national legislature in an uncusomarily positive light. The courts, from the lowest levels to the highest, consistently opposed the president's schemes to place himself "above the law" and out of the reach of the judicial process. These judicial heroics culminated in *United States v. Nixon*, in which the Supreme Court unanimously refused to allow the president to shield himself from legal scrutiny with a claim of "executive privilege." Presidential misbehavior in Watergate created an opportunity for the Supreme Court and Congress to serve in a visible manner as bulwarks of democracy and of law and order. Therefore I expect that, apart from other influences on confidence, Watergate increased public support for the incumbents of the Supreme Court. To assess the effect of Watergate, I use a dummy variable, scored 1 for August 1974, and 0 otherwise.

The latter part of the administration of Lyndon Johnson marked the beginning of a longer and rather persistent slide downward of public confidence in government and other institutions. Disorders in the streets of large cities, rebellion among students in universities, the morass in Vietnam, President Johnson's pronounced proclivity for hedging the truth—all these and more spelled a precipitous fall from the grace of the relatively trusting and perhaps naive public of the 1950s and mid-1960s. The precise reasons for the increased distrust, however intriguing, need not detain us (see Citrin, 1974; Miller, 1974), for the central point is that the Supreme Court, along with other basic institutions, fell victim to the new dispensation of pervasive cynicism. This we must take into account. Close observers disagree on the exact juncture at which public trust began to dissipate, but we do know from Figure 1 that support for the Supreme Court took the greatest proportionate drop from 1967 to 1971. Hence I

hypothesize that a decline occurred after 1967. To construct a properly specified model of support for the Court, I have included a dummy variable for the Johnson administration, scored 1 for surveys executed in 1966 and 1967, and 0 otherwise. Accordingly, I expect to encounter a positive regression coefficient.

Thus far I have adumbrated several alternative explanations of changes in confidence in the Supreme Court: economic conditions, the rate of crime, presidential popularity, judicial activism, support for the rights of the accused, and political events. Formalization in the fashion of a multivariate model yields:

$$Y_t = \alpha_0 - \alpha_1 X_1 - \alpha_2 X_2 + \alpha_3 X_3 - \alpha_4 X_4 - \alpha_5 X_5 - \alpha_6 X_6 - \alpha_7 X_7 + \alpha_8 X_8 + \alpha_9 X_9 + \epsilon_t, \quad (1)$$

where Y_t represents the degree of confidence; α_1 , the rate of inflation; α_2 , the level of unemployment; α_3 , presidential popularity; α_4 , the number of federal statutes the Court declares invalid; α_5 , the salience of the Court; α_6 , the rate of crime; α_7 , the percentage of support for the accused among the justices in a particular year; α_8 , the effect of Watergate; α_9 , the quantum drop in public trust after 1967; and ϵ_t , random errors. Based on the considerations outlined in the previous paragraphs, I believe we should find α_1 , α_2 , α_4 , α_5 , α_6 , and $\alpha_7 < 0$, and α_3 , α_8 , and $\alpha_9 > 0$.

Findings

I present in Table 1 the results of the estimating procedure for the comprehensive model sketched in the previous paragraph. These results portray, at the very least, an encouraging initial step toward a persuasive and powerful explanation of confidence in the Supreme Court. Taken as a whole, the equation provides a good, if not excellent, fit with nearly 80% of the variance explained; six of the nine coeffi-

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Table 1. Confidence in the Supreme Court

Independent Variables	Estimates ^a		Level of Significance
	<i>b</i>	<i>t</i> ratio	
Intercept	45.44	4.58	.000
Political events/conditions			
Watergate	10.58	2.79	.012
Johnson administration	12.53	2.89	.010
Presidential popularity	0.11	1.85	.082
Social/economic conditions			
Rate of crime	-0.00	-0.01	.986
Rate of unemployment	-0.55	-0.79	.436
Inflation	-0.51	-1.24	.231
Judicial actions			
Salience of the Court	0.08	1.97	.065
Invalidation of federal laws	-2.35	-1.90	.074
Court's support for defendants' rights	-0.21	-1.98	.064
	<i>R</i> ² = .79	<i>DF</i> = 17	<i>N</i> = 27

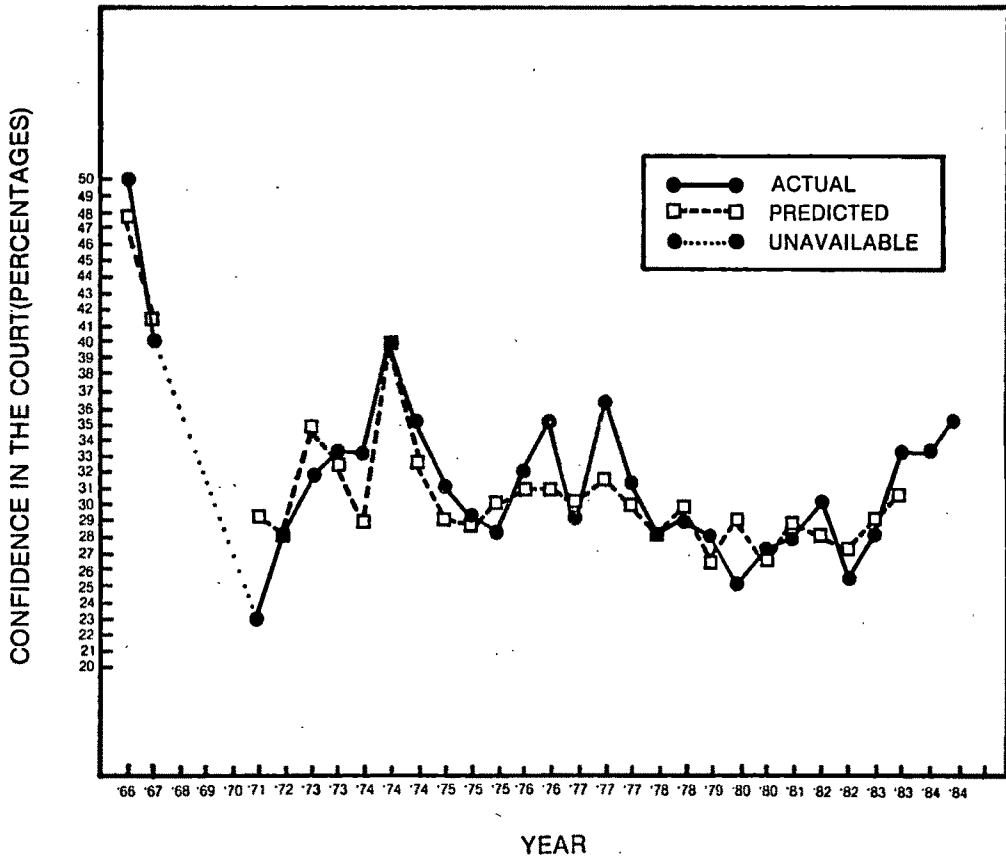
^aAfter testing for the significance of the autoregressive terms in an initial run, I have excluded them from this equation.

cients reach statistical significance, and the signs of eight of the nine run in the correct direction.⁴ Salience in the Court alone goes astray for reasons discussed shortly. According to the intercept, about 45.4% of the public would express considerable confidence in the incumbents of the Court if the several independent variables equalled zero. To be sure, students of presidential popularity achieve uniformly more impressive coefficients of determination, oftentimes as the result of using a lagged dependent variable. Yet the fit in Table 1 compares quite favorably with previous reports on evaluations of Congress (Parker, 1977) and would appear even better had I chosen to run the risk of including a lagged version of confidence in the Supreme Court. Figure 2 illustrates how the estimates track with the actual observations; with the exception of 1971, October 1976, and July 1977, the model performs remarkably well.

Over the period from 1966 through 1984, ebbs and flows of confidence in the

Court responded, to a surprisingly large extent, to the happenstances of political events. During and immediately after the consummation of Watergate and the resignation of President Nixon, support for the Supreme Court shot up more than 10 points. The Court benefitted from the president's woes, as I mentioned earlier, in part because of its association with defenses of the "Constitution" against what many in 1973 and 1974 perceived as frontal assaults on the underpinnings of the legal order. Similarly, before wholesale disillusionment and demoralization spread at the end of the Johnson administration, support for the justices ran a full 13% above the norm for the entire period under investigation ($\alpha_c = 12.5$). Public attitudes toward the Court, as well as toward virtually every element of the American "Establishment," fell in the late 1960s from the lofty pedestals of the years of Eisenhower and Kennedy. The Court may have suffered somewhat more than others from loss of public esteem because

Figure 2. Actual vs. Predicted Patterns of Confidence in the Court:
1966-1984



of perceived connections with problems of race, crime, and cultural disorder, but the justices shared the company of a distinguished group.

Confidence in the Court, unlike support for Congress and the president, reflects the palpitations of economic conditions in what I can most charitably describe as a muted fashion. Bad economic times do not, of course, translate into increases in public esteem for the justices, but they do not seem to hurt a great deal. Thus, for example, the rate of unemployment failed to muster a statistically significant coefficient even as it appeared to manifest the expected negative effect ($\alpha_2 = -0.55$; $p < 0.43$). Infla-

tion presaged, as I hypothesized, deleterious consequences for public support for the Court; for each increase of 1% in the consumer index of prices, the proportion that recorded considerable esteem dropped more than half of 1% ($\alpha_1 = -0.51$; $p < .23$). This relationship, though far from secure, survived a passel of tests I contrived to detect spuriousness. How can we account for the differential effects of inflation and unemployment? For one, although most of us express concern over the plight of the jobless, the rate of unemployment directly affects only a fraction of the citizenry; even at the high tides of economic depression, only a few feel the pinch. To the contrary, because

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everyone purchases goods and services, inflation, particularly at high levels, presumably holds a greater potential for wreaking havoc with the overall public mood and feelings of confidence. It is commonplace that extended bouts of inflation call into doubt the performance of political and economic actors. That jobs and the stability of prices should on the whole make only a modest dent in confidence in the Court reflects, without doubt, the lack of any direct connection in the public mind between the justices and economic activities. In an odd way, then, the lack of compelling relationships between confidence and economic conditions testifies to the rationality of the ebb and flow of public evaluations of the Supreme Court.

Surprisingly, despite record-shattering rates of change in the 1960s and 1970s, crime as recorded by the F.B.I. had no influence on the amount of confidence the public bestowed upon the Supreme Court. Indeed, of all of the variables, the rate of crime manifests the most diminutive regression coefficient and the least significant *t* ratio in the statistical sense. In the face of steep and very real increases in the rate of crime, the U.S. public apparently failed to hold the Court responsible for this social problem. The middle 1960s, a time of accelerating rates of crime, encompassed high levels of support for the Supreme Court; the late 1970s, a period of markedly less-pronounced movements upward in the incidence of criminal activities, produced relatively low evaluations of the justices among the public. Others have shown that the mass public, in thinking about personal safety, responds not to actual, official reports of the incidence of personal and property crimes but, rather, to the mass media's coverage of the problem of crime (MacKuen, 1979). Thus, if the local newspaper or television station dramatizes the threat of crime, members of the public manifest more anxiety about personal

security. Similarly, in making choices on the size of requests and appropriations, the president and Congress apparently weigh the mass media's coverage of crime as heavily as the F.B.I.'s measures of crime (Caldeira and Cowart, 1981; but see Caldeira, 1981). In sum, from the results in Table 1 and earlier research, it seems likely that the media must focus on crime before people express much concern and place blame on the Court.

Proponents of the view that the citizenry associates the justices and the Supreme Court with the presidency can find considerable succor in the statistical results in Table 1. Presidential popularity does indeed exercise a statistically significant, albeit modest, influence on the amount of confidence the public accords the Court. For each 10% increase in popular approval of the president, *ceteris paribus*, public confidence in the Supreme Court increases by approximately 1% ($\alpha_3 = .114$; $p < .082$). Furthermore, in analyses not presented here, the relationship between presidential popularity and confidence in the Court grows ever stronger and more secure in successively more parsimonious versions of the model. These results, moreover, should encourage scholars who have hypothesized that the amount of public support for various institutions tends to move upward and downward together—to a considerable degree in lock step. Members of the public, in this conception, record happiness or displeasure with our institutions across the board. Some auxiliary statistical analyses provide further evidence of this phenomenon. For example, confidence in the Court and in Congress track closely during much of this period.⁵ Without more extensive data, however, I cannot make an informed choice between the two formulations outlined here. Let it suffice to say that, for whatever reason, presidential popularity and confidence in the Supreme Court most of the time move in the same direction.

Contrary to conventional wisdom, in the case of the Court, familiarity does not breed contempt—or so the results in Table 1 seem to suggest. Greater public visibility of the Court, in fact, translates into a higher degree of support among the public. For each increase of 12% in the amount of coverage of the Court in the *New York Times*, *ceteris paribus*, Harris and NORC have recorded on average an increase of 1% in confidence in the Supreme Court ($\alpha_3 = .077$; $p > .065$). Here, as in some other instances, the relationship gains strength and security in successive variations of the basic model. Some scholars, as I noted earlier, have suggested the possibility that public knowledge of the Court leads to greater support for the justices (Kessel, 1966; Murphy et al., 1973, pp. 41–43). Yet the weight of evidence from cross-sectional studies, I thought, went rather clearly against the happy idea that an appropriately enlightened public would demonstrate a proper appreciation of the Supreme Court (Murphy et al., 1973, pp. 42–43). So the fundamental question remains, Why does public opinion behave in this fashion? It is, after all, difficult to believe that the Supreme Court reaps increased public esteem from publicity on the controversial decisions the justices must inevitably hand down each term. Quite to the contrary, I believe, we can account for the positive association between the salience of Court and public confidence in the justices if we concentrate on the institutional focus of much reporting in the newspapers. Apart from the usual flow of stories on decisions, the *New York Times* and other newspapers on occasion write up individual justices or the Court as a whole, for the most part divorced from the content of the docket. Even so-called investigative stories on the Supreme Court or particular justices almost always present the institution in an extraordinarily positive light. The conjunctions between the ebb and flow of the

Times's coverage of the Court and the series on public confidence provide some circumstantial evidence for these speculations. In the final analysis, I suspect the content of the stories on the Court matters a good deal more than the sheer number. That, of course, is an empirical issue and one someone can test with further data.

As expected, judicial activism, in the form of invalidations of federal statutes, cost the Supreme Court dearly in the court of public opinion. Interpretation of the regression coefficient starkly illustrates the high price the Court pays for opposing the will of Congress. For each federal statute the Court has struck down in the past 17 years, public confidence in the justices has declined about 2.4% ($\alpha_4 = -2.35$; $p < .07$). The losers of litigation, inside and outside the Court, have made such a claim for so long and so many times that the putatively negative consequences of judicial activism have risen to the status of a shibboleth. Nonetheless, the evidence presented here buttresses this ancient pearl of wisdom. To the extent that federal law at any point represents the strong preferences of a majority, the Supreme Court treads on thin ice in the act of invalidating large numbers of statutes. Over the years, as the Supreme Court invalidates federal laws, the justices amass a coalition of enemies, groups and individuals whose views of public policy have lost favor and who will naturally accord the high bench less confidence. Expressed consistently, judicial opposition to Congress may in and of itself lead to resentment and loss of trust among the public—not because of any great love of the national legislature but, rather, as a result of a perception that the Court has upset the balance of our constitutional system. Citizens may indeed view Congress as the “broken branch,” but they also have a stake in the process by which national officials check one another. There is, of course, no way of knowing with these data why the

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public responds in a negative fashion to judicial negations of congressional statutes. Regardless of the precise mechanism, however, I have adduced strong evidence that judicial activism does exact a significant cost in the coin of public confidence.

Public support has, in fact, responded in a significant fashion to changes in the Court's solicitude for the rights of individuals accused of crime. As the Supreme Court has manifested proportionately greater sympathy for criminal defendants, the public has in turn registered a sharp drop in confidence in the justices. More specifically, for every increase of 10% in the share of cases decided in favor of the accused, public support has declined somewhat more than 2% ($\alpha_7 = -0.21$). Two percent does not sound like a devastating blow, but concrete applications provide a graphic illustration of the consequences of the Court's drift in public policy toward the accused: as an empirical matter, judicial support for criminal defendants varied from 12% to 83% during the period from 1966 to 1984, and a shift of 20% or more from year to year happened several times. It goes without saying that members of the public do not make subtle distinctions in their thinking about the direction of the criminal law, and in view of the soup-like quality of individual responses to law and legal institutions, I do not wish to argue that the citizenry forms sharply defined conceptions of the Court or its policies. However, these results suggest that the mass public as a whole responds in a systematic fashion to shifts in the public policies the justices enunciate. Clearly, the public has little sympathy for either the esoterica of criminal procedure or the people who most often utilize these safeguards and apparently translates these attitudes into lack of confidence in the Court. Thus, like judicial activism, solicitude for the accused exacts significant costs to the Supreme Court among the citizenry.

Conclusion

Public support for the Supreme Court varies a great deal over time and eludes simple accounts, but in this article I have attempted to trace meaningful shifts in confidence and, more importantly, to frame and evaluate alternative explanations of the dynamics of esteem for the Court. These rival hypotheses include political events; the rates of crime, unemployment, and inflation; presidential popularity; judicial activism; salience of the Court among the public; and the justices' solicitude for the rights of the accused. Broadly speaking, I conclude that during the period from 1966 through 1984, crime and economic conditions played relatively small roles as determinants of public confidence and that political events and judicial actions registered striking impacts on changes in the public's view of the justices. More specifically, in descending order, the Johnson administration, Watergate, judicial support for the accused, the salience of the Court, invalidations of federal laws, and presidential popularity brought about statistically significant movements in public feelings toward the justices of the Supreme Court during this period.

What do these results mean? Citizens, as individuals, evince little or no knowledge of or concern for the Court; to the extent that they express sensible opinions, they base judgments on the vaguest and crudest of ideological frameworks. Social scientists have demonstrated this over and over again, and yet, in the aggregate, shifts in public confidence in the Court march to the beat of a markedly policy-oriented drummer. Public evaluations of the Court do not float freely, in a seemingly aimless fashion, unconnected to the perturbations of the political and legal processes. Rather, in evaluating the justices, the public appears to respond to events on the political landscape and to actions taken by the Supreme Court. If,

for example, the Court adopts a position against a law-making majority, the public accordingly exacts a cost in confidence—a rational calculus indeed. In broad outlines, then, the dynamics of aggregate support for the Court bear a remarkable resemblance to those for Congress and the presidency (Kernell, 1978; Parker, 1977). It is easy to forget that individuals' responses to the presidency and Congress hardly constitute a model of clarity or stability, for scholars of these institutions have spoiled us with crisp, neat political explanations of presidential popularity. Until now, however, scholars of the Court have worked solely with individual-level data and have, naturally enough, emphasized the poor quality of public evaluations of the justices. In this article, with aggregate data, I have portrayed the relationship between the public and the Court as somewhat more rational and calculated than have previous researchers. I do not, of course, wish to press my claim for rationality very far, but I do think that in the literature we have oversold our pessimistic view of public evaluations of the Court, just as students of elections for many years unfairly characterized voters as ignorant and apathetic. For the Supreme Court, as for Congress and the presidency, the movement of public—as opposed to individual—opinion comes much closer to meeting our conventional prescriptions.

Notes

I appreciate the assistance of Brad Lockerbie, a graduate student at the University of Iowa, who gathered much of the data for this project. Chia-Hsing Lu of the Laboratory for Political Research at Iowa helped in the construction of files and the workings of SAS.

1. On a number of occasions during the 1970s, Gallup asked the public, "Would you tell me how much confidence you, yourself, have in each one [a list of institutions, of which the Court was one]—a great deal, quite a lot, some, or very little?" Gallup apparently taps a phenomenon one might label *diffuse support*. Such support in fact and by definition

varies little and offers scant play for the investigator. Indeed, one scholar has argued, in part on the basis of Gallup's polls, that public ratings of the Court have changed little in 50 years (Handberg, 1984). By contrast, NORC and Harris focus on the performance of the current justices, very much as in evaluations of the president. A respondent in this context introduces policy-oriented considerations as a matter of course—or at least more so than in responding to Gallup's items. Since I aim to disentangle the connection between public opinion on the one hand and social conditions and political events on the other, the data from Harris and NORC suit my purposes well.

2. In Figure 1, and later in Figure 2, I have chosen to include the years 1968, 1969, and 1970 in order to retain a clear temporal sequence. Instead of interpolating for these years, I have used only actual observations in the statistical analyses.

3. Of course, I could just as easily have used the number of state laws the Supreme Court invalidated in a year as an indicator of judicial activism. I chose federal law instead because I wanted to capture conflict between the Court and national law-making majorities. In future work, I hope to refine this measure to take into account the age of law-making majorities.

4. Because of the ample specification of the model, serial correlation does not pose a threat here. For that matter, in many diagnostic analyses I detected no signs of intolerable levels of multicollinearity.

One reader has raised a question about the effects of the very high observations of confidence in the Court in 1966 and 1967 on the estimates of the model's coefficients. These observations, he suggested, might overwhelm the rest of the series and color all of the results. To test this notion, I removed the data for 1966 and 1967, deleted the variable for the Johnson administration, and ran the statistical analysis again. The new results are quite similar to those presented in Table 1; the main difference is that the quality of the fit declines.

5. Yet in preliminary multivariate analyses, I estimated the effect of confidence in Congress on support for the Court and found no significant relationship. That, of course, indicates the spuriousness of the simple relationship between the levels of confidence in the two institutions.

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FREUD AND FREEDOM OF SPEECH

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In this essay I develop a psychoanalytic defense of freedom of speech that is implicit in Freud's works, principally in his discussions of verbal slips and jokes. Freud argues that freedom of speech benefits people by providing a harmless outlet for aggression, suggesting that it is better to express aggression in words than in violent deeds or to repress it altogether. The psychoanalytic defense of free speech has affinities with various liberal defenses, but it is partial because apolitical; it emphasizes the emotional self-expression of speakers as opposed to the rational persuasion of listeners. The intellectual roots of the contemporary concern with "freedom of expression" (as opposed to "freedom of speech") can be found in Freud: to focus on freedom of expression is to ignore the qualitative differences among forms of self-expression and to neglect the specifically political character of speech.

Sigmund Freud was mostly apolitical, both in his life and in his work;¹ to the extent that he had political convictions, however, he described them as liberal.² My purpose here is to suggest that, at least with respect to one significant political question, namely, freedom of speech, Freud's liberalism follows directly from psychoanalytic premises. By focusing primarily on two of Freud's books whose political import has not been sufficiently perceived, *The Psychopathology of Everyday Life* and *Jokes and their Relation to the Unconscious*, I will point to the psychoanalytic grounds underlying Freud's advocacy of freedom of speech.

Freud's main reason for writing these two books was not, of course, to defend freedom of speech; he wanted primarily to heighten his and his readers' psychological knowledge, not to indicate the possible political implications that such heightened knowledge might bring. Nevertheless, a number of Freud's observations and remarks in both books im-

plicitly defend freedom of speech. In this essay I will, accordingly, focus upon the liberal position that Freud adopted but was content to relegate to the intellectual background.

Freedom of speech was a question of importance to Freud for a very good reason: speech itself was and is at the center of psychoanalytic therapy. Freud (1890a, p. 283) claims that "words are the essential tool of mental treatment. . . . [The] science [of psychoanalysis] sets about restoring to words a part at least of their former magical power."

Furthermore, Freud coupled his therapeutic concern with speech with a more general concern that he and others be allowed to speak, to write, and to think freely. A number of Freud's pronouncements seem for this reason to place him squarely in the tradition of liberal defenders of intellectual freedom like Milton, Spinoza, Locke, and Mill. In view of Freud's interests, it is not surprising that many of these pronouncements express his belief in the desirability of speak-

ing freely about sex. For example, he argues against the "astonishment and horror" that he expects his readers to feel upon learning of the sexual matters he discussed with his patient Dora, who was 18 years old at the time of her treatment:

There is no cause for indignation. . . . It is possible for a man to talk to girls and women upon sexual matters of every kind without doing them harm . . . so long as . . . he adopts a particular way of doing it. . . . The best way of speaking about such things is to be dry and direct; and that is at the same time the method furthest removed from the prurience with which the same subjects are handled in 'society'. . . . I call bodily organs and processes by their technical names. . . . *J'appelle un chat un chat.* (Freud, 1905e [1901], chap. 1, p. 48)

Similarly, in quoting from the memoirs of Daniel Schreber, where Schreber describes himself as "this person who lets himself be f-d," Freud (1911c [1910], chap 1, p. 20) remarks, "I reproduce this omission from the *Denkwürdigkeiten*, just as I do all the peculiarities of their author's way of writing. I myself should have found no reason for being so shamefaced over a serious matter." Finally, Freud's explanation of the reason for his having given the "love instincts the name of sexual instincts" is relevant here:

The majority of 'educated' people have regarded this nomenclature as an insult, and have taken their revenge by retorting upon psycho-analysis with the reproach of 'pan-sexualism'. Anyone who considers sex as something mortifying and humiliating to human nature is at liberty to make use of the more genteel expressions 'Eros' and 'erotic'. I might have done so myself from the first and thus have spared myself much opposition. But I did not want to, for I like to avoid concessions to faintheartedness [*Schwachmütigkeit*]. One can never tell where that road may lead one; one gives way first in words, and then little by little in substance. (Freud, 1921c, chap. 4, p. 91)

Freud's defense of intellectual freedom extends beyond discussions of sexuality however. In fact, he criticizes the censorship that he believes people impose upon themselves with respect to thinking about their sexuality in part because he believes

the censorship is extended to other matters:

How can we expect people who are under the dominance of prohibition of thought to attain the psychological ideal, the primacy of intelligence? . . . So long as a person's early years are influenced not only by a sexual inhibition of thought but also by a religious inhibition and by a loyal inhibition derived from this [i.e., a political inhibition deriving from unthinking loyalty to divine right monarchy], we cannot really tell what in fact he is like. (Freud, 1927c, chap. 9, p. 48)

That Freud's defense of intellectual freedom aims at more than the extension of sexual freedom is most clearly established by the following consideration: even though Freud believed in 1938 that "in Soviet Russia they . . . have been wise enough to give [people] a reasonable amount of sexual liberty," he still criticized its leaders for having "robbed them of any possibility of freedom of thought" (1939a [1937-9], prefatory note 1 to essay 3, p. 54).

These statements suggest that Freud defends intellectual freedom for reasons similar to some of those motivating earlier figures such as John Stuart Mill. Freud, like Mill, appears to advocate freedom of speech and thought in order to facilitate the intellectual progress of the individual and the human race—in order to foster personal growth or development toward "the psychological ideal, the primacy of intelligence." Nevertheless, one senses a significant difference in tone between Freud and Mill: underlying Freud's praise of honest speech is an apparent hostility or aggressiveness directed against conventional "society," the "fainthearted" home of "shamefaced[ly]" hypocritical or dishonest speech. Freud evidently prides himself on his honesty, which leads him both to express his anger at society and consequently to anger society. For Freud, intellectual freedom in some way appears to connote not only the search for truth but also the willingness to give vent to one's own aggressiveness and to brave the

aggressiveness of others. I believe this is significant, for we will see that aggression, both as manifested by Freud and as understood by him, is central to what is original and psychoanalytic in the Freudian defense of freedom of speech and emotion (which differs in important ways from the traditional and intellectual "Millian" defense of freedom of speech and thought).

In order to understand Freud's specifically psychoanalytic grounds for defending freedom of speech, it is useful to begin with two statements in which he and his one-time collaborator Breuer explain the reason for the importance of speech in psychoanalytic therapy. In the first, they write that "language serves as a substitute [*Surrogat*] for action; by its help, an affect can be 'abreacted' [i.e., a disturbing emotion can be discharged] almost as effectively [as it can by an action]" (Freud and Breuer, 1893a, p. 8). Developing this point in the second statement, Breuer adds that "telling things is a relief" (Freud and Breuer, 1895d, chap. 3, sec. 3, p. 211).

However, these statements are primarily of interest not because of what they tell us about the function of speech within psychoanalytic therapy but because Freud's view of the social function of ordinary speech in some ways resembles his view of the function of the speech directed by a patient to his or her analyst. In society, as in therapy, Freud believes that "language serves as a substitute for action"—that "telling things is a relief." These beliefs lead Freud to construct his psychoanalytic argument suggesting both the advantages and, secondarily, the limitations of freedom of speech. Freud has been described as "the great liberator . . . of speech" (Marcus, 1975, p. 294); by assessing Freud's psychoanalytic argument in comparison with more traditional arguments, my intention is to elucidate this description of Freud and to evaluate its adequacy. I do so hoping to increase

not only our understanding of Freud but also our understanding of the more traditional liberal defense of freedom of speech.

Cursing and Censorship

Freud himself suggests the analogy that I have proposed between the function of speech in therapeutic situations specifically and in social situations generally:³

The most adequate reaction [by which heightened emotion can be lessened] is always a deed. But, as an English writer has wittily remarked, the man who first flung a word of abuse at an enemy instead of a spear was the founder of civilization. Thus the word is the substitute [*Ersatz*] for the deed, and in some circumstances (e.g., in Confession) the only substitute. Accordingly, alongside the adequate reaction there is one that is less adequate. (Freud, 1893h, p. 36; I have made the translation more literal.)

What is crucially important about this statement is Freud's assumption that the first word (or at least the first relevant word in the civilizing process) was a word "of abuse." In saying this, Freud here implies something he states explicitly elsewhere: that social relations are inherently conflictual.⁴ One crucial function of speech is, therefore, to provide an outlet for our aggressiveness that is safer, both for us and for others, than physical violence. In an important sense (for which Freud elsewhere provides a theoretical justification) conflict is prior to and more fundamental than cooperation.⁵ Because this is already implicit in Freud's statement about the "word of abuse," one can say that Freud's understanding of language in some measure resembles Caliban's in Shakespeare's *Tempest*, for Caliban remarks to Prospero, "You taught me language, and my profit on't / Is, I know how to curse" (act 1, scene 2, lines 363–64).

Language makes it possible for us to curse and profits us by enabling us to work out our hostility to others in a

reasonably harmless manner. However, it is to some extent unfortunate, in Freud's view, that we are often unwilling to employ the linguistic vehicle for the exorcism of our aggression because we are unwilling to admit our aggressiveness (and more generally, our egoism) to ourselves. Thus, the aggressiveness displayed by Freud vis-à-vis society and those whom he derisively calls "the masses stems in part from his impatience at their failure to acknowledge and to act out their aggressiveness.⁶ Insofar as we are reluctant to acknowledge our aggressiveness, Freud contends that we curtail the freedom of our own speech excessively and harm ourselves by repressing our aggressiveness instead of expressing it in our speech. Free speech should profit us Calibans by enabling us to curse. It does not always do so, however; we suppress our desire to curse because to a great extent—too great—we are Calibans with a bad conscience.

Our bad conscience is apparent in a phenomenon whose very name suggests its relevance to the question of freedom of speech—the phenomenon Freud calls censorship. Freud employs the concept of censorship most prominently in his discussions of dreaming: censorship accounts for the fact that dreams must be interpreted—for the fact that a dream is less "the fulfilment of a wish" than it is "a (disguised) fulfilment of a (suppressed or repressed) wish" (Freud, 1900a, ch. 2, p. 121, and chap. 4, p. 160, respectively).

To explain why dreams are censored would take me well beyond the scope of my argument; for my purposes here, it is sufficient to note what is censored in dreams. Dream censorship is directed against things

that are invariably of a reprehensible nature, repulsive from the ethical, aesthetic and social point of view—matters of which one does not venture to think at all or thinks only with disgust. These wishes, which are censored and given a distorted expression in dreams, are first and foremost manifestations of an unbridled and

ruthless egoism. (Freud, 1916–17, lecture 9, p. 142)

In other words, in our dreams we censor the very things that lead Caliban to enjoy his cursing so much.

To make the metaphorical censorship of dreams comprehensible, Freud employs an analogy, urging us to consider the literal censorship that repressive political regimes impose so as to restrict freedom of thought:

I will try to seek a social parallel to this internal event in the mind [i.e., dream censorship]. Where can we find a similar distortion of a psychical event in social life? Only where two persons are concerned, one of whom possesses a certain degree of power which the second is obliged to take into account. In such a case the second person will distort his psychical acts or, as we might put it, will dissimulate. The politeness which I practise every day is to a large extent dissimulation of this kind: . . .

. . . A similar difficulty confronts the political writer who has disagreeable truths to tell those in authority. If he presents them undisguised, the authorities will suppress his words—after they have been spoken, if his pronouncement was an oral one, but beforehand, if he had intended to make it in print. A writer must beware of the censorship, and on its account he must soften and distort the expression of his opinion. According to the strength and sensitiveness of the censorship he finds himself compelled either merely to refrain from certain forms of attack, or to speak in allusions in place of direct references, or he must conceal his objectionable pronouncement beneath some apparently innocent disguise. (Freud, 1900a, chap. 4, p. 141)

As we have seen, Freud opposes the literal or political censorship of speech. It is noteworthy, however, that Freud's personal strategy in opposition to censorship differs somewhat from the strategy he ascribes to "the political writer": the political writer "soften[s] and distort[s] the expression of his opinion, . . . speak[s] in allusions in place of direct references, or . . . conceal[s] his objectionable pronouncement beneath some apparently innocent disguise." By contrast, Freud himself prefers to be direct, rather than seemingly accommodating in the manner of the political writer: he calls

a cat a cat, "avoid[s] concessions to faint-heartedness," and refuses to "give way [even] in words." One is tempted to say that the political writer must mask his aggression whereas Freud prefers freely to proclaim his. This difference between Freud and the political writer is important because it points to an implicit Freudian critique of politics altogether, as a realm of life in which aggression must be hypocritically papered over. We saw at the outset that Freud describes himself as being both liberal and apolitical; it is noteworthy that both of these characteristics apply to Freud's defense of freedom of speech. Freud's liberalism is apparent in his stance as a defender of freedom of speech, but the kind of speech that Freud is most concerned with defending and the grounds on which he defends it also testify to his apolitical tendencies. Freud's psychoanalytic defense of freedom of speech, I will argue, primarily suits speech that is fundamentally extra-political, perhaps even antipolitical.

Freudian Slips and Freedom of Speech

To this point we have seen that Freud advocates freedom of speech in part because he views it as a relatively safe outlet for our aggressiveness and egoism. On the basis of Freud's discussion of dream censorship, I have hypothesized that he views our reluctance to admit to our aggressiveness and egoism as a cause of restrictions upon freedom of speech. In order to test this hypothesis and to assess its relevance to politics, I now turn to analyses of *The Psychopathology of Everyday Life and Jokes and their Relation to the Unconscious*. I focus on these works in particular because they are pre-eminently concerned with language as it is employed by ordinary people in "everyday" social life: ordinary healthy people tell jokes and commit slips of the tongue. Because these books deal with everyday

life, a number of Freud's examples in them are directly relevant to the question of freeing speech from the constraints that are often, and perhaps ordinarily, imposed by political and social life.

Freud contends that some slips of the tongue result from our conscious attempts to suppress thoughts that occur to us; these attempts, in turn, can reflect our judgments that certain of our thoughts are unacceptable within our society (or at any rate to certain members of our society) even though the thoughts in question might be fully acceptable in other, conceivably better, societies. This is why Freud's discussion of slips of the tongue is relevant to freedom of political and social speech: Freud contends that some slips occur as a reaction against official governmental censorship of speech and that others occur as a reaction against the more informal censorship imposed by social conventions. Slips of the tongue can break through the censorship that political or social conditions lead us consciously to impose upon ourselves and can sometimes be viewed as a protest against the political or social conditions that, we believe, necessitate the censorship. In this sense, some slips of the tongue can be said to be relative to the regime or society in which they occur. Some of our slips may lead us indirectly to express the truths that we might not hesitate to proclaim openly under governments and in societies where freedom of speech was more widely permitted.

Freud gives a politically relevant example of such a slip. Quoting his pupil Victor Tausk, he tells of Herr A., a Jew who converts to Christianity so as to be able to marry his fiancée. The marriage produces two sons. Some years later, while on a summer holiday, he discovers that his hostess, who is unaware of his religious heritage, is an anti-Semite. Fearing that his sons, "in their candid and ingenuous way, would betray the momentous truth if they heard any more of the conversation, [he] tried to get them to leave the

company by sending them into the garden." But he then betrays his heritage himself (although his hosts, being providentially unaware of Freud's work, fail to interpret properly his betrayal): "I said: 'Go into the garden, *Juden* [Jews],' quickly correcting it to '*Jungen* [youngsters].' In this way I enabled the 'courage of my convictions' to be expressed in a *parapraxis*" (Freud, 1901b, chap. 5, pp. 92-93).

It is reasonably clear why Herr A. misspeaks himself in this example: he knows that by birth he is Jewish and knows that he does not want his hostess to know it. His belief that he must conceal his Jewish birth merely testifies to the fact that he lives in a society in which many people are anti-Semitic: his society confronts him with the unattractive alternatives of "betray[ing]" his Jewish birth either by revealing it against his will or by falsely denying it. In a freer society, in which his Jewish birth would not be an issue, he could admit it, even proclaim it, without being punished for it. Herr A.'s reference to the "courage of his convictions" suggests that his slip embodies a protest against a society in which he believes he will be penalized if he freely speaks a truth about himself.

In a number of other cases, in fact, Freud explicitly states that slips of the tongue can be viewed in this way, as reactions against and expressions of resentment toward political censorship, that is, political suppression of free speech. Thus Freud quotes the example of Professor M. N., a Swiss professor during World War I who was sympathetic to the Allies but who was prevented by Swiss neutrality from referring to the Germans as *boches*. In the course of a lecture, the Swiss professor tells a story to illustrate a point:

It concerned a German schoolmaster who had put his pupils to work in the garden, and in order to encourage them to work with greater intensity invited them to imagine that with every clod of earth that they broke up they were breaking a French skull. Every time the word for "German"

came up in the course of his story N. of course said "*allemand*" quite correctly and not "*boche*". But when he came to the point of the story he gave the schoolmaster's words in the following form: *Imaginez-vous qu'en chaque moche vous écrasez le crâne d'un Français*. That is to say, instead of *motte* [the French word for clod] —*mochel* (Freud, 1901b, chap. 5, pp. 72-73)

Freud quotes the analysis of this example (which he describes as an "exceedingly fine" one) given him by his source for the story, Dr. L. Czeszer:

Anxiety about committing a political indiscretion, perhaps a suppressed desire to employ the usual word in spite of everything—the word that everyone expected—and the resentment of one who was born a republican and a democrat at every restriction on the free expression of opinion—all these interfered with his main intention of giving a punctilious rendering of the illustration. The interfering trend was known to the speaker and he had, as we cannot but suppose, thought of it directly before he made the slip of the tongue. (Freud, 1901b, chap. 5, pp. 72-73)

It is true that the discussion of republican resentment "at every restriction of the free expression of opinion" comes to Freud from his informant; the words are not Freud's own. Shortly afterward, however, Freud himself makes a similar point in defense of "the free expression of opinion." He does so in discussing an example in which a subordinate's slip of the tongue while proposing a toast to his superior results in his effectively mocking his superior. Freud's analysis of the slip leads him to this conclusion:

If I were the Principal who was being honoured at the ceremony to which the speaker contributed the slip, I should probably reflect on the cleverness of the Romans in permitting the soldiers of a general who was enjoying a Triumph openly to express in the form of satirical songs their inner criticisms of the man who was being honoured. (Freud, 1901b, chap. 5, pp. 82-83)

The cleverness of the Romans was manifested in their permitting inferiors openly to express their inner criticisms of their superiors. For all that Freud is a critic of the "masses," for all that he is not himself a "democrat," one can understand

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him to be making a more general claim in this passage: he implies that societies in which the "masses" or the subjects can openly criticize their superiors (at least at times, if not necessarily always) are superior to societies that deny them this privilege.

I will explain why this is so below. In the meantime, however, we should consider a third example, in which Freud discusses a misprint found in an Austrian telegram (sent shortly before the end of World War I) that referred to the "active and interrupted co-operation" between Germany and Austria (Freud, 1901b, chap. 6, p. 121, translator's emphasis). Freud comments: "Only a few weeks later it was possible to express one's opinion more frankly about this 'mutual confidence,' and there was no longer any need to take refuge in a slip of the pen (or misprint)" (Freud, chap. 6, p. 121).⁸ Just as the word can substitute for the more desirable deed on occasions when it is impossible to act, so can the misprint be a substitute for the more desirable free expression of one's actual opinion on occasions when this is impossible or impolitic.

One thing unites all of the slips that we have examined: they all testify to the aggressiveness of their perpetrators. Each slip reveals an aggressiveness that social considerations often impel people to conceal. Herr A. alludes to his Jewish birth, which would offend his hostess; Professor N. attacks the Germans; the subordinate's toast mocks his superior; an Austrian official expresses his frustration at the terms of cooperation with Austria's German ally.

These examples seem to suggest not only that we wish to speak freely but also what we wish to speak freely: we apparently wish to be freer than we often are to proclaim the truth of our self-centeredness, notwithstanding that the self-centeredness of each individual runs the risk of bringing about conflict among all individuals. I have already pointed out

that Freud's emphasis on the conflictual character of human beings is familiar from his later works, yet it is already in evidence in *Psychopathology*. In a revealing passage added to the work in 1907, Freud makes the following observation about himself, and then extends it to others:

It is as if I were obliged to compare everything I hear about other people with myself; as if my personal complexes were put on the alert whenever another person is brought to my notice. This cannot possibly be an individual peculiarity of my own: it must rather contain an indication of the way in which we understand "something other than oneself" in general. I have reasons for supposing that other people are in this respect very similar to me. (Freud, 1901b, chap. 6, pp. 24-25)

Yet people not only compare themselves with others; they also do not want to be subordinated to others. Freud's most general explanation of the causes of parapraxes provides particularly relevant evidence of his emphasis upon human self-assertiveness:

In healthy people, egoistic, jealous and hostile feelings and impulses, on which the pressure of moral education weighs heavily, make frequent use of the pathway provided by parapraxes in order to find some expression for their strength, which undeniably exists but is not recognized by higher mental agencies. (Freud, 1901b, chap. 12, p. 276)

In Freud's view, then, parapraxes testify not only to our desire to speak freely but also to our egoistic character. Insofar as we are egoists, only with difficulty can we adjust to the constraints imposed upon us by social life. Thus, parapraxes suggest there is a conflict between the egoism that characterizes us as individuals and the consideration for others that social and political life demand of us. Twice in *Psychopathology* Freud explicitly discusses this conflict. In one passage, he criticizes conventional politeness: "Since I have come to recognize that I have often mistaken other people's ostensible sympathy for their real feelings, I have been in

revolt against these conventional expressions of sympathy, though on the other hand I recognize their social usefulness" (Freud, 1901b, chap. 7, p. 155). In a second passage, he argues that people should express their dislikes openly, rather than allow their parapraxes to express their dislikes for them indirectly: "And this is no doubt the punishment [i.e., that people take offense at our ostensibly unintentional neglect of them] for people's internal dishonesty in only giving expression under the pretext of forgetting, bungling and doing things unintentionally to impulses that would be better admitted to themselves and to others if they can no longer be controlled" (Freud, 1901b, chap. 9, p. 211).

To say that Freud postulates a conflict between individual self-interest and social cohesion is not, of course, to produce an original insight. Freud's major reason for writing *Civilization and its Discontents* was to explain the sources of this conflict, which he thought was chiefly responsible for the discontents that, he believed, characterize civilization. Nevertheless, Freud's argument in *Psychopathology* appears to differ from the later argument in *Civilization* in one important respect. *Civilization and its Discontents* attempts to show why all social orders are deficient but is not particularly concerned with differentiating among social orders; by contrast, *Psychopathology* might seem to suggest that some social orders—that is, liberal social orders, those which provide greater outlets for individual self-interest and for the desire for free self-expression—would therefore be superior to others. Even in *Psychopathology*, Freud implies that all social orders constrain the individuals who compose them; yet if all social orders do not exercise the same degree of constraint and if the degrees to which they do exercise constraint differ significantly, there might be good Freudian reasons for preferring liberal social orders to nonliberal ones.

It is true that Freud does not give us much in the way of normative guidance in *Psychopathology*. He does not tell us how to strike the balance between the "revolt" against "conventional expressions of sympathy" and the recognition of "social usefulness"; nor does he tell us when we should "admit" our hostile impulses to ourselves and when (and how) we should "control" them. Nevertheless, the evidence in *Psychopathology* indicates Freud's support for a society in which people would feel freer to speak and act honestly and openly on behalf of their own self-interest, even at the risk that this would lead to more conflict within society. Elsewhere, Freud provides a good explanation of his position in an argument he makes concerning the behavior of neurotics (which, I suggest, he would apply to social behavior generally):

A certain number of people, faced in their lives by conflicts which they have found too difficult to solve, have taken flight into neurosis and in this way won an unmistakable, although in the long run too costly, gain from illness. What will these people have to do if their flight into illness is barred by the indiscreet revelations of psychoanalysis? They will have to be *honest*, confess to the instincts that are at work in them, face the conflict, *fight for what they want*, or go without it; and the *tolerance* of society, which is bound to ensue as a result of psycho-analytic *enlightenment*, will help them in their task. (Freud, 1910d, pp. 149–50, emphasis added)

In other words, Freud advocates a liberal society, whose members will accept the extent to which they are Calibans and cease to have bad consciences for it. To achieve this end, that society must be enlightened by psychoanalysis and brought to a state of tolerance—tolerance of the fact that if human beings are honest with themselves and with one another, they will recognize that they must fight with one another for what they want. Free speech is essential for this society; it enables society's members to declare exactly why and for what they are fighting. Furthermore, it has the added advantage

of enabling them to fight with one another without physically harming one another, by providing them with the less dangerous verbal equivalent of war.

Jokes and their Relation to Freedom of Speech

Like *The Psychopathology of Everyday Life*, Freud's work *Jokes and their Relation to the Unconscious* emphasizes both the inevitability of conflict and the utility of speech as an outlet for conflict. Freud explains the purposes of jokes in this way: "There are only two . . . and these . . . can themselves be subsumed under a single heading. It is either a *hostile* joke (serving the purpose of aggressiveness, satire or defence) or an *obscene* joke (serving the purpose of exposure)." Jokes "make possible the satisfaction of an instinct (whether lustful or hostile) in the face of an obstacle that stands in its way" (Freud, 1905c, chap. 3, pp. 97 and 101, respectively).

Freud's discussion of hostile instincts is of particular importance for my argument, for it is another clear prefiguration of Freud's treatment of aggression in *Civilization and Its Discontents* some 25 years later:

Since our individual childhood, and similarly, since the childhood of human civilization, hostile impulses against our fellow men have been subject to the same restrictions, the same progressive repression, as our sexual urges. We have not yet got so far as to be able to love our enemies or to offer our left cheek after being struck on the right. Furthermore, all moral rules for the restriction of active hatred give the clearest evidence to this day that they were originally framed for a small society of fellow clansmen. In so far as we are all able to feel that we are members of one people, we allow ourselves to disregard most of these restrictions in relation to a foreign people. Nevertheless, within our own circle we have made some advances in the control of hostile impulses. (Freud, 1905c, chap. 3, p. 102)⁹

These "advances" consist in the substitution of hostile words for hostile deeds and then in the substitution of ostensibly jok-

ing hostile words for serious ones:

As Lichtenberg puts it in drastic terms: "Where we now say 'Excuse me!' we used to give a box on the ears." Brutal hostility, forbidden by law, has been replaced by verbal invective. . . . Since we have been obliged to renounce the expression of hostility by deeds . . . we have developed a new technique of invective. . . .

. . . We are now prepared to realize the part played by jokes in hostile aggressiveness. A joke will allow us to exploit something ridiculous in our enemy which we could not, on account of obstacles in the way, bring forward openly or consciously; once again, then, the joke will evade restrictions and open sources of pleasure that have become inaccessible. (Freud, 1905c, chap. 3, pp. 102-3)

In short, Freud claims that jokes psychologically benefit us by enabling us to express hostility that in the absence of jokes we would feel obliged to suppress.

The political implications of Freud's contention that jokes serve us as a vehicle for the expression of hostility are apparent from Freud's own examples. To begin with, it is of some interest that the contemporary figure whose jokes Freud cites most often is himself a political figure, whom Freud refers to by the alias "Herr N."¹⁰ Herr N. is

one of the leading men in Austria, who, after important scientific and public work, now fills one of the highest offices in the State. I have ventured to make use of the jokes which are ascribed to him, and all of which in fact bear the same impress, as material for these researches, above all because it would have been hard to find any better. (Freud, 1905c, chap. 2, p. 22)

Without exception, the jokes of Herr N. that Freud quotes are all insult jokes, or "pieces of invective" (Freud, 1905c, chap. 3, p. 103). Herr N.'s verbal aggression must be expressed in the form of jokes, Freud explains, because

the high position he occupies makes it impossible for him to give out his [critical] judgements [as bluntly as he would like by] . . . exclaim[ing them] aloud. . . . They therefore bring in a joke to their help, and this guarantees them a reception with the hearer which they would never have found in a non-joking form, in spite of the

truth they might contain. (Freud, 1905c, chap. 3, p. 103)

Herr N. is characterized by

a strong inclination to invective [that] is held in check by a highly developed aesthetic culture. By the help of a joke. . . . [his] inhibition is lifted. . . . The satisfaction of [his] purpose is made possible and its suppression, together with the "psychical damming-up" that this would involve, is avoided. (Freud, 1905c, chap. 4, p. 118)

Herr N. is a public figure who has "a strong inclination to invective," but his "high position . . . makes it impossible for him" to give vent to his invective freely, "in spite of the truth" its expression "might contain." The example of Herr N. is significant for my argument because he is emblematic of public life as Freud generally conceives of it, both in his desire to insult and in his inability to insult openly. According to Freud, political life gives us cause to want to take out our aggressiveness upon others but also often prevents us from doing so, in words as well as in deeds; in particular, political life causes subjects to resent their sovereigns but often makes it difficult for them to express this resentment.

These characteristics of political life are revealed in the two most politically significant jokes Freud recounts. Both jokes are told of and to sovereigns by their subjects, and both enable the subjects to circumvent restrictions on their freedom of speech and indirectly to make criticisms of the sovereigns that they would be unable to make directly. The first joke concerns the relation of a physician to his king:

The King condescended to visit a surgical clinic and came on the professor as he was carrying out the amputation of a leg. He accompanied all its stages with loud expressions of his royal satisfaction: "Bravol bravol my dear Professor." When the operation was finished, the professor approached him and asked him with a deep bow: "Is it your Majesty's command that I should remove the other leg too?" (Freud, 1905c, chap. 2, pp. 72-73)

The political import of this joke is evi-

dent: the physician resents what he takes to be the king's assumption of his subservience to the king. The physician does not feel that he is at liberty directly to express his resentment, however; instead he expresses his resentment indirectly, by making the joke, and thereby "making himself understood by saying the opposite of what he thinks but must keep to himself" (Freud, 1905c, chap. 2, p. 73). The joke enables him, a subject, to criticize his sovereign, notwithstanding his political subordination to his sovereign; it enables him to circumvent the censorship that can restrict subjects' freedom of speech with respect to their sovereign. In this sense, his joke is a blow struck on behalf of equality between subjects and sovereigns, on behalf of the extension of freedom of speech to subjects as well as to sovereigns.

This joke is of particular interest in that one can discern a subjective, autobiographical reason for Freud's recounting of it. Freud himself was a physician who believed that he had for a long time been unjustly subordinated by a monarchy: he believed that political interference from Austria-Hungary's imperial government had been responsible for his having been passed over initially for promotion to a professorship at the University of Vienna¹¹ (and subsequently for his receiving it).¹² One of Freud's teachers spoke to him about "personal influences which appeared to be at work against [Freud] with his Excellency [i.e., the imperial Minister of Education, who was empowered to make decisions concerning promotions at the state-run university] and . . . advised [Freud] to seek a personal counter-influence" (Freud, 1954, p. 343, letter to Wilhelm Fliess of 11 March 1902). When at last his promotion was approved, Freud wrote that it was "as if the role of sexuality had been personally recognized by His Majesty, the interpretation of dreams confirmed by the Council of Ministers, and the necessity of the psycho-analytic therapy of hysteria car-

ried by a two-thirds majority in Parliament" (Freud, 1954, p. 344, letter to Fliess of 11 March 1902). Common to both the physician in the joke and to Freud in real life is a resentment at the political assumption that the work of scientists should be thought to be subject to the judgments, whether approving or disapproving, of politicians.

Nor need we deduce Freud's resentment of subjection to monarchic authority and of the need to placate it from this joke alone. He makes his rejection of kings' right to rule commoners perfectly explicit elsewhere in *Jokes*:

"We, by the ungrace of God, day-labourers, serfs, negroes, villeins . . ." is how Lichtenberg begins a manifesto (which he carries no further) made by these unfortunates—who certainly have more right to this title than kings and princes have to its unmodified form. (Freud, 1905c, chap. 2, p. 77)¹³

In addition, Freud's other antimonarchist joke is relevant here:

Serenissimus was making a tour through his provinces and noticed a man in the crowd who bore a striking resemblance to his own exalted person. He beckoned to him and asked: "Was your mother at one time in service in the Palace?"—"No, your Highness," was the reply, "but my father was." (Freud 1905c, chap. 2, pp. 68–69)¹⁴

Freud's subsequent analysis of this joke is of great importance:

The person to whom the question was put would no doubt have liked to knock down the impertinent individual who dared by such an allusion to cast a slur on his beloved mother's memory. But the impertinent individual was Serenissimus, whom one may not knock down or even insult unless one is prepared to purchase that revenge at the price of one's whole existence. The insult must therefore, it would seem, be swallowed in silence. But fortunately a joke shows the way in which the insult may be safely avenged. (Freud, 1905c, chap. 3, p. 104)

With speech as his vehicle, Serenissimus's interlocutor finds an outlet for the resentment that he feels as a result of his subjection to an insulting and "impertinent"

sovereign. Only that he can convert his criticism of the sovereign into a joke enables him to air his criticism; he finds it possible to voice in jest an attack that he could not possibly voice in earnest. In this example, as in the previous one, a joke allows a subject to satisfy the serious desire to criticize his political superior. In this sense, one can regard political jokes as opening wedges in the effort to expand political freedom of speech or, at any rate, as compensating substitutes given prevailing restrictions on freedom of speech.

In his gloss on the subject's retort to Serenissimus, Freud again makes his political point quite clearly:

Tendentious jokes are especially favoured in order to make aggressiveness of criticism possible against persons in exalted positions who claim to exercise authority. The joke then represents a rebellion against that authority, a liberation from its pressure. The charm of caricatures lies in this same factor: we laugh at them even if they are unsuccessful simply because we count rebellion against authority as a merit. (Freud, 1905c, chap. 3, p. 105)

Thus, like political criticism generally, the joke is a vehicle specifically for the expression of discontent with the inequalitarian relationship between ruler and ruled. In this respect, the desire for freedom to criticize political superiors reflects an egalitarian discontent with the fact that there are political superiors. Freedom of political speech thus acts as a palliative in response to this discontent: it enables those who are ruled, and are therefore politically inferior, to achieve a partial equality with their rulers—their political superiors—by cutting them down to size. Freud alludes to this egalitarian dimension of comedy when he speaks of

the method of degrading the dignity of individuals by directing attention to the frailties which they share with all humanity, but in particular the dependence of their mental functions on bodily needs.¹⁵ The unmasking is equivalent here to an admonition: such and such a person, who is admired as a demigod, is after all only

human like you and me. (Freud, 1905c, chap. 7, p. 202)

If it is inevitable that there be an inequalitarian relationship between subject and sovereign, it is at any rate preferable to mitigate that inequality by allowing subjects to joke (and by extension to speak) more freely about and to sovereigns. Here, too, we see that even if all political orders arouse discontent, some political orders (i.e., liberal ones), in which political freedom of speech is permitted, might be preferable to others in that they might arouse significantly less discontent. In this respect, the liberalism of *Jokes* is surprisingly reminiscent of the liberalism of Kant's essay "What Is Enlightenment?": if Kant (1963, pp. 5, 10) contends that the watchword of the liberal state is to "argue . . . but obey!" so does Freud suggest that we "joke, but obey." If we are to be constrained to obey, the least that we can obtain in compensation is the freedom to laugh at those who constrain us to do so.

Freedom of Speech and Catharsis

We have seen indications in both *The Psychopathology of Everyday Life* and *Jokes and their Relation to the Unconscious* that Freud advocates free speech as an outlet for aggression. In order to get a better sense of how such an outlet works and why it is necessary, we must see how Freud's advocacy of free speech is grounded in his psychological theory proper. In this context, one of Freud's earliest works, which he wrote in collaboration with Breuer, is of particular importance. In the work (which is a "preliminary communication" of the findings that they were subsequently to publish in their book-length study of hysteria), Freud and Breuer explain that hysterical symptoms arise when people fail to find an outlet for their aroused emotions:

The fading of a memory or the losing of its affect depends on various factors. The most important of these is *whether there has been an energetic reaction to the event that provokes an affect*. By "reaction" we here understand the whole class of voluntary and involuntary reflexes—from tears to acts of revenge—in which, as experience shows us, the affects are discharged. If this reaction takes place to a sufficient amount a large part of the affect disappears as a result. Linguistic usage bears witness to this fact of daily observation by such phrases as "to cry oneself out" [*sich ausweinen*], and to "blow off steam" [*sich austoben*, literally, "to rage oneself out"]. If the reaction is suppressed, the affect remains attached to the memory. An injury that has been repaid, even if only in words, is recollected quite differently from one that has had to be accepted. (Freud and Breuer, 1893a, p. 8)

This passage is crucial for an understanding of the psychological underpinnings of Freud's advocacy of free speech because it points to what is distinctively psychoanalytic in this advocacy, as opposed to the more familiar reasons for it that Freud holds in common with earlier liberals. According to Freud, the benefit provided by free speech is primarily psychological and emotional, not political or intellectual: free speech is beneficial because it enables us to "blow off steam" or "to get things off our chest." Freudian theory celebrates free speech, not primarily because it enables the individual or the community to make wise decisions—to choose the most suitable offering at the marketplace of ideas—but because we are psychologically better off when we are able to express our emotions than we are when we must suppress them. The exercise of free speech helps us to feel better more than it helps us to think better or to choose better.¹⁶

The theme of the emotional benefits that result from "blowing off steam" recurs in a little-known but instructive essay, "Psychopathic Characters on the Stage," which Freud wrote around 1905 and presented to a friend but never published. In the essay, Freud explains how attending theatrical performances can have positive psychological consequences:

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The purpose of drama is to . . . open up sources of pleasure or enjoyment in our emotional life. . . . In this connection the prime factor is unquestionably the process of getting rid of one's own emotions by 'blowing off steam'; and the consequent enjoyment corresponds . . . to the relief produced by a thorough discharge. (Freud, 1942a [1905-6], p. 305)

Spectators achieve this relief vicariously; what they cannot accomplish in deed in their real lives is accomplished for them in speech—the speech of the drama's hero, whose imaginary life is depicted on the stage:

The spectator is a person who experiences too little, who . . . longs to feel and to act and to arrange things according to his desires—in short, to be a hero. And the playwright and actor enable him to do this by allowing him to *identify himself* with a hero. . . . In these circumstances he can allow himself to enjoy being a "great man," to give way without a qualm to such suppressed impulses as a craving for freedom in religious, political, social and sexual matters, and to "blow off steam" in every direction in the various grand scenes that form part of the life represented on the stage. (Freud, 1942a [1905-6], pp. 305-6)

We learn from Freud's analysis of the drama that one can blow off steam, not only by speaking oneself, but also by listening to others who dare to speak what one fears to say oneself. In either case, however, speech (one's own or another's) is necessary if steam is to be blown off, if the "suppressed . . . craving for freedom in religious, political, social and sexual matters" is to be satisfied. Speech both partially compensates us for and provides us with a surrogate for what we are denied in deed. The political and social constraint that is insurmountable in deed is to some extent overcome when we are granted freedom of speech.

Freud begins his essay with a reference to Aristotle's *Poetics*: It "has been assumed since the time of Aristotle [that] the purpose of drama is to arouse 'terror and pity' and so 'to purge the emotions'" (Freud, 1942a [1905-6], p. 305). The reference to "purg[ing] the emotions" is,

of course, an allusion to the Aristotelian doctrine of catharsis, with which Freud evidently was familiar. For this reason, it is noteworthy that *catharsis* was the name Freud borrowed from Breuer to designate the form of psychotherapy that Freud later transformed into psychoanalysis.¹⁷ When Anna O., the first analysand, spoke of the "talking cure" (Freud and Breuer, 1895d, chap. 2, sec. 1, p. 30) and when Freud and Breuer (1893a, p. 17) claimed that their "psychotherapeutic procedure . . . brings to an end the operative force of the idea which was not abreacted in the first instance, by allowing its strangled affect to find a way out through speech," they were referring to catharsis—Freud did not employ the term *psycho-analysis* until 1896 (see Freud, 1896a, p. 151).

As it happens, there is one scholarly interpretation of the Aristotelian doctrine of catharsis that is of particular interest because of its possible connection with the Freudian doctrine of catharsis. This interpretation appears in an essay written by Freud's wife's uncle, Jacob Bernays, which led to "widespread interest in catharsis. . . . [After the essay's republication in 1880] catharsis was one of the most discussed subjects among scholars" (Ellenberger, 1970, p. 484).¹⁸

In Bernays's presentation, Aristotelian catharsis can almost seem to be a precursor to the catharsis of Breuer and Freud, for Bernays portrays Aristotelian catharsis as a medical phenomenon. He claims that catharsis is "a designation for treatment of a disturbed person, in which the physical influences what is mental; it does not seek to transform or to suppress [*zurückzudrängen*] the disturbing element but to stir it up, urge it forward, and thereby achieve the alleviation of the disturbance" (Bernays, 1880, p. 16). Bernays suggests that catharsis be seen as a process of "excitation of pity and fear," leading to "the alleviating discharge of such . . . emotional states" and argues

that this view of catharsis is superior to others in that it "allows the medical metaphor to shine through, as Aristotle did himself in [his reference to catharsis] in the *Politics*" (Bernays, 1880, p. 21).

As we have seen, Freud's advocacy of free speech is clearly reminiscent of the medical understanding of catharsis proposed by Bernays. The passions (or, to use a term employed by Bernays [1880, p. 66] as well as by Freud, [Freud-Breuer, 1893a, p. 8] the "affects") can be "disturbing element[s]." They should be dealt with, not by being "suppress[ed]," but by being "stir[red] . . . up" and "discharge[d]."¹⁹ For Bernays, the spectator of the theatrical piece achieves "the alleviating discharge"; for Freud, people who allow themselves freely to speak their own piece achieve a comparable alleviating discharge.

Freud and Liberal Defenses of Freedom of Speech

As I have argued, Freud's defense of free speech is primarily a psychological one. Nevertheless, there are important political and sociological presuppositions underlying it that should be made clear because they point to an important affinity between Freud and an element within the liberal tradition.

We have seen that Freud advocates free speech because of his belief in the utility of blowing off steam. We have not yet really considered, however, why it is that steam must be blown off. This question is implicitly answered in those passages in *Psychopathology and Jokes* that prefigure the critique of social life that Freud was later to elaborate in *Civilization and its Discontents*: it is already apparent in *Psychopathology and Jokes* that the "steam" that must be blown off results from the friction generated by social interaction. Society and government must restrict people to keep them from harming

one another, but this restriction itself is perceived by people as a kind of harm. That being the case, the liberalism evident in Freud's advocacy of free speech is intelligible as a partial response to the anti-social character of human beings that he discusses in *Civilization*: it is in the interest of both government and subjects to limit the government's restrictions upon its subjects, to the extent it is possible and safe to do so. People must be prevented from hurling spears at one another because spears cause physical damage; since people arguably do little real damage to one another by hurling words of abuse, a prudent government might decide to grant people an outlet for their aggressiveness (at the same time lessening their frustration at being governed) by granting them freedom of speech—by freeing them to hurl words of abuse at one another. In a way, Freud's view is expressed in the words of the childhood chant "Sticks and stones may break my bones, but words can never harm me."²⁰ In view of Freud's belief that childhood in some way reveals the deepest truth about human nature, it is altogether appropriate that this slogan's sentiment should underlie his psychoanalytic advocacy of free speech.²¹

In different forms, arguments like this can be found in a number of liberal thinkers. Hobbes (1972, chap. 13, sec. 17) speaks of "the harmless . . . liberty of subjects." For Hobbes, as for Freud, subjects must be denied the liberty that leads them to harm one another but, in compensation, they can and should be granted "harmless liberty." In order for people to enjoy this harmless liberty, "it is necessary that there be infinite cases which are neither commanded nor prohibited, but every man may either do or not do them as he lists himself. . . . The more is left undetermined by the laws, the more liberty [subjects] enjoy." To secure subjects their harmless liberty, Hobbes (1972, chap. 13, sec. 15) counsels governments against excessive legislation,

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wherein "such things are forbidden as reason of itself prohibits not of necessity."

Hobbes's argument is, of course, about liberty generally not about freedom of speech specifically. A version of his argument is made more specifically relevant to the issue of freedom of speech by a later liberal, Adam Smith. Smith (1937, bk. 5, chap. 1, pt. 1, p. 668) writes that "that degree of liberty which approaches to licentiousness can be tolerated only in countries where the sovereign is secured by a well-regulated standing army" and that "the rudest, the most groundless, and the most licentious remonstrances . . . can give little disturbance . . . to a sovereign . . . supported . . . by a well-regulated standing army." For this reason, Smith contends, supporters of personal freedoms (such as freedom of speech) should also support the strength and power of the sovereign.²² Smith presents freedom of speech as a benefit that subjects can enjoy only when they live under a government strong enough to keep the peace securely. Thus, Hobbes implies and Smith states that people can be free to hurl words of abuse at one another only when their sovereign is in sufficient control of their actions to prevent them from hurling spears at one another (and at him). Freud amplifies their argument by asserting that permission to hurl words provides an additional social benefit: it compensates the subjects, by enabling them to blow off steam, for the frustration they feel as a result of being prevented from freely employing their spears.

The premises behind all of these arguments are that obedience is a condition that people resent, that they can be compelled to obey, but that it is prudent to avoid employing more coercion than is strictly necessary. It follows from these premises that people can perhaps be coaxed to obey in some instances by not being compelled to obey in others. Thus, in order to secure the obedience of their subjects when they need to have it,

governments should take care not to insist upon the obedience of their subjects when they do not need to have it. This conclusion was well stated in 1841 by the liberal historian Macaulay (1907, p. 419): "It may be laid down as a universal rule that a government which attempts more than it ought will perform less." Freud's advocacy of free speech rests upon a similar assumption: that government should not attempt altogether to repress human aggressiveness but should instead allow it to be harmlessly expressed in speech.²³

Freud and the classical liberals agree on the advisability of freeing speech from political controls, but there is an important respect in which his attitude toward speech differs from theirs. It is true that some liberals share Freud's view that free speech is defensible in part as a safety valve that permits people to express their frustrations harmlessly, but this is far from being the only defense of free speech that liberals historically have offered. Liberals have celebrated free speech not only because it does no harm but also because it can do much political good; they have contended that free political debate enables the political alternatives to emerge more clearly, which enables the political community to choose more wisely among them. For this reason, liberals like John Stuart Mill have praised speech for its capacity to arouse people to action and to transform the political world.

This aspect of the liberal defense of free speech has no analogue in the psychoanalytic defense of free speech that we have been considering. For this reason, Freud's defense of free speech is in some respects also a critique of speech: insofar as Freud advocates freeing the everyday speech of ordinary political and social encounters to create a safety valve for the aggressiveness that such conflictual encounters engender, he depreciates the significance of free speech by being oblivious to its capacity to transform the political and social world.

A Political Critique of Freud

Freud's critique of political speech is apparent in his understanding of speech as an *Ersatz* for a deed; speaking is a "less adequate" means for working off an affect than is doing (Freud, 1893h, p. 36; cf. the reference to an injury being repaid, "even if only in words" [emphasis added] in Freud and Breuer, 1893a, p. 8, quoted above). It is preferable to complain about an intolerable situation than to ignore it or to pretend that it is tolerable, but to complain about it is still very different from and far inferior to the taking of steps actually to alter and improve the situation. A quotation from Lessing employed by Freud suggests the problem I am addressing here: "Not all are free who mock their chains" (Freud, 1905c, chap. 3, p. 92). It is, again, better to mock one's chains than to celebrate one's confinement, but it is better still to break one's chains and to end one's confinement.

To complain about one's confinement truly liberates insofar as it leads to deeds that end confinement. Insofar as it serves only as an *Ersatz* for ending confinement, complaining may increase that confinement by making it seem less intolerable. For this reason, Freud is somewhat ambivalent about free speech, and for this reason, as I have already suggested, Freud's favorable assessment of free speech prefigures Marcuse's condemnation of repressively tolerant free speech. To make this point differently, Freud, like Marx (whose intellectual career also began with a condemnation of censorship),²⁴ wants primarily to "change" the world: if Marx fears that speech can assist us only to "understand" the world (see the last of the "Theses on Feuerbach," in Marx and Engels, 1978, p. 145), Freud fears that it can assist us only to express our resentment of it. Accordingly, in Freud's view as in Marx's, people "are in no way combating the real existing world when they are merely combating

the phrases of this world" (*The German Ideology*, in Marx and Engels, 1978, p. 149).

The affinity between the Freudian defense and the Marcusean critique of free speech in itself suggests the former is in some way deficient. It is deficient because an adequate defense of free speech must be Millian—that is, must celebrate the capacity of free speech to improve political deliberation—as well as psychoanalytic. To be fully defensible, speech must be thought to be more than a substitute for action in the external world; it must also be thought to be a preparation for action in the external world, in fact itself a form of action in the external world.

Freud is, of course, quite correct to note that words are often not followed by deeds—that in fact a word often is nothing but an *Ersatz* for a deed. Nevertheless, the view of the word as an inferior substitute for the sword is inadequate. It is particularly inadequate with respect to politics and still more particularly so with respect to liberal politics, for in politics the word does not so much replace the sword (as the means through which we take out our aggression on others) as it directs the employment of the sword. In all regimes (and particularly deliberative regimes) it is through words that political figures determine how the sword is to be employed, who is to employ it, and against whom it is to be employed. This is to say that speech serves not only as a vehicle for the expression of emotion but also (and more importantly) as the medium in which political decisions are made; an adequate defense of free speech must recognize this deliberative function of speech as well as the expressive function emphasized by Freud.

Nor is this the only respect in which Freud's defense of free speech is somewhat inapplicable to politics. At best, his theory suggests an ideal (speakers' ability freely and unreservedly to give vent to

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their emotions) that even good political communities (and especially, in some measure, good political communities) can and should never approach.

I say this because the completely candid airing of one's views, however therapeutic it may be, is out of place in politics.²⁵ Language itself suggests this in the etymological relationship between "politics" and "polite," in the phrase "an impolitic remark." It is worth recalling the Swiss Professor N., who is said by Freud's informant to have uttered the neologism *moche* in part in reaction to his "anxiety about committing a political indiscretion" (Freud, 1901b, chap. 5, p. 73); in his case and in many others, to say what one would like to say is to say something that politically one ought not to say.

Politics requires discretion for the very same reasons that social life in general does, and there need be nothing hypocritical about this requisite discretion. In politics as in social life generally, words are intended to have consequences for the behavior of others; thus, speakers weigh their words more or less carefully and do not utter the first thing that comes into their minds out of consideration for the possible consequences of their words upon others. With respect to politics, better political communities, in which statesmen are more concerned with the welfare and the political views of those whom they rule, might in fact require their statesmen to be more cautious and circumspect in their political speech than would worse political communities.

Even if it is true that good political communities enable their citizens freely to voice the truth as they see it, political figures must nevertheless exercise caution and circumspection in the way they choose to express the truth. Political success requires one to have supporters, and political rhetoric must be employed in order to attain and to maintain support. Simply voicing the truth is insufficient; the statesman must also consider how to

voice what he takes to be the truth—how to express it so as to make it most persuasive to his listeners. Thus, politics necessarily places constraints on self-expression: the aim of political speakers is not primarily to express their views but to induce others to accept them.

A political defense of free speech must therefore take into account the effect of speech upon its listeners. Freud's defense is too apolitical in that it emphasizes the effect upon the speaker.²⁶ In all political circumstances, consideration of the listeners will constrain the speaker. In some respects, liberal democracies may constrain the speaker less than other governments, but even liberal democracies cannot entirely make possible the sort of therapeutic blowing off of steam that Freud emphasizes in his advocacy of free speech.²⁷ To advocate free speech on psychoanalytic grounds, in other words, is perhaps less to advocate free government than it is to advocate no government.

There is something solipsistic about the psychoanalytic defense of freedom of ordinary political and social speech, in which the goal of speech is less to persuade the listener than to heal the speaker. By emphasizing the emotional value of using words to express one's feelings, Freud deemphasizes the political value of using words to persuade others to accept one's ideas; thus the psychoanalytic defense of free speech can be said to be excessively "expressive" (of the speaker) and insufficiently "impressive" (i.e., intended for the audience). If free speech is reduced to getting things off one's chest, one can too easily forget that its purpose is primarily to get ideas out of one's head and, more importantly, to get them into the heads of others.

In this respect, it is interesting to consider what has been called "one of the more curious developments in recent intellectual history[:]" . . . the metamorphosis of freedom of speech and press into

freedom of expression *tout court*. . . . The glide from 'the freedom of speech' . . . to 'freedom of expression' has been a significant one" (Canavan, 1984, pp. xi-xii). Following Canavan, I suggest this metamorphosis is significant because it points to a decreasing concern with the specifically political character that speech potentially has. Many forms of self-expression are not and cannot be political (e.g., playing musical instruments) because they are not and cannot be used for purposes of persuasion. Speech can be persuasive (although, of course, it need not be intended to be); for that reason, freedom of speech (unlike the freedom of other forms of self-expression) is politically essential if self-government is to be ensured. It may be beneficial for a speaker to express his emotions; but for him to do so is very different and very much less important politically than it is for him to express his ideas persuasively and coherently.

The contemporary shift from freedom of speech to freedom of expression, which results from the presupposition of the essential equivalence of all the myriad forms of self-expression, is significantly prefigured in Freud's psychoanalytic understanding of speech; this leads him to neglect the possible political significance of speech. More importantly, his emphasis upon the affective as opposed to the intellectual significance of words leads him, in a sense, to depreciate the significance of speech altogether.²⁸ Speech is not always needed to express emotions—grunts and grimaces can often accomplish this just as well. Speech is, however, needed to express ideas. To the extent that Freud himself deemphasizes this, one can almost say there are Freudian grounds for the attempt to a contemporary psychotherapist to outdo Freud by suggesting "the primal scream" as a suitable substitute in the psychotherapeutic situation for the lengthy, hypervocal exchange with an analyst.²⁹

Thus, although Freudian psychoanalysis provides a defense of free speech, it is a partial defense that does not defend it for all of the right reasons. In defending free speech, it may be helpful to recall that Aristotle speaks in the *Poetics* of the utility of purging our emotions; it is still more helpful to recall that he also speaks, in the *Rhetoric*, of the utility of employing our emotions to persuade our listeners to accept our political ideas. It is also helpful, in defending free speech, to recognize its role in securing free government and to recall the benefits that can result from free government as well as (and as opposed to) the constraints imposed by all government.

Notes

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1. See Eastman (1942, p. 270): "What are you politically?" I asked. [Freud answered:] "Politically I am just nothing."

2. See Freud's letter to Arnold Zweig of 26 November 1930: "I remain a liberal of the old school" (Freud and Zweig, 1970, p. 21).

3. It is important to realize, however, that the analogy is only a partial one. It is true that both psychoanalytic patients and average healthy social beings benefit from "telling things" and from the ensuing discharge of disturbing emotions; but the patients benefit more because they can tell things to an analyst, who is trained (as ordinary social interlocutors are not) to interpret their statements and to assist them in understanding and dealing with their disturbing emotions. As will become clear below, the ordinary members of society receive a catharsis from speaking freely; they do not, of course, receive a psychoanalysis.

4. See *Civilization and its Discontents*: "Men are not gentle creatures who want to be loved, and who at the most can defend themselves if they are attacked; they are, on the contrary, creatures among whose instinctual endowments is to be reckoned a powerful share of aggressiveness. As a result, their neighbour is for them not only a potential helper or sexual object, but also someone who tempts them to satisfy their aggressiveness on him" (Freud, 1930a, chap. 5, p. 111).

5. See "Instincts and Their Vicissitudes": "Hate, as a relation to objects, is older than love. It derives

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from the narcissistic ego's primordial repudiation of the external world with its outpouring of stimuli" (Freud, 1915c, p. 139).

6. See, "'Civilized' Sexual Morality and Modern Nervous Illness," where Freud (1908d, p. 197) criticizes the "well-behaved weaklings who . . . become lost in the great mass of people that tends to follow, unwillingly, the leads given by strong individuals."

7. Freud's German word, correctly translated as "candid," is *freimütiger* (literally, "free-spirited"). The translation conceals an allusion to freedom in the German text: Herr A.'s sons, who might speak freely and reveal their Jewishness, are free in a way that Herr A. himself is not. Note also that the sons' *Freimütigkeit* is the opposite of the "faintheartedness" (*Schwachmütigkeit*) to which, as we have seen, Freud himself "like[s] to avoid concessions."

8. Freud's German word, translated as "more frankly," is again *freimütiger*, and again the translation conceals an allusion to freedom in the German, which suggests that the Austrians were freer when they could openly complain about their German allies than they were when they could not (cf. note 7, above).

9. Compare this passage with that on pages 108–16 of *Civilization and Its Discontents* (Freud, 1930a, chap. 5), in which Freud again discusses the impracticability of the injunction to "love thine enemies" (p. 110) and again contends that "a comparatively small cultural group . . . allow[s] the aggressive instinct [an outlet in the form of hostility against invaders]" (p. 114).

10. According to Strachey's editorial note, "it seems likely that Herr N. was Josef Unger (1828–1913), Professor of Jurisprudence and from 1881 President of the Supreme Court" (Freud, 1905c, chap. 2, p. 22).

11. Note that the physician in Freud's joke is always referred to as "the professor."

12. See Jones (1953–57, vol. 1, pp. 338–41). Freud's promotion was passed over from 1897 through 1901 and was finally granted in 1902.

13. For Freud's rejection of monarchy, see also Schorske (1981, p. 189), who, in listing "the political values [Freud] retained all his life," speaks of Freud's "contempt for royalty and aristocracy (in 1873 as a senior in the Gymnasium Freud had proudly refused to doff his hat to the emperor)."

14. In an editorial note, Strachey explains that *Serenissimus* was "the name conventionally given to Royal Personages by comic periodicals under the German Empire" (Freud, 1905c, chap. 2, p. 68).

15. This statement is of additional interest in that it points to an egalitarian thrust underlying Freud's own work: few figures, after all, have stressed "the dependence of . . . mental functions on bodily needs" more than Freud.

16. In this context, it is instructive to compare John Stuart Mill with Freud. In *On Liberty* Mill, like

Freud, argues that people benefit psychologically from having and from exercising freedom of speech, but Mill's argument emphasizes the free development of the intellect, whereas Freud's psychoanalytic argument emphasizes the free play to be given to the emotions: "The greatest harm done is to those . . . whose whole *mental development* is cramped, and their *reason* cowed, by the fear of heresy. Who can compute what the world loses in the multitude of promising *intellects* combined with timid characters, who dare not follow out any bold, vigorous, independent train of *thought*?" (Mill, 1961, p. 284, emphasis added). This contrast does not, of course, apply to Freud's nonpsychoanalytic or Millian argument in behalf of intellectual freedom, which is particularly evident in *The Future of an Illusion* (see discussion above).

17. For an account of the transformation of catharsis into psychoanalysis, see *An Autobiographical Study* (Freud, 1925d [1924]; chaps. 2–3, pp. 19–31).

18. There is good reason to believe Freud was both generally familiar with Aristotle's *Poetics* and specifically acquainted with Bernays's interpretation of it. Freud's library contained two translations of the *Poetics* out of only five classical Greek works (Trosman and Simmons, 1973, p. 679). Furthermore, one of the translations contains an essay in which "the Jacob Bernays interpretation of the Aristotelian theory of catharsis is in the main taken as a basis" (von Berger, 1897, p. 73); the essay goes on to assert that "the cathartic treatment of hysteria, which the physicians Josef Breuer and Sigmund Freud have described, is very suitable for making the cathartic effect of tragedy understandable" (von Berger, 1897, p. 81). The translation to which the essay is attached is by an acquaintance of Freud, the noted classicist Theodor Gomperz, who hired Freud to translate some of John Stuart Mill's works into German and whose wife, who at one time was a patient of Freud's, attempted to use her influence to secure for Freud the promotion that he desired at the University of Vienna (see Jones, 1953–57, vol. 1, pp. 55–56, 340, and discussion above). In light of Freud's acquaintance with Gomperz, I think it particularly unlikely that he should have been unaware of the views of the eminent classicist whose niece he had married.

19. Note that Freud's word, translated as "repress" (*verdrängen*), shares a common root with Bernays's word that I have translated as "suppress" (*zurückdrängen*), differing only in its employment of another prefix.

20. To say that words do little harm is, of course, to tell only half of Freud's story: it is true that abusive words do not harm their recipients as much as physical attacks do, but it is equally true (and at least as important) that abusive words do help their speakers (though not as much as physically attacking their opponents might help them).

21. The Freudian argument that I have reconstructed here on behalf of free speech is, however, turned on its head by Marcuse (1969). For Freud, free speech is a compensation granted to subjects because the necessity that subjects obey is taken for granted (as is the subjects' perception that obedience is unpleasant). For Marcuse, since obedience ought not to be taken for granted, free speech is problematic: subjects should not settle for the compensation that the right to speak freely offers them but instead should cease to obey; by ceasing to blow off steam and ceasing to obey, subjects could and should bring the "[steam] engines of repression" to a halt. Marcuse (1969, p. 115) criticizes "nonconformity and letting-go in ways which leave the real engines of repression in the society entirely intact, which even strengthen these engines by substituting the satisfactions of private and personal rebellion for a more than private and personal, and therefore more authentic, opposition." I return to the connection between the arguments of Freud and Marcuse below.

22. The same argument is made by Kant (1963, p. 10): "Only one who . . . has a numerous and well-disciplined army to assure public peace, can say: 'Argue as much as you will, and about what you will, only obey!'"

23. It is noteworthy that one of Freud's judgments about neuroses makes the same point in a medical context that Macaulay makes in a political context: "Experience teaches us that for most people there is a limit beyond which their constitution cannot comply with the demands of civilization. All who wish to be more noble-minded than their constitution allows fall victims to neurosis; they would have been more healthy if it could have been possible for them to be less good" (Freud, 1908d, p. 91).

24. See Cuddihy (1974, p. 121): "In February 1843 the first political article Marx ever wrote, on censorship, appeared in the Swiss magazine *Anekdoten*. Significantly, Freud's first great work appearing at the century's end, *The Interpretation of Dreams*, was also to deal with censorship."

25. In this context, Freud's description of his unsuccessful attempt to psychoanalyze a political figure is of some interest, suggesting as it does the potential conflict between the psychoanalytic insistence on candor and the prudence demanded by politics: "I once treated a high official who was bound by his oath of office not to communicate certain things because they were state secrets, and the analysis came to grief as a consequence of this restriction" (Freud, 1913c, p. 136).

26. I should make it clear that I do not assert that Freud personally was altogether unaware of the considerations I raise here or that he would altogether have denied their relevance; recall his "recognition of the] social usefulness" of "conventional expressions of sympathy" (Freud, 1901b, chap. 7, p. 155). What I do assert is that Freud's psychoanalytic defense of free speech is partial at best; it is in-

adequate to the considerable extent that it fails sufficiently to emphasize the social and persuasive character of speech.

27. See Podhoretz (1984, p. 56): "In a time when nothing is concealed, whether in private life or public, when all the formerly dirty little secrets are being uncovered, when all the 'closets' are being emptied, when what used to be covert, whether in Nicaragua or the bedroom, is now exposed to the full glare of publicity—when, in short, the whole of existence seems to be regulated by one gigantic sunshine law—we have no reason to be surprised if the memoirs of public officials are less and less governed by the decorous inhibitions of the past. Nevertheless I for one am still—though probably not for much longer—at least a little surprised at how outspoken public officials are becoming." I would argue that the phenomenon to which Podhoretz here calls attention is itself a testimony to the impact upon American society of a vulgarization of the Freudian ethics of honesty and self-expression.

28. See, for example, *Delusions and Dreams in Jensen's "Gradiva"*: "We remain on the surface so long as we are dealing only with memories and ideas. What is alone of value in mental life is rather the feelings. No mental forces are significant unless they arouse feelings" (Freud, 1907a, chap. 2, pp. 48–49, emphasis added).

29. See Janov (1970, p. 91): "Though the scream is the most usual reaction, it is neither the sole nor the perennial response to sudden vulnerability to Pain. Some people moan, groan, writhe, and thrash about." They do not, it appears, need to speak. See also Janov's (1970, p. 388) distinction between "conventional [word-oriented] psychotherapy," which "has been acceptable to middle-class intellectuals," and "the Primal view."

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REGULATORY ENFORCEMENT IN A FEDERALIST SYSTEM

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*F*ederal agencies integrate federal, state, and local political demands at the operational level of service delivery. They balance conflicting political demands and task requirements as they attempt to develop feasible enforcement routines capable of attracting support and resources in multiple arenas without undermining central support for budgetary resources and statutory authority. Our regression analysis of annual enforcement data from 1976 through 1983 for all 50 states indicates that even the relatively isolated enforcement procedures of the Occupational Safety and Health Administration respond significantly to state-level political and task differences. Enforcement activities responded most consistently to daily enforcement contacts with interest groups. State differences in task conditions—particularly workplace accident and unemployment rates—also elicited instrumental responses, while differences in the party and ideology of elected officials elicited more symbolic actions. State agencies, with their smaller size and greater flexibility, were even more responsive than the federal agency to political and task differences. This integrative function of bureaucracy needs further attention in democratic theory.

*T*wo contrasting images of public bureaucracy have dominated the study of federal agencies, particularly those responsible for regulatory enforcement. One image focuses on central controls over agency structure, tasks, and budgets and emphasizes the importance of elected officials and pressure groups in determining bureaucratic behavior. The other image, more dominant in the field of public administration, focuses on the nature of the policy task and the organizational characteristics of a bureaucracy and notes that organizational imperatives dominate the actions of public bureaucracies. This analysis contributes to recent efforts to integrate and expand these images in order

to understand the dynamic role of federal agencies in a disjointed, incremental, federalist system (Bendor, Taylor, and Van Gaalen, 1985; Chubb, 1985, Moe, 1985a). Although the argument applies more broadly, this research is focused on regulatory enforcement and presents evidence from our study of the enforcement activities of the Occupational Safety and Health Administration (OSHA).

Both images are rooted in the classical distinction between political and administrative questions that lies at the base of democratic theory. Political officials acquire the authority to settle political questions through the electoral process, while bureaucrats are delegated authority to settle administrative questions because of

their technical expertise. Although this distinction has long been discredited, current political science studies still reflect these images largely because of specialization that focuses attention on either electoral or bureaucratic institutions, but seldom on both.

Studies of political institutions note that the president, Congress, and courts not only determine the general tasks and organizational form of regulatory agencies but also control enforcement activities directly through various oversight powers. Even if not regularly used, potential controls are presumed sufficient to make bureaucrats responsive to central political changes (McCubbins, 1985; McCubbins and Schwartz, 1984; Weingast and Moran, 1983). Quantitative empirical studies have provided evidence about bureaucratic responsiveness to a major political realignment in congressional committees, (Weingast and Moran, 1983; but see Moe, 1985b), to the periodic realignments corresponding to different presidential regimes (Moe, 1982), and to more incremental changes in several federal institutions (Chubb, 1985; Moe, 1985a). Party alignments of the president and Congress have been argued to affect regulatory budgets (Kemp, 1985). A few state-level studies have indicated that state bureaucracies are even more responsive than the federal bureaucracy to political influences, particularly from business groups (Marlow, 1981; Marvel, 1982; Williams and Matheny, 1984), and one city-level study found responsiveness to patronage institutions but not to organized interest groups (Jones, 1981).

Studies focusing primarily on implementation by administrative agencies, on the other hand, tend to view elected officials as relatively distant, minor participants in the daily battles within the bureaucracy that determine policy impacts. The specialized technical, legal, and administrative knowledge, the authority to make specific decisions, the

control over important streams of information, the reputation for revenge and reliability, the lack of electoral incentives, and the staying power of bureaucratic participants give administrative agencies considerable resources to thwart specific demands from elected officials (Allison, 1971; Bardach, 1977; Neustadt, 1980). The influence of interest groups over regulatory agencies is more widely acknowledged, particularly because they are considered more persistent and adept at bureaucratic politics than elected officials (Bernstein, 1955; Lowi, 1969; Mitnick, 1980; Noll and Owen, 1983). However, their ability to influence enforcement (as opposed to rulemaking) decisions—particularly in regulatory agencies like OSHA, which processes large numbers of cases through a complex sequence of specialized routines—is limited by case processing requirements (Diver, 1980) as well as by legal restrictions from the Administrative Procedures Act and related court decisions designed to limit the ability of powerful groups to influence individual enforcement actions. Furthermore, in conflict-ridden political environments such as OSHA's (McCaffrey, 1982; Mendeloff, 1979), conflicting pressures from contending groups provide added incentives for enforcement officials to guard the image of impartiality and avoid even the appearance of responding to enforcement requests from individual elected officials.

Thus, although elected officials and interest groups have been shown to influence aggregate national enforcement policies, the extent of their control over the details of enforcement and the variance of enforcement actions across geographic regions are less well established. Studies of bureaucratic output in other contexts, particularly in the delivery of urban services, have found the distribution of output to be determined not by political influence but by routines established by the agency to perform its

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assigned tasks (Jones, Greenburg, Kaufman, and Drew, 1977; Levy, Meltsner, and Wildavsky, 1974; Lineberry, 1977). Similarly, case studies of enforcement agencies have tended to discount political influences over bureaucratic activities, explaining agency behavior in terms of the professionalism of enforcement staffs, established work routines, task requirements for detecting and prosecuting violations, attitudes towards regulated firms, and more idiosyncratic behavior of inspectors (Carson, 1970; Diver, 1980; Hawkins, 1984; Hawkins and Thomas, 1984; Kagan, 1978; Katzman, 1981; Kelman, 1982; Nivola, 1976; Shover, Lynxwiler, and Clelland, 1983; Weaver, 1977).

Explanations of enforcement behavior that emphasize internal factors complement political explanations to the extent that internally induced variations in enforcement output come about in response to different task conditions for statutory tasks. Indeed, responsiveness to statutory objectives requires the observed role of expertise and professional beliefs in establishing work routines (Bardach and Kagan, 1982; Kagan, 1978; Kagan and Scholz, 1984; Kaufman, 1960; Kelman, 1982; Shover et al., 1983) in order to achieve statutory goals. Other regulatory studies reflect a common perception of bureaucracy (Niskanen, 1971) that internal factors reflecting bureaucratic interests, bureaucratic cultures, and staff idiosyncrasies diminish the agency's concern with statutory goals, thereby reducing the likelihood of both task and political responsiveness. (Katzman, 1981; Nivola, 1976; Silbey, 1984; Weaver, 1977). Furthermore, as implementation studies have made clear, political demands from various local, state, and federal institutions are frequently in conflict with statutory goals, and the statutory goals themselves seldom lead to unambiguous definitions of appropriate tasks. The degree to which

federal enforcement is or should be altered to cater to local variation in task conditions or political demands remains ambiguous in descriptive as well as prescriptive theory.

Our study of OSHA probes the degree to which its enforcement activities vary in response to political and task differences among states. OSHA provides an excellent subject because it undertakes or supervises extensive enforcement actions of a relatively similar nature in all states, because its regulations have remained relatively stable during the period of study, and because its inspectors had developed a reputation in its early years of being legalistic and unresponsive to important task factors (Bardach and Kagan, 1982), making it a difficult test case for enforcement responsiveness. Because we are primarily concerned with the responsiveness of established enforcement agencies, we included state-level enforcement data from 1976 to 1983, the period after the problems of OSHA's formative years had diminished. During this period few new laws and organizational tasks were imposed on OSHA, and yet there were differences among states and considerable changes in political and task factors expected to affect enforcement (Centaur Associates, 1985). Before presenting the results of our analysis, we will discuss hypotheses concerning (1) the political factors likely to influence OSHA, (2) the task factors likely to influence OSHA, (3) the instrumental and symbolic enforcement responses available to OSHA, and (4) the comparative responsiveness of federal and state agencies that enforce OSHA regulations.

HYPOTHESIS 1. Agencies respond to changes in multiple political environments.

The primary hypothesis to be tested in this study is that bureaucracies respond rationally to multiple political and task

signals from their environment, altering activities in different time periods and geographic regions according to these signals. Given the complex structure of American politics and the multiple paths of influence over government agencies, however, it is not at all clear which political signals the agency should be expected to follow. A narrow legalistic view of democratic authority would presumably argue that only federal elected officials should influence federal bureaucracies like OSHA, and only through legitimate policy channels. Thus, the party affiliation and ideological inclination of a state's congressional contingent should have an overall impact on aggregate national enforcement levels but not on the particular level of enforcement in the given state. Once the national policy is set, differences in enforcement levels among states should reflect only differences in task conditions. With few exceptions (Chubb, 1985), research has inadvertently supported this narrow view because of the primary focus on national-level enforcement responsiveness.

A pragmatic agency, however, is likely to respond to the more subtle concerns of congressmen for their particular districts by initiating more intensive enforcement efforts in areas and industries where congressmen hope to maintain strong labor support and by developing more cooperative enforcement programs in areas and industries where business backing is important to congressmen. Such variation in enforcement may, in some instances, reflect conscious strategies by agency leadership to gain or maintain support from particular elected officials but may also reflect the local enforcement official's inclination to take advantage of an elected official's willingness to provide local leadership and implementation resources. The first strategy attempts to increase the agency's formal resources (budgets and statutory authority) while the second attempts to increase external resources

provided by outside groups. Of course, the second strategy is aimed not only at elected officials but also at interest groups, whose resources are required to supplement formal resources (Bardach, 1977; Pressman and Wildavsky, 1976; Selznick, 1966), particularly given the chronic insufficiency of the enforcement resources allocated to OSHA and other regulatory agencies to establish a credible enforcement threat (McCaffrey, 1982; Mendeloff, 1979; Smith, 1976). To the extent that required local resources are more problematic than formal resources, the cumulative effect of local electoral and interest group networks would be expected to have a greater impact on national enforcement activities than national political alignments would have. Indeed, federal agencies may, of necessity, seek local alliances required for program success, even at the cost of alienating some congressional support (Selznick, 1966).

Local interest group support can be even more important to enforcement officials than the support of local elected officials because their members can directly help or hinder enforcement. Enforcement agencies routinely meet with affected organizations at various levels to discuss inspection procedures, conflicting interpretations of regulations, and other enforcement problems. In addition, the act establishing OSHA, like most other regulatory enforcement legislation, provides several points at which individuals from affected business and labor organizations directly affect the enforcement process. Labor representatives, for example, can increase the likelihood that violations will be found and penalized by filing complaints with OSHA alleging violations in their workplace and by accompanying OSHA officials during inspections. Although all workers have the right to file complaints anonymously, complaints are most likely to be filed when systematically encouraged by active unions.

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Business, on the other hand, can increase the cost of enforcement actions—thus reducing the level of enforcement activities—in many ways, particularly by appealing citations to the special appellate body, the OSHA Review Commission. As appeals increase, inspectors must spend more time carefully documenting citations and preparing testimony. In addition, appeals boards almost automatically reduce fines and dismiss some citations: for example, during one period in California, penalties were cut by an average of 50%, according to CAL-OSHA officials. Again, appeals by individual businesses are more likely when encouraged by active trade associations.

If agency responses are assumed to be rational, the signals to which OSHA responds provide some clues about the relative importance of formal and informal resources to the agency. To the extent that local resources are more important for enforcement than formal resources, direct actions by interest groups and local election networks should have a measurable impact on enforcement activities. Even state legislators and governors are then likely to have some influence over federal policies. To the extent that central resources are more important, only political forces filtered through Congress, relevant congressional committees, and the president should affect the distribution of enforcement activities, and task conditions should be a primary cause for enforcement variation between states.

Political data for the OSHA study was gathered to provide a preliminary mapping of how responsive enforcement is to federal and state elected officials and to interest groups. To control for and assess the impact of the Ford, Carter, and Reagan administrations on national enforcement levels during the period of the study, we include dummy variables for presidential administrations. To measure the political effect of a state's congress-

sional contingent, we assume that congressmen who vote more frequently for prolabor positions in Congress are also more likely to seek higher levels of enforcement in the district. We therefore calculate a prolabor ideological index for each state contingent based on the mean difference between trade union Committee on Political Education (COPE) (pro-labor voting) and National Association of Manufacturers (NAM) (probusiness voting) scores for all senators and congressmen from the state.

At the state level, ideological indicators like COPE and NAM scores are not consistently available. Given the prolabor nature of OSHA enforcement and the long-standing alliances of business with the Republicans and labor with the Democrats, we assume that Democratic governors and legislators are more likely to seek higher levels of enforcement. Although this simplification does not do justice to the diversity of partisan alignments in state politics, partisanship is used frequently in a similar manner in state policy studies (Cnudde and McCrone, 1969; Dye, 1969; Jennings, 1979; Morehouse, 1981; Sharkansky and Hof-ferbert, 1969). Thus, governors are represented by a dummy variable valued at 0 for Republicans and 1 for Democrats, and state legislatures are represented by the percentage of Democrats elected to the combined legislative bodies in the state.

For interest groups, we expect the level of enforcement activities to vary directly with the support provided by labor and inversely with the resistance offered by business. We include the number of worker complaints per 1,000 industrial workers filed with OSHA as the best available measure of the direct activity of labor to influence enforcement but unfortunately could not use the business equivalent—the number of citations appealed by firms—because of missing data.¹

In summary, we expect that OSHA en-

forcement will be most vigorous during periods of and in states with high levels of labor complaints and more Democratic and prolabor elected officials than pro-business and Republican ones, although we are uncertain whether responsiveness will be more significant for legislative or executive officials at the state or federal level.

HYPOTHESIS 2. Agencies respond to changes in task environments.

Economic models of enforcement note that enforcement efficiency requires the shifting of resources among enforcement arenas with different task characteristics until marginal returns in each area are equal (Stigler, 1970). Thus, in order to obtain maximum political credit for implementing the formal tasks assigned by central political authorities with a given budget, an agency would vary its enforcement activities over time in different states to reflect differences in task-related conditions affecting the return on enforcement activities.

However, organizational and political problems limit an agency's ability to adapt to changes in task-related factors. Implementation research has emphasized the difficulty agencies face in assembling and coordinating effective routines to accomplish any task (Bardach, 1977), even for modest policy goals in relatively tranquil political environments (Pressman and Wildavsky, 1976). Once effective routines have been established, organizational rationality requires that they be standardized, that variation be minimized, and that core technologies (in OSHA, for producing and processing citations) be buffered from environmental fluctuations (Thompson, 1967), all of which reduce the variation to be expected in enforcement due to task differences. Thus, task responsiveness becomes diluted by organizational necessities.

Conflicting political signals further reduce an agency's potential task respon-

siveness. First, the centrifugal force of local political pressures and the bureaucratic interests of local operating units pose a problem that federal agencies frequently solve through standardizing procedures and output beyond the requirements for organizational efficiency (Kaufman, 1960). Professional standards, acceptable measures of task output and of task-related differences in local conditions, and agreement on the causal relationships between agency activities and outcomes (e.g., between OSHA's enforcement efforts and the reduction of risk) generally aid agencies in preserving the discretion of local operating units to vary output in response to local conditions. In compact urban agencies delivering standard services like fire protection and road maintenance, for example, technical decision routines have been found to determine the difference in level of service among an agency's service areas. Political differences between areas had less of an effect for these professionalized services (Levy et al., 1974) but a greater effect on less-professionalized urban bureaucracies; bureaucratic decision rules had less of an effect on less-professionalized urban bureaucracies (Jones, 1981; Jones et al., 1977). Because federal agencies like OSHA generally have less-developed professionalism, less-established technologies, greater organizational problems imposed by broader mandates and jurisdictions, and more complex political environments than urban agencies, task responsiveness becomes more questionable. For OSHA in particular, the contentiousness of the political environment and the difficulty in establishing appropriate task-related justifications for different levels of enforcement decrease the likelihood of task-related responsiveness.

Two testable decision rules about the efficient use of scarce resources emerged from interviews with enforcement officials as potential indicators of task responsiveness for OSHA. First, we expect that enforcement activities will be

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highest where industrial accident rates are highest because enforcement professionals tend to believe that enforcement resources are most effectively deployed where high accident rates indicate that safety problems are more numerous. Accident rates affect inspectors, who look at past accident records of firms during inspections and who tend to look harder, to cite more of the detected violations, and to assess maximum penalties in firms with higher accident rates. Similarly, OSHA managers are aware of aggregate accident figures and know that they will be criticized when rates go up. As a measure of accident rates, annual safety and health incident rates were aggregated at the state level from annual Bureau of Labor Statistics survey data for the years 1974–1983.²

Second, we expect that enforcement should be sensitive to general economic conditions, despite the strict statutory language requiring inspectors to cite any observed violation regardless of economic conditions. In informal interviews, OSHA enforcement officials at all levels expressed concern about contributing to unemployment when times are bad and seemed inclined to give firms more leeway when the business cycle was down. They are more willing to be aggressive when business is good and employment is less a problem because firms have greater resources to respond and workers are more likely to support the efforts of OSHA inspectors. Furthermore, OSHA is less politically vulnerable to business complaints during good times than during bad times. Annual state data on unemployment and per capita income were collected to test the effect of economic conditions on enforcement actions. We expect enforcement to be inversely related to unemployment and positively related to income, although the use of income as a proxy for economic conditions is complicated because it relates to a range of other economic factors with unknown relationships to enforcement.

It is possible that task variables such as accident rates and economic conditions will affect enforcement indirectly by increasing political demands for enforcement (Moe, 1985a). Because they include political variables to control at least partially for changes in political demand, our estimates for task variables primarily reflect the direct task responsiveness of OSHA. Also, because we are concerned with changes in enforcement over time as well as between states, we recognize that even after changes in the agency's environment are detected, organizational adjustments require time to plan, implement, and institutionalize. Since only annual data was available, our model assumes a one-year lag before the full effects of political and task factors on enforcement activities are felt.

HYPOTHESIS 3. Agencies tend to respond instrumentally to task changes and symbolically to political changes.

Agencies can respond to political and task influences with activities that vary in their costs and their perceived likelihood of producing desired effects. Given the number of external factors affecting the industrial accident rates OSHA is charged with moderating (Mendeloff, 1979; Smith, 1976), political oversight of OSHA—as of most agencies—is based on measures of enforcement activities rather than on outcomes (e.g., accident rates). Because the relative effectiveness of different activities is not well established, particularly for those not actively involved in the process, bureaucrats may have considerable discretion in deciding which activity to increase to satisfy increased political demand. The hypothesis to be tested here is that bureaucrats will respond to political demands by changing lower-cost, “symbolic” output that may help generate the desired political support, even if it may have no effect on accidents, but will respond to task factors with “instrumental” output that agency

professionals consider to be more likely to affect outcomes, even though it is also more costly. In organizational terms, responsive bureaucrats cope with the political environment by buffering critical enforcement activities and with the task environment by monitoring task changes and routinely altering critical activities to adapt to changes.

Studies assessing the effectiveness of OSHA enforcement have focused primarily on three activities for which data are readily available: inspections, citations for violations, and assessed penalties (Jones, 1984; McCaffrey, 1983; Viscusi, 1979). In these studies, the number of inspections is generally used to represent the probability of finding a violation, while the number of citations and the amount of assessed penalties reflect the extent of punishment imposed for violating the law. Together, they are assumed to determine the added incentive to comply with the safety and health standards produced by OSHA's enforcement effort. Empirical findings about the effectiveness of these activities in reducing accident rates are mixed, but the administrative nature of each activity suggests considerable differences in the instrumental value of each in reducing the risk of workplace accidents (Centaur Associates, 1985).

The number of inspections is seldom found to affect accident rates, perhaps because it can be increased simply by visiting more places for a shorter period, which increases the visibility of enforcement efforts but may or may not increase the likelihood of finding violations. The number of citations can also be changed with relatively low cost by citing relatively minor violations that exist in most workplaces because a firm is unlikely to expend resources appealing minor citations with small monetary penalties. Inspections and nonserious citations, then, provide a relatively inexpensive symbolic response that at least makes enforcement efforts more visible to OSHA's labor clients.

On the other hand, an increase in the number of citations for serious violations—those imposing imminent danger of injury—is more likely to provoke resistance from cited firms because serious citations impose higher fines, increase the likelihood of future inspections, and enhance the threat of liability if injuries do occur. For the same reasons, serious citations also impose the greatest incentive to correct the kinds of violations most likely to cause accidents and, therefore, are generally viewed by OSHA officials as an important instrumental response mechanism. Similarly, increases in serious and even nonserious penalties are more likely to impose greater costs on OSHA through increased resistance by firms but are considered effective instrumental responses to increases in accident risks.

Consequently, we expect that political responsiveness will be most pronounced for inspections and nonserious citations, while task responsiveness will be more evident for serious citations and penalties. To test this hypothesis, state-level enforcement data on inspections, citations, and penalties were collected from annual OSHA enforcement reports, with serious and nonserious citations and penalties recorded separately.³ To control for variance due to the difference in the size of states, all figures were standardized per thousand manufacturing workers because OSHA enforcement focuses primarily on these workers.

HYPOTHESIS 4. State agencies are more responsive than federal agencies.

OSHA provides a unique opportunity to compare the responsiveness of state and federal bureaucracies performing the same task because authorized state agencies are allowed to enforce the federal law if OSHA certifies that state programs are at least as effective as the federal counterpart. Twenty-two states chose this option for most of the period under study. The summary data for political and task

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Table 1. Comparison of State Level Political and Task Factors for State and Federal Programs, 1974-1982

Factors	Mean and Standard Deviation for				Difference in Mean as Percent of Federal Program Means
	State Programs		Federal Program		
	\bar{X}	sd	\bar{X}	sd	
Political Factors					
Labor (Complaints per 1000 workers)	3.06	3.9	1.81	1.2	69
Congress (Mean COPE-NAB scores)	-7.68	45	-6.52	39	-18
Governor (Democrat = 1, Republican = 0)	0.60	.50	0.71	.46	-14
State legislature (% Democrats)	63.83	16	65.10	21	-2
Task Factors					
Accidents (per 1,000 workers)	9.48	1.6	8.93	1.1	6
Income (per capita, 1972 dollars)	4,794.80	780	4,543.40	580	6
Unemployment (% workforce)	7.18	2.2	6.90	2.1	4
Standardization Factor					
Workforce in manufacturing (in 1,000s)	361.69	420	422.60	430	-14

Sources:

Governor: *Congressional Quarterly Weekly Report*, November issues for election years, 1974-1982.

State legislature: *The Book of the States*, from 1976-77 to 1982-83.

Congress: *The Almanac of American Politics*, 1978, 1980, 1982, 1984.

Accidents: *State Data on Occupational Injuries and Illnesses*, Bureau of Labor Statistics, U.S. Department of Labor, 1982 and 1983.

Unemployment, income, workforce in manufacturing: *Statistical Abstract of the United States*, Bureau of Labor Statistics, U.S. Department of Labor, 1977, 1979, 1981, 1983.

variables presented in Table 1 indicate that the labor force in state-program states is considerably more involved in OSHA enforcement, yet the states tend to be somewhat more Republican. They are smaller and slightly richer, with somewhat higher unemployment and accident rates. The differences in political and task factors are minor compared with the differences in standardized enforcement measures presented in Table 2, where states with state programs consistently score higher than states with federal programs.

Why some states chose to pay approx-

imately 50% of the costs for the right to enforce OSHA regulations with state agencies is not clear from this data. Most analysts agree that states assume regulatory responsibility with the expectation that the state bureaucracy would be more responsive to state conditions, presumably to business interests assumed to dominate at the state level (Marvel, 1982; Williams and Matheny, 1984). However, greater enforcement action in states with state programs indicates that OSHA is effective in setting legally required minimum enforcement standards that business interests would be little in-

Table 2. Comparison of Selected State-Level Enforcement Data for State and Federal Programs, 1975-1983

Fiscal Year	Nonserious Violations		Serious Violations		Total Penalties (\$)	
	State	Federal	State	Federal	State	Federal
1976	103.2	37.2	1.5	1.2	1,985.5	1,027.6
1977	88.1	20.1	2.6	2.6	1,246.2	863.8
1978	78.9	11.6	6.4	3.7	2,245.5	1,156.1
1979	68.7	8.7	6.3	3.6	2,102.1	1,204.1
1980	64.2	9.1	6.8	4.3	1,944.6	1,559.0
1981	64.8	9.2	7.0	4.3	1,980.0	1,577.6
1982	60.1	7.1	6.1	2.4	1,317.3	519.8
1983	60.4	8.4	7.2	2.8	1,160.3	686.3

Source: Data provided by the Occupational Safety and Health Administration from their computerized data base for federal states and from annual reports for state-program states.

Note: Violations and penalties are standardized for interstate variations by calculating them per 1,000 workers employed in manufacturing in the states.

terested in exceeding. Thompson and Scicchitano's (1985) analysis of the OSHA selection process is probably correct in concluding that states with the greatest geographic distance and perceived differences from Washington were most likely to opt for their own programs. In any case, there is no clear bias in the state program selection process that would preclude using observed differences to compare the responsiveness of state and federal agencies.

Structural differences between the state and federal programs support the assumption that state bureaucracies should be more responsive to political demands, particularly demands from state institutions (Kaufman, 1960; Marlow, 1981; Marvel, 1982; Scholz, 1982). State bureaucracies are small enough to be able to rely more on informal, personal supervision than can federal bureaucracies, which require more formal procedures, standards, and reports to ensure uniformity of actions in geographically dispersed areas. State bureaucracies generally have fewer levels of hierarchy between field personnel and political appointees, who are more sensitive to the political environment than civil service profes-

sionals. Even state civil servants are more likely to identify career opportunities with the state and, therefore, to be more sensitive to state political interests.

In addition, there are more elected state officials per state than elected federal representatives, and their access to state bureaucracies is likely to be greater than to the federal bureaucracy. Indeed, one of the purposes of standardized procedures and placement rotation in the federal bureaucracy is to reduce political responsiveness to the "parochial" interests of local elected officials. Furthermore, the federal program does not even have a coordinating office below the regional level as a focus for state-level political demands.

The expected difference in task responsiveness between the two programs is less clear, particularly because the difference in civil service systems and professionalism is not well known. Some argue that the characteristics that increase the political penetrability of state bureaucracies also decrease professional concerns with statutory goals (Marlow, 1981; Marvel, 1982). Professional prestige, salaries, civil service protection, training, and career opportunities are generally

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assumed to be better in federal than in state enforcement programs. However, complaints of legalistic, "nit-picking" enforcement by federal OSHA, particularly in its early years, indicate that the federal agency was dominated by inflexible, standardized decision routines that provided little room for professional task responsiveness (Bardach and Kagan, 1982; Smith, 1976). To test whether state and federal programs differed significantly in political and task responsiveness for our data set, a standard dummy variable analysis was used to estimate the difference in slopes and intercepts of the two different groups of states.

Method of Analysis

Annual data for the variables discussed in the previous sections and listed in Tables 1 and 2 were gathered for all states for the period 1976 to 1983, providing a total of 400 observations. The regression model estimates the impact of the lagged political and task variables on each of the enforcement variables:

$$\begin{aligned} \text{Enforcement}_{i,t} = & C \\ & + \text{Political Factors}_{i,t-1} \\ & + \text{Task Factors}_{i,t-1}, \end{aligned} \quad (1)$$

where i represents the states and t the fiscal year. Separate slope and intercept terms for state and federal programs were estimated by using a standard dummy variable analysis. The problems associated with analyses that pool time series from multiple state units require special estimation techniques (Stimson, 1985). Since estimations based on pooled state data are well suited to studying bureaucratic output and policy impacts, but underused, details about the specific form of the estimated equation and the Parks method of estimation are included in the Appendix. By combining cross-sectional and time-series effects, it should be noted

we are making the simplifying assumption that spacial and temporal relationships are similar enough to be combined; for example, a one-unit difference in accident rates is assumed to have the same effect on enforcement whether it represents the difference between two states or between two time periods for the same state.

Table 3 presents the estimated coefficients for the six categories of enforcement activities that were studied, with each column representing an independent estimation for each activity. Results for state and federal programs are presented separately, except that separate estimates could not be calculated for variables representing presidential administrations and governors.⁴ The starred estimates in bold print indicate that the factor labeled on the left had a statistically significant impact on the enforcement activity, and underscoring indicates that the estimated impacts on state and federal programs are significantly different from each other.

The results presented in Table 3 appear to be quite robust.⁵ Although the following discussion of findings will focus only on statistically significant results, coefficients generally had the expected signs, even for estimates insignificantly different from 0. The unadjusted R^2 of the equations ranged from .4 to .6, indicating that about half of the variance in enforcement was accounted for despite the lack of several theoretically interesting measures expected to influence enforcement.

Discussion of Results

Agency Responsiveness to Multiple Political Signals

Two conclusions about political responsiveness stand out clearly from the analysis. First, OSHA exhibited a remarkable responsiveness to all political factors included in Table 2. Each political factor significantly affected at least two enforcement activities, and overall, 27 of the 54

possible coefficients were significant with the expected sign. The four unexpected negative signs for nonpresidential factors will be discussed below.

Second, the direct interest group measure for labor clearly exerted the most

consistent influence on enforcement activities, significantly affecting every activity. The magnitude of labor's impact on enforcement during the period is more difficult to compare with other variables because the variables have no common

Table 3. Enforcement Activity Response to Political and Task Factors

Factors	Inspections	Citations		Penalties		Total
		Nonserious	Serious	Nonserious	Serious	
Political Factors						
Carter (FYs 1978-81)	1.09	2.49*	1.40*	-50.17*	70.58	-49.63
Reagan (FYs 1982-83)	5.22	-2.43	.87	-129.41*	-283.67	-195.58*
Governor	1.98*	10.30*	.26	95.42*	35.13	116.69*
Congress						
State	.04	.01	.01	1.06*	-.39	1.45*
Federal	.02	.17*	.00	.98*	1.01	2.18*
State legislature						
State	.32*	.58*	-.02	1.14	-1.92	-1.95*
Federal	.03*	-.28*	-.03	-3.11	-5.46	-2.18
Labor						
State	5.23*	7.46*	1.01*	68.38*	161.24*	207.23*
Federal	1.01*	-3.62*	.53*	-22.35*	124.13*	187.36*
Task Factors						
Accidents						
State	.23	.63	-.06	5.90*	10.68*	7.55*
Federal	-.00	-.28	.01	-4.51*	5.59*	.62*
Unemployment						
State	-.02	.19	-.01*	3.90*	-1.98*	-3.89*
Federal	-.06	-.07	-.01*	.42	-1.47*	-.39
Income						
State	.00	.03*	.00	-.00	.02	.15
Federal	-.00	-.01*	-.00	-.15	-.19	-.06
Intercept Terms						
State program	-42.35*	-197.98*	6.63	-769.00	303.87	-436.82
Federal program	34.43*	135.20	11.81	1317.13	1858.47	595.46
R ² (uncorrected)	.64	.62	.45	.43	.50	.54

Note: The data used for this analysis are the same as those used for Table 2 and come from the same sources. The figures are estimated regression coefficients from the pooled data for all 50 states from 1976 through 1983, calculated by the Parks method for the equation presented in the Appendix. Federal coefficients are calculated to be comparable with the state coefficients, using the federal dummy variable. Where starred estimates are shown in bold print, the factor labeled on the left had a statistically significant impact on enforcement activity at the .05 level; the underscored estimates indicate that the estimated impacts on state and federal programs are significantly different from each other.

*Statistically significant at the .05 level (t test), with 383 degrees of freedom for t statistics. Underscoring indicates that the difference between the state and federal programs is statistically significant at the .05 level.

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scale of measurement and because inferences about relative impacts based on regression coefficients are statistically problematic due to multicollinearity and measurement error problems (Moe, 1985, p. 1109). Since the relative impact of each variable depends on its variance during the period as well as the responsiveness coefficient, we might ask what impact each variable is estimated to have when it changes an amount equivalent to one standard deviation (reported in Table 1). For example, the impact on total penalties in the state program of an increase equivalent to the standard deviation would amount to \$816 for labor complaints, compared with \$65 for Congress, -\$31 for state legislatures, \$12 for accident rates, and -\$8 for unemployment. This effect is even greater than the -\$195 change representing the entire effect of the Reagan administration. Using this crude test, labor's impact on all enforcement variables is considerably greater than that of other variables, although we cannot claim from this test that the difference is of statistical significance. The consistency and magnitude of the effect of labor on all enforcement actions underscores the importance of direct enforcement resources provided by interest groups. We suspect that a measure of direct business actions would similarly be quite important in shaping enforcement actions.

Presidential Administration. The results confirm the importance of presidential influence over administrative actions (Chubb, 1985; Moe, 1982, 1985a). Both the Carter and Reagan administrations were primarily concerned with reducing OSHA's early reputation as a "nit-picking" agency that repeatedly cited insignificant violations while ignoring more serious ones. In response, OSHA significantly reduced the penalties associated with nonserious violations in the years following the Ford administration, although, surprisingly, the number

of nonserious citations was not significantly reduced in response to Reagan and actually increased under Carter. The Carter administration, reflecting the traditional democratic affiliation with labor interests, shifted penalties from nonserious to more serious safety-related violations, resulting in no significant decrease in total penalties. The Reagan administration, reflecting the traditional Republican concern with business interests, reduced penalties for nonserious citations by even greater amounts without a corresponding increase in penalties for serious citations, leading to a significant decline in total penalties as well.

Although the separate effect of presidential administration on state and federal programs was not estimated because of technical difficulties, the enforcement data in Table 2 suggest that presidential administrations may be able to increase OSHA activities in state-program states (as Carter did) more readily than they can decrease them (as Reagan attempted). The drop in serious violations and in total penalties during the Reagan years (the 1982 and 1983 fiscal years) was much more dramatic for the federal program than for the state programs, while the initial rise in serious violations under Carter (fiscal year 1978) was more dramatic for state programs. This result supports the importance of the statutory provision that provides that federal OSHA sets minimum but not maximum enforcement levels for state programs.

Congress. A prolabor congressional delegation increased nonserious and total penalties in both state- and federal-program states but increased nonserious citations only in federal-program states. Given the lack of direct congressional authority over the state programs, it is surprising that only nonserious citations exhibited significantly greater responsiveness for the federal program compared with state programs (coefficients

are underlined in Table 3 when differences between state and federal programs are statistically significant). The similar effect on both state and federal penalty activities suggests that the resources provided by local electoral networks are more important to agencies than formal resources controlled by Congress.

Although studies of the Federal Trade Commission (Weingast and Moran, 1983) and the National Labor Relations Board (Moe, 1985a) found that congressional committee membership had a significant effect on aggregate national enforcement actions, no significant influence was found on state-level OSHA enforcement activities. The measures of party identification of OSHA's oversight committee members (1 for each Democrat, -1 for each Republican, and 0 for states with no members) for Senate, House, and combined committee membership were insignificant in all equations and, therefore, dropped from the final estimates presented in Table 3. The relative importance of the state's delegation as a whole compared with committee membership again indicates that for OSHA, district action may be a more potent congressional mechanism for influence than formal committee oversight (but see Chubb, 1985). It should be noted that OSHA officials firmly deny any informal influence of elected officials at the local level, but the pervasive influence of local interest group actions combined with the likely influences of local electoral networks indicate the need for further studies of the complex, subtle interactions between political and bureaucratic institutions at the local level.

State Governors and Legislators. As expected, states with Democratic governors had significantly more enforcement actions in almost every category, presumably because of the governor's proximity to administrative channels, both federal and state. As noted earlier, coefficients for state and federal programs were

not estimated separately, eliminating another test of the relative importance of informal networks and local resources versus formal authority. The results for the state legislators provide some evidence that both mechanisms operate, since a higher percentage of Democrats in state legislatures significantly increased inspections in federal-program states where the legislature has no formal authority but increased both inspections and nonserious citations in state-program states where they do have formal authority. Legislative influences were less pervasive and consistent than gubernatorial influences because the large number of legislators diffuses that source of influence. The unexpected negative influence on federal program nonserious citations might involve the kind of expected trade-off discussed below for labor, but the negative influence on state program total penalties remains puzzling, perhaps reflecting the inadequacy of our simple assumptions about party coalitions in state legislatures.⁶

Labor. As noted, direct labor interaction through the filing of complaints had the most consistent effect on all enforcement output. State programs were significantly responsive for all output, and the federal program showed positive responsiveness for everything but nonserious citations and penalties. These negative relationships must be understood in conjunction with the positive relationship for serious citations and penalties because higher levels of labor complaints appear to induce a shift in resources from nonserious citations to serious ones with considerably higher penalties (thus the higher total penalties despite the decline in nonserious penalties). This tradeoff in the federal program may reflect successful attempts by management to improve the productivity of OSHA's complaint responses, which in the early 1970s were considered by many to have unduly dominated the determination of inspec-

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tion targets in areas where unions were most active. In the state programs, no such tradeoff takes place, suggesting that sufficient enforcement resources are available to attend to both serious and nonserious violations.

Agency Responsiveness to Task Signals

The response to task signals was significant but less consistent than the response to political signals, with significant coefficients in the expected direction occurring in only 11 of 36 possible relationships, or about 30%, compared with 50% of the possible relationships for the political factors. On the other hand, if we limit our attention to the instrumentally most effective enforcement activities, serious citations and serious penalties, both accident and unemployment rates appear, along with labor, to be the most important influences on enforcement. Once labor is excluded, the political factors representing electoral institutions show 13 significant coefficients affecting inspection, nonserious citations, and nonserious penalties—the more symbolic responses discussed earlier—compared with only one significant coefficient (the Carter Administration) for serious citations and penalties. These findings support the argument that symbolic responses are reserved for changes in elected officials, while instrumental responses in key enforcement activities are reserved for task environment changes relevant to formal statutory objectives. The impressively robust effect of labor on all activities indicates that consistent, day to day contacts between interest groups and the enforcement bureaucracy bring the most consistent responses, both symbolic and instrumental.

Higher accident rates, the task variable most directly related to OSHA's statutory goals, brought significant increase in all penalties except for non-

serious violations in the federal program, where the same tradeoff discussed previously for labor appears to decrease penalties for nonserious violations as attention is focused on serious violations. Higher unemployment, the most relevant measure of business cycle activity, led to the expected reduction in serious citations and serious penalties. Here the tradeoff between serious and nonserious penalties is evident even for the state program, where a greater number of serious violations apparently get reclassified and penalized as nonserious when higher unemployment signals an adverse business climate, leading to the negative coefficient for serious penalties and the positive coefficient for nonserious penalties. Higher per capita income affects only nonserious citations and has an unexpected negative effect in federal programs. The poor performance of this indicator probably reflects confounding relationships between per capita income and government budgets as well as industrial development and related business/union political strengths.

Responsiveness of OSHA State Agencies and the Federal Agency to Political and Task Signals

The results in Table 3 provide strong evidence that, for OSHA enforcement, state bureaucracies are considerably more responsive than the federal bureaucracy to all task and political factors, with the expected exception of Congress. Significant differences in the slope estimates for state and federal programs were found in the 15 relationships with underlined coefficients in Table 3, and the state coefficient unambiguously indicated higher responsiveness in the expected direction in all but three cases. Two of these cases involve the aforementioned tradeoff for nonserious citations and penalties related to unemployment; the state program slope is in any case greater than that of the

federal program for the instrumentally important serious and total penalties. The other case relates to congressional influence, where the main surprise is that the federal agency is not significantly more responsive for all enforcement activities. The comparison of the 20 cases in which at least one program has significant estimates (indicated by an asterisk in Table 3) supports the same general conclusion because state program coefficients are greater in the expected direction in all but four comparisons, three involving Congress and unemployment as already explained and one involving the state legislature.

It is interesting to note that with all variables set to zero, the state program intercept term indicates a lower level of enforcement than is indicated for the federal program. Thus, the greater responsiveness of state agencies to political and task demands apparently produces the observed higher mean level of enforcement by state agencies indicated in Table 2. In sum, bureaucrats in both programs respond to different levels of political and task conditions, but state program bureaucrats respond with greater changes in output and with fewer constrained tradeoffs between serious and nonserious violations.

Conclusion

The image of public bureaucracy reflected in this study is that of an organization that responds rationally to political demands but does so in a complex, federalist environment in which statutory commands and oversight by central institutions provide only one set of conflicting signals. The role of federal agencies in the American policy process is not simply one of translating central political decisions into organizationally efficient routines because central institutions seldom can provide the bureaucracy with sufficient resources for such implementation. In-

stead, the creative role of the bureaucracy requires the development of organizationally feasible tasks that will gain and maintain sufficient support from critical actors in multiple operational arenas without undermining central support needed for formal budgets and statutory adjustments.

Thus, bureaucracy plays the important and seldom-recognized role of integrating political demands made at various levels of the American federalist system by incrementally adapting central policies to fit into varied and changing local conditions. The agency's organizational imperative to develop stable routines that provide it a secure existence in its multiple task environments force it to balance the administrative need for uniformity of tasks with the demands for diversity of services in different environments. As agencies adjust and modify routines in response to pressures from groups with formal and informal resources, the political system "learns" which policy interventions work and what they can do. Changes in central policies and oversight by central institutions provide only one input into this process, one that the agency itself helps shape through its strategic responses.

Understanding the responsiveness of bureaucracies to the range of political and task factors in their environment is an important step in understanding the role of federal agencies in the policy process. This study of OSHA indicates that even the relatively isolated enforcement routines for standardized national regulations respond consistently to daily interactions with interest groups involved in the enforcement process. Professional reactions to statutory concerns, represented in the study by accident and unemployment rates, also elicited strong instrumental responses, while responses to elected officials tended to emphasize more symbolic actions. The smaller size and greater flexibility of state bureaucracies allowed greater responsiveness than

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was observed in the federal bureaucracy.

These results raise three research questions of considerable importance for understanding the role of bureaucracy in democratic theory and for the design of policy institutions. First, we need to know more about the patterns of bureaucratic responsiveness. For example, do legislators and interest groups exert their influence on the state bureaucracy as a whole, or would further disaggregation of enforcement to functional categories (industries) or electoral districts indicate an even more localized pattern of response to political pressures? Do contiguous political and administrative boundaries increase political responsiveness at the cost of task responsiveness?

Second, we need to know more about specific adaptive mechanisms through which these changes in political and task conditions affect incentives, professional norms, and standard procedures in enforcement bureaucracies. Case studies of the enforcement process have not analyzed the kinds of specific adaptive mechanisms that would explain the results of this analysis, perhaps because personalities, idiosyncrasies, and the standard procedures that dominate the daily world of enforcement agencies tend to overwhelm the systematic political influences that appear more clearly in quantitative analyses such as this one. Furthermore, the pejorative connotation of patronage, the illegality of political influence on individual enforcement procedures, and the positive value of professional, autonomous enforcers all lead bureaucrats to respond defensively to questions of political influence, creating research problems similar to those encountered in trying to analyze the mechanisms by which lobbyists influence legislators (Manley, 1970). More focused studies are needed to isolate and catalogue the complex and subtle mechanisms that link political and bureaucratic institutions on the local and central levels.

Finally, analyses of central political controls over bureaucracy need to take account of the integrative function of agencies in our federal system. At least part of the difficulty between agencies and central institutions represents the ongoing conflict between central and local political leaders over incremental adjustments of government activities to changing conditions in different communities. Just as changes in electoral laws alter the responsiveness of elected officials to different groups, so do changes in the organization, budgets, and central supervision of agencies alter their responsiveness to different elected officials and interest groups. The great advantage and the major difficulty in the development of a normative theory of bureaucratic enfranchisement is that it must simultaneously consider the problems of democratic representation in a federalist system and the pragmatic requirements of adaptive, productive service delivery organizations.

Consider, for example, the problem of designing regulatory policies such as OSHA. Responsive enforcement of uniform federal rules provides one potentially useful compromise between the inevitable inefficiencies associated with imposing uniform rules on state units differing in their policy demands and task conditions (Advisory Commission, 1984; Bardach and Kagan, 1982) and the "balkanization" problems and excessive technical costs associated with state-level regulation (Scholz, 1982). The appropriate level of responsiveness, however, must certainly depend on the implementation resources of contending groups in different arenas as well as on the kinds of costs associated with excess uniformity or variability for the particular policy issue. Furthermore, appropriate responsiveness mechanisms must balance the need to control bureaucratic self-interest, to promote pragmatic compromises among competing groups, and to minimize disruptive

alterations in productive routines.

The answers to these critical problems require an integrated perspective concerned with both political institutions and public bureaucracies. Formal models analyzing political-bureaucratic interaction at the central level (Bendor, et al, 1985; McCubbins, 1985; Miller and Moe, 1984) may provide a useful starting point for this development. However, analyses of agency accountability to national institutions need to include questions about agency adaptability to multiple legitimate pressures in our federalist system. The rich redundancy of political demands in the federal system and the flexibility of bureaucratic responses, even when enforcing standardized laws, provides an as yet poorly understood component of our disjointed incremental political system.

Appendix

There are several models for estimating regression equations that are appropriate for different kinds of pooled time-series and cross-sectional data sets (Stimson, 1985). Because state-level aggregate data are taken from contiguous geographic units (states) defined by arbitrarily drawn boundaries, both heteroscedasticity and mutual correlation problems between cross-sectional observations are likely to exist (Berk, Hoffman, Maki, Rauma, and Wong, 1979, p. 402; Kmenta, 1971, p. 512). For the serial observations, we suspect that stochastic forces affecting individual state enforcement in any given time period affect the enforcement in proximate time periods as well. However, because most longitudinal regulatory studies suggest that with yearly data, higher order autocorrelations decline rapidly and tend to be negligible (e.g., Berk et al., 1979; Moe, 1985a) and because our data consist of a short time series of eight annual observations, we believe that a first-order autoregressive assumption for the data is appropriate.

Combining these assumptions, we require an estimation model that allows heteroscedasticity and mutual correlation to exist between cross-sectional units and first-order autoregression to exist within the individual unit.

Parks (1967) has developed a consistent and asymptotically efficient three-step estimation technique for such a model that is contained in the commonly available SAS statistical package. Very briefly, the first step of the Parks estimation method uses ordinary least squares (OLS) to estimate the parameters of the autoregressive model; the second step applies OLS to the transformed model (in which autocorrelations have been corrected) to estimate the contemporaneous (mutually correlated) covariances; and the final step uses the estimated covariances to get the regression coefficients. The Parks method takes account of three possible error terms: (1) that associated with a first-order, autoregressive error structure; (2) that associated with heteroscedasticity; and (3) that associated with mutual correlation between cross sections. Therefore, it is expected to produce more efficient regression estimates than an OLS method.

In order to examine simultaneously the effect of both state and federal programs on OSHA enforcement, we employ the dummy-variable technique to test for differences in both the initial level of enforcement activity and bureaucratic responsiveness between the two groups within one overall model. The test for the program differences will be carried out by testing for intercept dummy and slope dummy variables representing the differential effects between the two programs. The exact model to be tested is therefore

$$E_{i,t} = c + b_0F + b_{1h}P_{ih,t-1} + b_{2k}T_{ik,t-1} + b_{3h}F*P_{ih,t-1} + b_{4k}F*T_{ik,t-1} + u_{i,t}, \quad (2)$$

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where

$$i = 1, 2, \dots, N;$$

$$t = 1, 2, \dots, T;$$

$E_{i,t}$ represents the individual enforcement measure for state i during period t ;

F is the intercept dummy variable that equals 1 for federal program states and 0 for state program states;

$P_{ih,t-1}$ is the lagged measure of political variable h for state i ;

$T_{ik,t-1}$ is the lagged measure of task variable k for state i ;

$F^*P_{ih,t-1}$ and $F^*T_{ik,t-1}$ represent interactive variables of federal programs and groups of lagged political and task variables;

c and the subscripted b terms are the regression coefficients to be estimated; and

$u_{i,t}$ is the stochastic disturbance that is assumed in the Parks model to have the following properties:

$$E(u_{it}^2) s_{ii} \quad (\text{heteroscedasticity}),$$

$$E(u_{it}u_{jt}) = s_{ij} \quad (\text{mutual correlation}),$$

$$u_{it} = \rho_i u_{i,t-1} + e_{it} \quad (\text{autoregression}),$$

where

$$e_{it} \sim N(0, V_{ii}),$$

$$E(u_{i,t-1}e_{jt}) = 0,$$

$$E(e_{it}e_{jt}) = V_{ij},$$

$$E(e_{it}e_{js}) = 0 \quad (t \neq s),$$

and

$$i, j = 1, 2, \dots, N.$$

Note that in this model the value of the parameter ρ (first-order autocorrelation) is allowed to vary from one cross-sectional unit (state) to another and that the variances of e are denoted by the symbol V . Also, the initial value of u is assumed to have the following properties:

$$u_{i0} \sim N(0, V_{ii}/1 - \rho_i^2),$$

$$E(u_{i0}u_{j0}) = V_{ij}/1 - \rho_i\rho_j.$$

Further information can be found in Kmenta (1971, pp. 513-14).

In the model, the constant c represents the initial level of state program enforcement. The estimated coefficient of intercept dummy variable F , b_0 indicates the difference in the initial enforcement level between federal and state programs. The estimates of b_{1h} and b_{2k} measure the effects of individual political and task variables on enforcement activities of state programs. Finally, the estimated coefficients of interactive variables— b_{3h} and b_{4k} —measure the direction and significance of the difference in enforcement responsiveness between the federal and state program. The estimates of responsiveness for the federal program, as presented in Table 3, are then the sum of b_{1h} and b_{3h} for political variable h and of b_{2k} and b_{4k} for task variable k .

Notes

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1. We recognize that the number of complaints provides only a crude indicator of the complex range of factors influencing enforcement interactions among business, labor, and OSHA, particularly because labor at times uses complaints to harass business during contract negotiations and because OSHA responds only minimally to such negotiation-related complaints. Since the direction of bias from these influences should make it more difficult to find significant instances of labor influences, they should not jeopardize conclusions when significant relationships are found.

We also attempted to test whether bureaucrats were sensitive to the relative electoral strength of political groups or only to electoral pressures as filtered by elected representatives but found no satisfactory measure of business and labor electoral strength. Campaign contributions, the percentage of the labor force unionized, and the percentage of income produced in manufacturing and construction (the two business classifications most affected by OSHA) were considered but rejected, the first two because of missing data and the last because it would

not distinguish business from union strength in the state.

2. The survey did not include six states for the entire period and seven other states for the latest years. Estimates for states not in the survey were made by multiplying the percentage of total state workers in each two-digit Standard Industrial Classification (SIC) category by the national accident rates for those categories and summing over all categories. The effect of replacing state data with national estimates should decrease variance among states and therefore diminish the likelihood that significant responsiveness would be found.

3. Data for states in the federal program were obtained from a computer printout of annual data prepared by OSHA at the authors' request, and data from states with approved state programs were obtained from annual state report forms supplied by OSHA's Office of State Programs. Although the lack of reporting consistency among states for many of the items on these forms precluded the analysis of other interesting variables, the data on inspections, citations, and penalties are generally accepted as relatively consistent.

4. Attempts to estimate separate state and federal coefficients for the administration and governor variables were unsuccessful because of the instability of estimates when dummy slope variables interacted with these dummy variables, indicating that the large number of results of 0 and 1 introduced multicollinearity problems.

5. A number of analyses were performed that are not reported because of space limitations. Changing the variables in an equation had some effect on the magnitudes of different estimates but did not result in the reversal of signs and had only minor effects on determining which variables had statistically significant effects. We also determined, on the basis of standard *f* tests, that the sum of residual squares in the pooled model was not significantly greater than the combined sum of the residual squares of individual cross-sectional analyses, thus providing a statistical justification for preferring the pooled estimates over separate cross-sectional estimates of coefficients.

6. We attempted to account for the obvious difference between southern Democrats and others by using a dummy variable to represent the southern states, but the interaction term was dropped because it was insignificant in all equations and did not eliminate the unexpected sign.

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THE POLITICS AND ECONOMICS OF THE NEW CITY DEBT

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This paper contrasts nonguaranteed city debt with taxation and general obligation debt. Drawing upon Bureau of the Census, Advisory Commission on Intergovernmental Relations (ACIR) and Municipal Year Book data, the analysis shows that both per capita tax revenue and outstanding general obligation debt are best explained by background factors such as total population, functional scope, and region, while fiscal strain plays a secondary role. Political structures, in the shape of legal constraints on taxation and debt and of form of government, do not account for patterns of these traditional revenue sources. By contrast, nonguaranteed debt is best explained by a model in which fiscal strain has a paramount role, but with both legal constraints on taxing and regional differentiation contributing significantly to the explanation, at least for data collected prior to 1978. The findings suggest a two-tiered model of the revenue side of fiscal decision making, with historical accommodations to powerful economic and demographic factors dominating taxing and general obligation debt, while nonguaranteed debt serves as a flexible instrument of shorter-term strategy.

In the nearly thirty years since Herson's (1957) characterization of the "lost world of municipal government," scholars have left virtually no stone unturned in efforts to bring the methods of empirical social science to bear upon an understanding of urban politics and policy. But there are some stones left remarkably undisturbed. Even in the wake of the fiscal convulsions of the mid-1970s, research on municipal indebtedness has been quite exceptional. In part, this is because municipal finances generally occupy only a portion of the attention of urban scholars. Even within the local government finance realm, there is a bias toward analysis of expenditures rather than revenues because the former are more readily conceptualized as policy outputs (MacManus, 1977, p. 266). To the extent that revenues are of interest, the politics of taxation is usually at issue.

Attention to debt takes a back seat at best.¹

If city debt is still a part of the "lost world" of municipal politics, there is ample reason to rescue it from such a backwater. Indebtedness, like taxation or expenditure policy, is an important component of urban fiscal policy and, according to Lamb and Rappaport (1980, p. 3), an area of heavy activity:

The most dynamic area of the investment industry today, as for the past decade, remains largely untouched in academic or trade literature: the market for municipal securities, which help to finance state and local government functions through the issuance of debt. . . . Taken altogether, the dollar volume of debt issued from 1970 to 1978 by municipalities was about double the total issuance of all corporate debt.

More important, there has been a quiet revolution in the past fifteen years with respect to city indebtedness. Long-term

municipal debt can be divided into two types: (1) general obligation bonds, sometimes referred to as full faith and credit debt, and (2) nonguaranteed debt, encompassing a broad variety of revenue bonds.² Newcomer, Trent, and Flores-Kelly (1983, p. 218) aptly describe the differences between the two types:

General obligation debt is secured by the full faith and credit of a municipality, that is, an unlimited claim on its taxes. Often this type of debt, because it is secured, carries a lower interest rate than most bond yields. . . . There are various types of nonguaranteed bonds, but they are typically sold with the expectation that repayment will be made through a specific revenue source such as a user charge.

Another important difference between the two types of debt has to do with the number of limitations and accountability devices that surround general obligation debt, as compared with revenue bonds. A large number of states set limits on the amount of general obligation debt that municipalities can have, and in many places there are also requirements for voter approval before general obligation bonds can be issued; by contrast, nonguaranteed debt is by and large free from caps and referenda requirements (Newcomer et al. 1983, pp. 218-19; Sbragia, 1983, pp. 72-73).

The quiet revolution referred to above has to do with the explosive growth in the nonguaranteed type of debt, leading to a turnabout in the mixture of debts characterizing urban government. Traditionally, general obligation debt predominated, constituting 60% of long-term municipal debt outstanding in the latter part of the 1960s and through much of the 1970s; however, by the latter part of the 1970s, the revenue bond revolution had begun, and by 1981 and 1982, nonguaranteed debt constituted the major part (59%) of outstanding municipal debt (Bureau of the Census, 1984, p. x).

Yet another reason for scholarly interest to turn to municipal indebtedness is that the growth in local government debt,

and especially in the nonguaranteed type, has not occurred without some controversy, at least within the narrow confines of the intergovernmental policy community with a stake in the matter. Nonguaranteed debt has always been undertaken primarily for facilities and infrastructure development, from the earliest uses for canal and turnpike development to current revenue bonds for "sewers, gas service, telephone systems, parking, airports, ports, rapid transit, stadiums and other recreation facilities, solid waste disposal systems, college dormitories" (Lamb and Rappaport, 1980, p. 14), and the like. Criticism has arisen, however, as revenue bonds, whether for facility development or other functions, have been issued for private users:

The controversy has been triggered by an explosive growth in the volume of tax-exempt bonds issued to benefit private users—bonds ranging from mortgages for purchases of single-family homes, to loans for corporations building pollution control facilities, and to loans to fund facilities for industrial development. (ACIR, 1984b, p. 115)

In a period in which competition for economic development at the local level makes for a blurring of the line between public and private uses, this criticism of private uses of nonguaranteed debt may seem misplaced. However, private uses exacerbate the explosive growth in nonguaranteed debt and lead to an erosion of federal revenue because of the tax exemption of interest on state and local bond issues. Not surprisingly, there has been an ongoing debate about uses of tax-exempt bonds and periodic federal efforts to limit them (Sbragia, 1983). These federal efforts include the Revenue and Expenditure Control Act of 1968, which set some limits on the volume of Industrial Development Bonds (IDBs); the Tax Equity and Fiscal Responsibility Act of 1982, which held that small issue IDBs would not be federally tax-exempt if used for various recreational or commercial pur-

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poses; and the Mortgage Subsidy Bond Tax Act of 1980, which restricted the tax-exempt status of revenue bonds issued to subsidize single family housing mortgages (ACIR, 1984b, pp. 118-19). Currently, the possibility of removing tax-exempt status from municipal revenue bonds is again on the congressional agenda. In short, municipal debt, and perhaps especially the nonguaranteed type, is of interest because it is problematic from the intergovernmental perspective and is chronically the focal point of federal control efforts.

Finally, the study of city debt provides an opportunity for a fresh look at the debate over *political* versus *environmental* explanations in urban scholarship—a debate that has never been adequately resolved (Peterson, 1981, p. 10). Here, political structure variables, fiscal strain measures, and background factors that reflect longer-term, exogenous forces are incorporated into a comparative analysis of urban debt and tax revenue patterns.

Hypotheses and Measurement Issues

Hidden behind the national trends in urban indebtedness is substantial variation across cities in patterns of general obligation and nonguaranteed debt. For the sample of 250 cities upon which this research is based, general obligation debt outstanding in fiscal year 1977 ranged from less than \$1 per capita in communities such as Lakewood, California; Ferndale, Michigan; and Falls Township, Pennsylvania to more than \$1,000 per capita in communities such as Wilmington, Delaware; Braintree Township and Taunton, Massachusetts; and New York City. A similar range of variation existed in 1981.³ With respect to nonguaranteed debt, variation across communities is more striking. In fiscal 1977, nonguaranteed debt outstanding ranged

from less than \$1 per capita in many communities to more than \$1,087 in Decatur, Alabama; \$1,995 in Gainesville, Florida; and \$2,492 in Ashland, Kentucky. In fiscal 1981, again there are many communities with less than \$1 per capita in outstanding nonguaranteed debt; but there is also Jonesboro, Arkansas at \$3,342 per capita and Orlando, Florida at \$3,991, while Ashland, Kentucky had increased its outstanding nonguaranteed debt to \$6,419 per capita.⁴

How might we account for this variation in debt across local governments? Essential to this analysis is the central theme that, partly because it is an innovative fiscal instrument and partly because it is less visible and less enshrined in accountability devices, the incidence of nonguaranteed debt will offer some important contrasts with taxation and traditional, general obligation debt. Hence, the analysis entails a comparison of the predictors on the one hand of per capita tax revenue and outstanding general obligation debt and on the other of outstanding nonguaranteed debt per capita.

Three major classes of independent variables follow. First, is a set of background variables that, largely on the basis of expenditure studies, have been deemed to be important in the analysis of urban fiscal outcomes. Just as with per capita spending, cities' per capita taxation, general obligation indebtedness, and revenue bond indebtedness might be expected to be a positive function of city size, so total *population* is considered as a predictor. Liebert (1976) makes a convincing argument that understanding urban fiscal activity requires attention to differences across U.S. cities in the scope of the functions handled by the city. For this reason, *functional scope* must be included in the analysis. The functional scope index used here is an additive one, based on a count of the following functions for which the Census of Governments reported expenditures in excess of one dollar per capita

Table 1. Regional Variation in City Taxation and Debt

Variables	Regional Means for Sample Cities			
	South	West	Northeast	Midwest
Per capita tax revenue	\$154	130	302	121
Outstanding general obligation debt per capita	258	79	276	178
Outstanding nonguaranteed debt per capita	437	144	14	147
Number of cases	71	49	74	56

for the year at issue: welfare, health and hospitals, education, sewers and sanitation, highways, parks and recreation, and housing and urban renewal (or community development).⁵

Consistent with the basic theme that nonguaranteed debt is an innovative, less visible and less traditional revenue instrument than taxation or general obligation debt, we hypothesize that background variables such as functional scope and city size will be powerful predictors of per capita tax revenue and general obligation debt but quite weak predictors of outstanding nonguaranteed debt. Stated another way, taxation and traditional debt are expected to be responsive to basic, Wagner's Law-style forces (Lowery and Berry, 1983, pp. 667-68), which largely reflect a city's longer-term history. Decision making about revenue bonds, however, is more a reflection of current pressures and opportunities.

Regional variation must be taken into account for similar reasons and also because sunbelt versus frostbelt comparisons have become rather standard fare in analyses of a variety of urban phenomena. As Table 1 shows, however, regional differences in the fiscal phenomena at issue here are more complex than a simple sunbelt versus snowbelt breakdown.⁶ There is some regional variation in per capita city taxes, although the difference is primarily one that contrasts northeastern cities with all

others. General obligation debt is relatively high in both the Northeast and the South, but perhaps the most distinctive regional manifestation with respect to general obligation debt is the low level on average for western cities. Finally, southern cities show dramatically higher levels of revenue bond debt outstanding than any other region, while northeastern cities on average show very little involvement in this form of indebtedness.

Some of this regional variation may simply be a reflection of regional patterning in several of the independent variables outlined below. In this vein, Lineberry and Fowler (1967, p. 506) have argued that region is an interesting variable only insofar as it is an amalgam of differences in history, government structure, demographic patterns, and the like; hence a region variable constitutes a catchall explanation that fails to distinguish the relative importance of the individual variables that comprise it; analysis focusing on first-order variables that are encompassed by the region variable would be preferable. While acknowledging Lineberry and Fowler's point, this analysis treats region like the other two background variables—population size and functional scope. All three are considered, at least initially, along with the explanatory factors outlined below, such that the relative importance of the latter can be ascertained with these background factors taken into account.

Fiscal Strain versus Political Constraint and Response

Although the background variables outlined above must be considered, they do not occupy center stage on grounds of theoretical importance. That place is reserved for a comparison of the explanatory importance of the two other classes of independent variables. One class of independent variables taps various aspects of *fiscal strain*; the other reflects the *political structures* that present constraints (and opportunities) for decision makers.

Fiscal strain. Clark and Ferguson (1983, pp. 43-45) have identified three conceptions of fiscal strain:

One is that a city suffers fiscal strain if it has a weak or declining economic base, especially characterized by population loss. . . . A second conception stresses the municipal bond market which summarizes judgment of informed investors. . . . We consider neither adequate but propose a third incorporating them as components. . . . Our conception . . . leads us to focus on how it obtains resources from its "environment." . . . As the socioeconomic base changes, municipal officials must formulate new policies to maintain balance between resources and expenditures. Cities that do not adapt rapidly enough suffer fiscal strain.

For analysis of urban tax and debt patterns in 1977, three measures, corresponding to each of the three conceptualizations of fiscal strain identified above, can readily be obtained. First, as Clark and Ferguson note (1983, p. 43), *population change* is usually the indicator of fiscal strain in the sense of a declining economic base, and data on population change from 1970 to 1975 reasonably reflect change immediately preceding the 1977 fiscal year. Second, fiscal strain, as reflected in the municipal bond market, is measured by the city's *bond rating* (Moody's) for 1977. Third, as an indicator of the failure-to-adapt conceptualization of fiscal strain, a *fiscal strain index* has been constructed, along lines suggested by Clark and Ferguson (1983, pp. 54-55).

Specifically, the index of fiscal strain shows the extent of local public sector spending relative to city wealth.⁷ City wealth itself is an index, comprised of the summation of two elements: (1) proportion of the city's own source revenues from the property tax times median value of owner-occupied housing and (2) proportion of the city's own source revenues from non-property tax sources times median income.⁸

These three aspects of fiscal strain might be hypothesized to relate to tax, general obligation debt, and non-guaranteed debt patterns in somewhat different ways. For example, fiscal strain in its second (bond rating) sense (so coded that poorer bond ratings have higher values) would be expected to show a positive association with levels of both per capita taxation and traditional general obligation debt. This is because, while bond ratings have often been criticized for poor predictive power, they do represent summary judgments on the part of the financial community about the current fiscal health of the community. A heavy tax burden and substantial outstanding indebtedness (of the general obligation type) are precisely the sorts of phenomena that bond-rating services count toward a poorer bond rating. On the other hand, level of outstanding nonguaranteed debt should not be hypothesized to show such a linkage with bond ratings because the latter, at least as of 1977, represent judgments about the riskiness of the city's general obligation bonds, not its revenue bonds.

Fiscal strain is also reflected in population change. However, it is important to note that either dramatic loss or dramatic gain in population may represent fiscal strain for the community—hence, percentage change in population is transformed into a squared term for the analysis. For example, dramatic population growth might lead to a high level of borrowing in order to finance new infra-

structure, while dramatic population loss may mean that shrinking population bases are carrying the load of infrastructure maintenance and reconstruction, or it may stimulate city officials to undertake capital improvements in the name of economic development. In either case, it may be hypothesized that borrowing in one form or the other will escalate. On the other hand, we would not expect the same connection between population change and taxation patterns. Among growing cities, the tax base expands along with population (although not one for one); therefore, there is not necessarily an increase in per capita taxation even in the face of fiscal strains from growth. In declining cities, limits on public tolerance of taxes or public officials' attempts to regain a competitive position by minimizing taxes can act as stoppers on growth in per capita tax revenues. Intergovernmental revenues can also complicate any clean connection between urban decline and per capita tax revenues.

The fiscal strain index is hypothesized to have a positive, linear association with each of the fiscal outcomes of interest here; that is, a city that is strained by overall public expenditures that are large relative to the city's level of private wealth must presumably make heavy use of as many revenue instruments as possible to sustain its expenditure policies. Additionally, it can be hypothesized that the fiscal strain index should be a more powerful predictor of revenue policies than is population change because while the latter taps only the economic-demographic forces impinging upon the city, the former taps the mesh between city expenditure policies and these external forces. Stated another way, the relative strength of the population change variable and the fiscal strain index in accounting for tax and debt patterns provides an indirect test of Clark and Ferguson's contention (1983) that demographic and economic trends need not

spell fiscal strain, if cities adapt their fiscal policies accordingly. If that contention is true, then population change per se is a relatively poor indicator of fiscal strain and presumably, then, a poorer predictor of tax and debt patterns than is the fiscal strain index.

Political Structures as Constraints. Cities' adaptive responses (or failures to respond adaptively) do not occur in a vacuum, however. While the fiscal strain measures treated above point us toward environmental variables reflecting key external forces like demographic shifts and economic trends, political structures must also be considered. Two political structure explanations, each representing a form of constraint on public officials' tax and debt decision making, are considered here.

First is a family of hypotheses stipulating that reformed cities should show different taxing and debt patterns than unreformed cities. Specifically, a key hypothesis is that communities with reformed political structures should have greater levels of outstanding non-guaranteed debt than communities with unreformed political structures; by contrast, unreformed cities are hypothesized to have higher per capita tax revenues and higher traditional general obligation debt levels than reformed cities.

The expectation of higher taxation and higher traditional debt levels in unreformed cities is consistent with previous research showing that unreformed cities are more responsive to increased spending demands than are reformed cities (Lineberry and Fowler, 1967; Lyons, 1978). Other things being equal then, unreformed cities will end up taxing and borrowing more to sustain spending for services while reformed cities are more sensitive to demands for a low tax profile.

The picture changes considerably, however, when nonguaranteed, revenue bond debt is at issue, largely because of

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the purposes for which this form of debt is used and because nonguaranteed debt need not interfere with a low tax profile (i.e., it is to be paid off with user fees rather than taxes). Reform governments have been supposed to result from and reflect the values of a "public-regarding ethos" (Banfield and Wilson, 1963). This ethos was supposed to be linked with programmatic commitments like urban renewal (Wolfinger and Field, 1966, p. 307) that in more recent literature (e.g., Peterson, 1981) would be categorized as "developmental."

There is considerable evidence that reformism in municipal government was historically instigated by business groups and their allies, that it institutionalized business attitudes and practices within city government, and that these reformers were motivated by larger policy interests that fit nicely into the economic development category (Hays, 1984, pp. 56-59). More specifically:

The increasing size and scope of industry, the effect of floods on many business concerns, the need to promote traffic flows to and from work for both blue-collar and managerial employees—all contributed to this larger interest. The geographically larger private perspectives of upper-class, professional, and business groups gave rise to a geographically larger public perspective. (Hays, 1984, p. 59)

Yet, surprisingly little contemporary empirical analysis has been devoted to the alleged linkages between reformism, business dominance, and interests in citywide growth and development (for an exception, see Morlock, 1974).

Despite this relative paucity of empirical research on the matter, tacit acceptance of Banfield and Wilson's basic contention (1963) that business interests and reform government go arm in arm is quite widespread. What is most important here is that the special amalgam of business influence, growth interests, and economic development politics that is presumed to be especially characteristic of reform cities

is also the condition in which revenue bonds become prominent. They are typically issued either for infrastructure improvements necessary to sustain a competitive business and economic environment (water and sewer, power, or transportation facilities) or for designated "industrial development" purposes (ACIR, 1984b, p. 117).

In short, because of the economic development uses to which it is typically directed and because it can be used expansively without harming the city's low-tax image, nonguaranteed debt is expected to be the only one of the three fiscal instruments examined here that is used more extensively by reformed than non-reformed cities. The analysis thus provides one specific test of the presumed connection between reformism and business and industry-oriented policies.

Some empirical evidence bearing on this specific version of the reformism hypothesis has already been reported. Newcomer, Trent, and Flores-Kelly (1983, p. 223) show that, on average, the dollar volume of outstanding general obligation debt in mayor-council cities exceeds the volume of nonguaranteed debt while, in council-manager cities, outstanding nonguaranteed debt predominates over general obligation debt. The limitations of this evidence are that volumes of debt outstanding are not adjusted for city population size, that a multivariate approach is lacking, and that only one element of reformism is considered. These limitations can, however, be overcome.

Level of reformism is measured here with a three-point additive index constructed from the components that are classically the focus of reformism research: *form of government*, *constituency representation* in the local legislature, and *partisanship* (Lineberry and Fowler, 1967; Wolfinger and Field, 1966). The council-manager form of government is counted as a reform struc-

ture, as is the absence of parties on the ballot. The distinction between at-large and district elections has traditionally been the basis for reform versus non-reform categorization on the constituency representation dimension. However, the proliferation of mixed electoral systems makes a simple differentiation on this basis more difficult. In 1978, for example, nearly 40% of the sample cities for which the *Municipal Yearbook* provides information are listed as having a mixture of council members, nominated or elected both at large and by wards. Unfortunately, information is not provided about the number of council members chosen by each method. Information on the overall size of the council is available, however, and was used to construct a constituency representation component for the reformism index. Because the essence of the at-large versus the ward distinction is whether the council member has a relatively small, localistic constituency or is freed from the particularistic interests of the neighborhood, the population per council member was computed. Cities having larger than average "constituencies" on this measure (i.e., greater than 14,000 people per council member) received an added point on the reformism index; those with smaller than average constituencies did not. On the resulting three-point reformism index, 31 sample cities (16.4%) scored zero, 40 (21%) scored one, 87 cities (46%) had two reform structures, and 31 cities (16%) had all three.⁹

In addition to this form of government index, a pair of additional variables of the political structures type were considered as predictors of tax and debt levels—one reflecting constitutional or statutory *constraints on the property tax* and one summarizing legal *constraints on general obligation borrowing* as of 1977. A host of state constitutional or statutory restrictions surrounded municipal taxing powers and general obligation indebtedness levels

in many states even prior to the tax revolt era associated with the passage of Proposition 13 in 1978. Indeed, some commentators have argued that the growing popularity of nonguaranteed debt lies partly in that it is virtually free of such statutory and constitutional constraints and accountability devices (Sbragia, 1983, p. 72; Newcomer, et al., 1983, p. 219; MacManus, 1981, p. 78). The constraints prior to 1978 may have had fewer teeth than many of those that were to come later; but, to the extent that taxing and general obligation debt limitations were effective in limiting those sources of revenue, city officials might have turned more and more to revenue bonds in order to finance projects that would otherwise have been handled with general obligation bonds backed by tax revenues. In short, we may hypothesize that the stringency of legal constraints on taxing and general obligation borrowing will be negatively associated with per capita levels of tax revenue and outstanding general obligation debt (i.e., an intended impacts hypothesis) and positively associated with per capita levels of nonguaranteed, revenue bond indebtedness (a presumably unintended impact).

Some evidence of these effects is already available. MacManus (1981), for example, found that dependence on general obligation debt decreases and dependence on nonguaranteed debt increases as state imposed borrowing constraints on the former become more restrictive. She also found (1981, pp. 80–81) that reliance on nonguaranteed debt relative to general obligation debt increases sharply as property tax constraints become more severe.

While MacManus's results are consistent with hypotheses to be pursued here, the dependence measures she employs (proportions of total debt of each type) are less than optimal. A given city may show a high proportion of outstanding debt in the nonguaranteed category by

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Table 2. Hypothesized Predictors of Per Capita Tax Revenue, Guaranteed Debt, and Nonguaranteed Debt

Predictor Variables	Tax Revenue	Guaranteed Debt	Nonguaranteed Debt
Fiscal strain			
Bond rating ^a	+	+	0
Squared population change	0	+	+
Fiscal strain index	++	++	++
Political structures			
Reformism index	—	—	+
Tax constraint index	—	—	+
General obligation debt constraint index	—	—	+
Background factors			
Total population	++	++	0
Functional scope index	++	++	0
Region	Northeast	West	South or Northeast

Note: Symbols reported are + for hypothesized positive relationships, — for hypothesized negative relationships, and 0 where no association is hypothesized. A duplicated symbol, as ++, denotes a relationship that is expected to be especially strong. Given the distinctive regional patterns reported in Table 1, different regional dummy variables are relevant to each dependent variable because the region showed either distinctly high or distinctly low on that variable.

^aPoorer bond ratings are coded as having higher numerical values than stronger bond ratings to reflect the assumption that poorer bond ratings indicate higher levels of fiscal strain.

comparison with another city yet have a very low volume of outstanding nonguaranteed debt for its size—that is, it may simply have relatively little outstanding debt of either kind. The dependent variables used here are all based on per capita volumes to avoid this problem. However, the indexes of state property tax and borrowing constraint suggested by MacManus (1981) are adopted here, with some slight modification to update the 1973–74 information in her property tax constraint index with 1976–77 information.¹⁰

By way of summary, Table 2 shows a matrix of the three dependent variables to be compared, the independent variables to be considered, and the hypothesized relationships. The table highlights a few key comparisons emerging from the detail of the hypotheses: (1) The fiscal strain index is hypothesized to be the most potent of the fiscal strain measures and the only

one that has an impact on taxation and both forms of debt in the same manner; (2) each of the political structure variables is hypothesized to differentiate between the traditional revenue instruments (taxing and general obligation debt) on the one hand and nonguaranteed debt on the other, with reformism expected to have the same pattern of impacts as tax and borrowing restrictions; and (3) background variables like population size and functional scope are expected to be most of the story in explaining tax and general obligation debt variation, but not nonguaranteed debt.

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Table 3 shows the zero order correlations between each of the dependent variables and the predictor variables.

**Table 3. Correlations of Predictors of Per Capita Tax Revenue,
Guaranteed Debt, and Nonguaranteed Debt**

Predictor Variables	Per Capita Levels of			Number of Cases
	Tax Revenue	Guaranteed Debt	Nonguaranteed Debt	
Fiscal strain				
Bond rating	.08	.07	.11	164
Squared population change	.02	-.04	.26 ^a	189
Fiscal strain index	.35 ^a	.38 ^a	.49 ^a	163
Political structures				
Reformism index	-.16	-.11	.21 ^b	189
Tax constraint index	-.44 ^a	-.29 ^a	.23 ^a	188
General obligation debt constraint	-.17	-.09	.06	188
Background factors				
Total population	.34 ^a	.39 ^a	-.03	189
Functional scope index	.62 ^a	.49 ^a	-.06	189

^aSignificant at the .001 level or less

^bSignificant at the .01 level or less

Most of the hypotheses receive preliminary support, most notably those specifying (1) that the fiscal strain index will emerge as the most potent of the fiscal strain indicators and (2) that there will be a strong role for the background variables in predicting taxing and general obligation debt but not nonguaranteed debt. In addition, reformism is related to nonguaranteed debt as hypothesized but shows no significant association with the other two fiscal outcomes. The property tax constraint index performs precisely as hypothesized, suggesting that where constraints are stringent, taxes and general obligation debt are minimized but revenue bond usage escalates. The borrowing constraint index, however, shows no significant associations.

To assess the relative importance of each of the variables, however, multivariate analysis is in order. Table 4 presents the results of a pair of multiple regression analyses—one with tax revenues per capita and the other with per capita general obligation debt outstanding as the dependent variable. In each case,

dummy regional variables appropriate for the dependent variable (see Table 1) have been entered, along with predictor variables that emerged in Table 3 as promising explanatory factors.

Table 4 shows that, as expected, considerable variation in both of these traditional revenue policies can be accounted for by the same explanatory model. It is a model in which background variables such as total population size, functional scope, and region predominate. These are the types of variables that represent a city's longer-term history and which are relatively distant from immediate policy choices. Although much less important, the city's adaptive response to shorter-term environmental trends, as reflected in the fiscal strain index, is also a significant predictor because cities that bring their expenditures more into line with available wealth end up with lower per capita demands on the revenue side for either of these traditional fiscal instruments. In this multivariate analysis, the tax-constraint index fails to achieve statistical significance. In these years prior to passage

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Table 4. Explaining Patterns of Per Capita Tax Revenue and General Obligation Debt, 1977

Predictor Variables	Standardized Regression Coefficients	
	Per Capita Tax Revenue	Per Capita General Obligation Debt
1975 population	.29 ^a	.33 ^a
Functional scope index	.29 ^a	.23 ^b
Northeast	.34 ^a	—
West	—	-.21 ^b
Fiscal strain index	.17 ^b	.16 ^c
Tax constraint index	-.09	-.12
Adjusted R ²	.57	.41
Number of cases	161	161

^aSignificant at .001 level or less

^bSignificant at .01 level or less

^cSignificant at .05 level or less

of Proposition 13 and a host of other related measures, tax limitation laws on the books apparently had relatively little effect on the traditional revenue policies at which they were aimed.

The results in Table 4 can be compared with those in Table 5, which shows the results of a multiple regression in which the most promising predictors of nonguaranteed debt (from Table 3) are con-

sidered conjointly. In contrast to taxation and traditional borrowing, variation in nonguaranteed debt is primarily a function of fiscal strain, not background variables such as total size and functional scope. Both dramatic population change (on either the growth or the loss side) and, to an even greater extent, the fiscal strain index are significant predictors of outstanding nonguaranteed debt, suggesting

Table 5. Explaining Patterns of Per Capita Nonguaranteed Debt, 1977

Predictor Variables	Standardized Regression Coefficients
Squared population change	.19 ^a
Fiscal strain index	.45 ^a
South	.20 ^b
Northeast	-.17 ^c
Tax constraint index	.14 ^c
Reformism index	-.01
Adjusted R ²	.41
Number of cases	161

^aSignificant at the .001 level or less.

^bSignificant at the .01 level or less

^cSignificant at the .05 level or less

that involvement in this newer form of indebtedness is very much driven by shorter-term fiscal pressures and the city's adaptation or failure to adapt.

There is also a contrast to taxation and general obligation borrowing here in that the tax constraint index, while less important than the fiscal strain measures, is a significant predictor of outstanding nonguaranteed debt. Evidently, while the weaker forms of tax constraint that were on the books immediately prior to Proposition 13 had no substantial capacity to lessen per capita tax revenues or outstanding general obligation debt, they were associated with increased city efforts to fund projects through the alternative mechanism of revenue bonds.

The reformism index is unrelated to outstanding nonguaranteed debt once these other variables have been taken into account, while regional effects remain significant. However, region and form of government are confounded. The mean reformism index score (on a scale of 0 to 3) is only 0.8 for northeastern cities, contrasted to 1.3 for midwestern cities and a little over 2 for both southern and western cities. To explore the possibility that region variables may be absorbing the explanatory power that reformism might be expected to have, the regression was rerun without regional dummy variables. Still, the reformism index does not emerge as a significant predictor of nonguaranteed debt outstanding ($\beta = 0.09$; significance = 0.18). In short, there is no evidence to support the contention that business dominance and receptivity to economic development concerns led reformist cities to outstrip less reformed cities at revenue bond usage. Instead, there is evidence that, apart from reformism, revenue bonds were used heavily to finance the 1970s' push for a modernized, economically developed New South; and there is evidence that, for reasons that are unclear, the Northeast lagged behind other regions in use of this fiscal instrument.

A Replication of the Analysis: 1981

Among the more notable findings of this analysis of 1977 data is the relatively poor showing of the political structure variables relative to background variables (in the case of tax revenue and general obligation debt per capita) and fiscal strain (for all three dependent variables, but especially nonguaranteed debt). Neither the debt constraint index nor the reformism index is a relevant predictor of any of these fiscal policies; the property tax constraint index is not a significant predictor of either tax revenue or general obligation debt per capita, although it is of modest importance in accounting for nonguaranteed debt per capita.

It could be argued, however, that, apart from imperfect measurement, the choice of reformism and legal restrictions on public finance condemns the political structures explanation to limited importance. On the one hand, reform versus nonreform distinctions that once may have had considerable meaning have surely faded. It is not only that mixtures of reform and nonreform elements have become more and more common; it is also that a breed of new fiscal populist mayors has emerged from broken-down New Deal coalitions (Clark and Ferguson, 1983), and these officials are as fiscally conservative, business-oriented, and committed to economic development as their counterparts in reformed settings; in fact, they may be virtually impossible to distinguish from the classic city manager functioning in a highly reformed setting.

If the reformism index represents a political structure variable that had worn thin by 1977, perhaps the 1977 tax constraint index represents one whose time had not yet come. Beginning in about 1978, a fiscal limitation movement dramatically changed the situation with respect to state-level constraints on local taxing in many locations. This movement introduced many new vehicles for con-

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**Table 6. Explaining Patterns of Per Capita Tax Revenue,
General Obligation Debt, and Nonguaranteed Debt, 1981**

Predictor Variables	Standardized Regression Coefficients		
	Per Capita General Obliga- tion Debt	Per Capita General Obliga- tion Debt	Per Capita Nonguaranteed Debt
1980 population	.36 ^a	.21 ^b	—
Functional scope index	.44 ^a	.17 ^c	—
Northeast	.19 ^b	—	-.30 ^a
West	—	-.29 ^a	—
South	—	—	-.06
Squared population change	—	—	-.002
Fiscal-strain index	.02	.13	.58 ^a
Reformism index	—	—	-.03
Tax constraint index	-.11	-.15	-.006
Adjusted R ²	.53	.27	.35
Number of cases	133	133	133

^aSignificant at .001 level or less

^bSignificant at .01 level or less

^cSignificant at .05 level or less

straint, such that by 1981 the ACIR no longer reported some bits of information that were used in the 1977 property tax constraint index, while newly relevant information began to be reported. This includes information on whether municipalities are subject to limits on assessment increases, a feature that applied in six states by 1982, half of them having adopted this limitation since 1977. Also by 1982, full disclosure requirements applied to municipalities in nine states, five of them having adopted it since 1977.¹¹

An updated, 1981 version of the 1977 analysis is called for, then, in part to provide a more timely estimate of the relevance of tax constraint as a political structure. In addition, an updated analysis may help to shed light on the regional differences observed in the 1977 analysis. If the regional variables represent the traditions and accommodations of the city's long-term history, they should presumably endure in the 1981 analysis. Regional effects may, however, represent evidence of diffusion of fiscal innovation, at least

with respect to nonguaranteed debt; if so, such regional effects may have died out by 1981, when this innovation had become more widely used.

Table 6 presents the results of regression analyses for the 1981 levels of each of the dependent variables, using correspondingly updated versions of the independent variables introduced previously.¹² For reasons outlined above, a substantially new type of property tax constraint index is required. The index developed for this purpose is an additive one, composed of a count of the number of the following limitations that applied to the municipality in 1981, according to information presented by the ACIR (1983): (1) specific millage rate limits, applying only to narrowly defined services; (2) overall limits on the aggregate local government tax rate; (3) a property tax levy limit; (4) limit on assessment increase; and (5) full disclosure requirement.

Table 6 shows that, with respect to taxation and general obligation debt, regression results for fiscal year 1981 essentially

replicate those for 1977. Background variables, such as city size and functional scope, that predominated in accounting for these in 1977 still remain, as do regional effects. The fiscal strain index, of only modest importance in 1977, falls to insignificance in 1981.

With respect to nonguaranteed debt, there appears to have been greater change since 1977. Fiscal strain remains the paramount predictor, but only in Clark and Ferguson's adaptive sense (1983), as reflected in the fiscal strain index; dramatic population change is an insignificant predictor of nonguaranteed indebtedness in 1981. Cities that were subject to the pressures of dramatic population loss or growth in the early part of the 1970s were distinctive in their tendency to turn, in part, to infrastructure investments backed by revenue bonds. But by the latter part of the decade, revenue bond fever had caught on more generally, and cities experiencing dramatic population change were no longer distinctive in their accumulation of nonguaranteed debt.

Likewise, while the Northeast still trails other regions in revenue bond debt, the South no longer stands out as a uniquely heavy accumulator of this form of debt in 1981. The distinctively high levels of revenue bond indebtedness that were obtained in 1977, perhaps reflecting the push to build the infrastructure for an economically advanced New South, are now matched by similarly high levels elsewhere.

The key reason for updating the 1977 analysis, however, is to reconsider the role of the tax constraint measure in light of the new forms of constraint enacted in the wake of the tax revolt. There is some limited evidence of the intended impacts of the new tax constraint. While not achieving the standard .05 level of significance, the negative property tax constraint coefficients in the regression models for taxation and general obliga-

tion debt in 1981 are significant at the .089 and .06 levels respectively, giving some continuing life to the tax cutters' hope that the new forms of constraint would hold back the tide of taxes and tax-backed borrowing. However, Table 6 shows that post-1978 tax constraint is a completely insignificant predictor of nonguaranteed debt. It would appear that, contrary to the hypothesis, these constraints have not had the effect of escalating city dependence upon alternative finance instruments such as revenue bonds. At least, cities subject to stricter tax limitation have not been *visibly* accumulating more revenue bond indebtedness. The possibility of an underground level of reliance on revenue bonds is discussed below.

Discussion

The study of urban public finance has been characterized by an emphasis on the expenditure side and, as Peterson notes (1981, pp. 9-10), by fruitless efforts to isolate political structure variables that have substantial explanatory power.

If one governmental characteristic has no explanatory power, then the analyst need only search for a means of quantifying another . . . [but] a second problem has been the difficulty of incorporating within the bargaining model the numerous instances when "environmental" variables . . . account for much of the variation.

At first blush, the findings here would appear to be yet another affirmation of the importance of environmental variables and the comparative irrelevance of political structures. But let us consider the matter more carefully.

Comparative analysis of nonguaranteed debt as against taxation and general obligation debt reveals important differences, suggesting a two-tiered model of the revenue side of urban finance. The first tier is the realm of the more traditional fiscal instruments—taxes and general obligation borrowing, which are largely influenced by longer-term factors

that must be treated as givens, such as total population, functional scope, and regional location. None of the political structure variables introduced has marked explanatory power in this realm. Even when the more elaborate constitutional and statutory constraints of the post-Proposition 13 era are considered, we find only limited evidence of impacts on taxing and traditional borrowing. The first tier of the revenue picture, like a base budget, reflects long-standing accommodations to powerful economic and demographic realities and to the givens of state versus local allocations of functional responsibility.

Revenue bonds are important because they represent an important, even predominant, part of the second tier of the revenue picture—the realm of choices and adaptations above and beyond the baseline picture established by accommodation to longer-term forces. This second tier is a strategic arena, in which city officials maneuver to adapt to immediate fiscal strain. The adaptation need not involve painful choices about expenditure cuts; rather, it can involve the funding of projects through creative financing arrangements such as revenue bonds. An inclination to pursue such alternatives does not appear to be contingent upon the city's level of reformism. Rather than providing evidence for an iron triangle of reform government, business-oriented development interests, and revenue bond usage, the analysis is consistent with Peterson's argument (1981) that development is a unitary interest of cities generally.

At least in the initial pre-1978 period of escalating nonguaranteed debt, an important political structure variable did help to account for variation in this form of indebtedness. Cities that were subject to comparatively more stringent tax limitation measures were at the forefront of the revenue bond revolution. Curiously, the analysis also shows that the more dra-

matic forms of tax limitation that emerged after 1977 do not appear to have had the same effect. However, an important caveat must be introduced in this regard: it is possible for cities to be heavily involved in revenue bond-financed projects without appearing to be so, simply by creating special purpose, off-budget enterprises (OBEs):

These entities masquerade under a variety of disguises; they are called districts, boards, authorities, agencies, commissions, corporations, and trusts. . . . Their financial activities do not appear in the budget of the government unit or units that created them. Thus, politicians have been able to make part of the public sector simply disappear by forming separate entities . . . to conduct borrowing and spending activities. (Bennett and DiLorenzo, 1983, p. 34)

Bennett and DiLorenzo (1982) have argued that the proliferation of these OBEs and the revenue bonds that they use has been a direct response to tax and spending limitations. From this perspective, post-1977 fiscal limitation measures have had important impacts, but not necessarily the ones that limitation proponents had in mind: public officials have adjusted to stringent limitation in an even less visible way than in the early 1970s by hiding escalating revenue bond debt in off-budget enterprises.

In principle, examining urban debt practices would require analysis of the nonguaranteed debt of OBEs created by each city, as well as the general obligation and nonguaranteed debt issued in the name of the city itself. Unfortunately, such analysis is currently impossible. States do not monitor or report statistics for OBEs (Bennett and DiLorenzo, 1983, p. 37) and, except for some Bureau of the Census data on a selected set of the largest OBEs, officially reported data on debt levels and information enabling large scale matching of OBEs with the appropriate general-purpose governmental unit is unavailable. If Bennett and DiLorenzo are correct, however, more and more outstanding city debt is accumu-

lating in these OBEs, and, if the census bureau does not provide better data on them, future research on city debt may require original data collection on OBEs to supplement officially reported indebtedness.

In sum, there is some evidence here of the importance of one political structure variable—constitutional or statutory fiscal limitation—and the distinct possibility that this variable would show even more explanatory power were it not for the less visible aspects of recent revenue bond activity. More generally, environmental variables will surely dominate any analysis of traditional urban fiscal instruments, and some political structure variables, like reformism, have apparently been made irrelevant by the changing patterns of city politics. But the importance of other political structure variables is unlikely to be evident if the study of city money is restricted to the more traditional and visible revenue instruments. To the extent that political structures provide constraints or opportunities to which city officials respond, those adaptive responses are likely to involve flexible, less visible, and strategically manipulable fiscal instruments such as revenue bonds.

Notes

1. Clark and Ferguson's *City Money* (1983) is a good example of this point. In this extensive treatment of fiscal strain, taxing, spending, and municipal finance, only a handful of pages deal in any way with city indebtedness.

2. The vast majority of municipal debt (more than 95%) in fiscal 1981–82 is long-term rather than short-term, that is, payable more than one year after the date of issue (Bureau of the Census, 1984). This research focuses on the different forms of long-term debt; short-term debt is excluded from the analysis.

3. A random sample of 250 of the 831 municipalities or townships with a 1970 population of at least 25,000 was drawn. In much of the analysis, the *N* falls considerably below 250 because of substantial missing data, especially with respect to political structure variables such as the composition of the

local legislative council. Data on full-faith-and-credit and nonguaranteed debt of cities was obtained from the Bureau of the Census, 1979. Similar data for fiscal 1981 were obtained either from the Bureau of the Census, 1983 or, for smaller communities, from the census bureau's data tape for the 1982 Census of Governments, the latter also reporting fiscal 1981 information.

4. The research focuses on volume outstanding rather than volume issued in a given year because the latter would make the research too dependent upon the vagaries of the year selected for data collection. Particularly since long-term municipal debt frequently involves terms till payoff that are considerably longer than one year (10-year terms are not unusual), volumes of general obligation and nonguaranteed debt outstanding serve as much more useful indicators of the city's cumulative commitments in these spheres of indebtedness.

5. The functional categories used here are similar, but not identical, to those used by Liebert (1976). Liebert, for example, had separate categories for federally aided and general assistance welfare and included a judicial function category. The choice of categories for use in the functional scope index here was dictated by the expenditure categories used in the Census of Governments in both 1977 and 1982. The convention of counting a function only if the amount spent on it exceeds \$1 per capita is one suggested by Liebert (1976, p. 19).

6. The regional groupings used by Wolfinger and Field (1966, p. 474) were adopted here, with the exception that the small number of cities in the border state category were collapsed into the South category.

7. To control for intergovernmental transfers that can allow a city to spend more without extracting more from the local community, total general expenditures minus all state and federal intergovernmental revenues was used as the numerator of the fiscal stress index.

8. Estimates of median, owner-occupied housing value and median household income for 1977 were derived from census bureau figures (Bureau of the Census, 1978, 1984) on these variables for 1969 and 1979 (income) and 1970 and 1980 (housing value). In each case, the estimate was derived by adding seven-tenths of the variable's change in value across the decade to the base (value at the beginning of the decade).

9. Cities with missing data on any of the three components of the reformism index had to be excluded from the analysis. Hence, the effective *N* here of 189.

10. Specifically, the property tax constraint index used here is an additive index based upon the following items from the ACIR's (1977) *Significant Features of Fiscal Federalism, 1976–77*: (a) source of constraint (0 = none, 1 = statutory, 2 = constitutional, 3 = both), (b) millage rate limitation (0 = none, 1

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= over 10 mills, 2 = 1-10 mills), and (c) ability to exceed rate limits (0 = no constraints, 1 = few constraints, 2 = several constraints, 3 = severe constraints). The borrowing constraint index, on the other hand, is precisely as devised by MacManus (1981) from early 1970s information presented in ACIR (1976); since no updated publication of this sort of information was available, the borrowing constraint index could not be updated. However, it was property taxation rather than borrowing that was the focus of major constitutional and statutory limitation changes in the 1970s.

11. Full disclosure is defined by the ACIR (1984, p. 147) as "a procedure designed to promote public discussion and political accountability requiring local governing bodies to advertise and hold public hearings on proposed tax rate increases."

12. The functional responsibility index was updated using expenditure data for 1981. Percentage population change from 1975 to 1980 (again squared) replaced the 1970 to 1975 change variable, and 1980 total population was used instead of 1975 total population. Finally, the fiscal strain index was updated by using 1981 local expenditures in the numerator; the denominator consists of the summation of two elements: 1980 housing value multiplied by property tax revenue as a proportion of all local revenue and 1979 median household income multiplied by non-property tax local revenues as a proportion of all local revenue. Finally, 1981-to-1982 information to update the reformism index was available for the constituency size per representative component and the form of government component; however, updated information on partisanship was not available, so the cities' status on this component as of 1977 was retained in the updated index. The larger number of missing data cases on this updated reform index, however, accounts for the still further lessening of *N* in the 1981 analysis.

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Forthcoming in March

The following articles, controversy and research note have been tentatively scheduled for publication in the March 1987 issue:

Jonathan Bendor and Dilip Mookherjee. "Institutional Structure and the Logic of Ongoing Collective Action"

Richard L. Hall. "Participation and Purpose in Committee Decision Making"

Seif M. Hussein, Bruce Bueno de Mesquita, and David Lalman. "Modelling War and Peace."

Mark S. Kamlet and David C. Mowery. "Influences on Executive and Congressional Budgetary Priorities, 1955-1981"

Douglas Madsen and Peter G. Snow. "Recruitment Contrasts in a Divided Charismatic Movement"

Clement Henry Moore. "Prisoners' Financial Dilemmas: A Consociational Future for Lebanon?"

Peter Ordeshook and Thomas Schwartz. "Agendas and the Control of Political Outcomes"

Benjamin I. Page, Robert Y. Shapiro, and Glenn R. Dempsey. "What Moves Public Opinion?"

T. Wayne Parent, Calvin C. Jillson, and Ronald E. Weber. "Voting Outcomes in the 1984 Democratic Party Primaries and Caucuses"

Kenneth A. Shepsle and Barry R. Weingast. "The Institutional Foundations of Committee Power"

Peeverill Squire, Raymond E. Wolfinger, and David P. Glass. "Residential Mobility and Voter Turnout"

Aaron Wildavsky. "Choosing Preferences by Constructing Institutions: A Cultural Theory of Preference Formation"

CONTROVERSY

DOES HEAVY TURNOUT HELP DEMOCRATS IN PRESIDENTIAL ELECTIONS?

There is conventional political wisdom that high voter turnout in a U.S. presidential election advantages the majority party. Because the Democratic party has been the dominant party in recent decades, this turnout advantage is often believed to accrue to Democratic presidential candidates. In an article in the June 1980 issue of the Review, James DeNardo challenged this conventional view. Indeed, he claimed that the majority party was likely to suffer with increased turnout when the behavior of core and peripheral voters is taken into account. Harvey J. Tucker and Arnold Vedlitz take issue with DeNardo's reasoning and evidence, and DeNardo embellishes and under-scores his original case.

Political practitioners and social scientists have long embraced the proposition that high voter turnout in partisan elections works to the advantage of majority party candidates. In the last several presidential elections the Democratic party has sought to maximize its chances for success through voter registration drives and turnout campaigns. For many, the association between high turnout and Democratic party electoral success has acquired the status of conventional wisdom in U.S. politics.

DeNardo (1980) has argued that this conventional wisdom is incorrect. Moreover, he has claimed to present theoretical and empirical evidence to show that, in certain electoral districts, high turnout may actually work to the advantage of Republican party candidates. According to DeNardo, the electoral advantage of high turnout is sometimes the opposite of what most believe to be true.

We have reexamined the theoretical and empirical relationship between turnout and partisan advantage in U.S. presidential elections. We argue that

DeNardo's empirical test of his model was severely flawed. A more appropriate test finds little support for his model. We also argue that voting models such as DeNardo's are largely irrelevant to the interests of political practitioners. There are a number of important senses in which high turnout works to the advantage of Democratic presidential candidates.

The Conventional and Defection Models

What we call the *conventional model* links high turnout with Democratic party success through the single most important and powerful concept in voting behavior research: socioeconomic status (SES). The tenets of the socioeconomic linkage between turnout and Democratic party success are relatively straightforward:¹

1. The Democratic party is the majority party in the United States in terms of popular party identification.

2. Partisan support is linked to SES of potential voters. Individuals with higher

SES characteristics are more likely to support the Republican party; those with lower SES characteristics are more likely to support the Democratic party.

3. Turnout is linked to SES of potential voters. Individuals with higher SES characteristics are more likely to vote than are those with lower SES characteristics.

The conventional model recognizes the Democratic party as the majority party of the population of potential voters. However, the sample of those who actually vote in a given election is not expected to be randomly selected from the population. Low turnout is expected to work to the advantage of Republicans because their supporters, while a minority of the population, are more likely to vote. Thus, low turnout elections provide greater potential for Republican majorities. High turnout is expected to provide the conditions necessary for Democrats to translate their majority status in the population of potential voters into a majority of those who vote in a given election.

DeNardo (1980) does not challenge the tenets of the conventional model as incorrect. On the contrary, he accepts them. His fundamental critique of the conventional model is that it is incomplete. His rival model, which we call the *defection model*, adds the concepts of peripheral voters and vote switchers to the equation. DeNardo notes that others have shown that peripheral voters, those who vote only in high-turnout elections, have weaker partisan loyalties and are more likely to defect than are core voters. He argues that the partisan advantage of high turnout will depend upon the division of core and peripheral voters in a district. To the extent that high turnout stimulates peripheral voters to defect, high turnout will work to the advantage of the minority party. He says this is most likely to occur in predominantly Democratic districts.

To test his model, DeNardo first pre-

sents a scatterplot of national turnout rates and the Democrats' share of vote in presidential elections from 1932 to 1976. He characterizes these variables as "disimally unrelated" and argues that empirical data refute the predictions of the conventional model with "disconcerting regularity" (1980, p. 406). DeNardo then goes on to present a cross-sectional analysis of congressional elections. He analyzes empirical data from a sample of congressional districts in seven states for six selected elections between 1938 and 1966. He concludes that large turnout generally works to the advantage of the minority party.

DeNardo also finds from his cross-sectional analysis of congressional elections that Democrats benefit more from a big turnout in Republican districts than Republicans benefit in Democratic districts. However, that conclusion is qualified by the observation that the picture has changed over time because the cores have withered in both parties. As a result, he argues, the relationship between turnout and partisan advantage has become weaker in recent elections.

The Conventional and Defection Models Reconsidered

DeNardo finds the conventional model empirically incorrect, but his assessments of both the conventional and defection models are, in our view, very questionable. A scatterplot of the national turnout rate and the Democratic proportion of popular presidential vote is an inappropriate empirical test of the conventional model. Presidential elections are contested on the basis of state, not national, returns. A negative, positive, or neutral relationship between turnout and Democratic proportion of popular vote at the national level can be consistent with a positive relationship between these variables in most states. DeNardo is quite clear

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that the relevant units of analysis are election districts. However, his purported refutation of the conventional model for presidential elections does not employ election districts.

We believe DeNardo's cross-sectional analysis of congressional elections is also flawed, but for a different reason. A cross-sectional model defines high and low turnout and proportion of the vote *empirically*, in terms of contemporary national averages. Yet, both the conventional model and DeNardo's defection model define these concepts *theoretically*, in terms of their historical values within a given election district. Both models require longitudinal data and analysis.

An appropriate test of the conventional and defection models requires analysis of longitudinal data from election districts. DeNardo's analysis of presidential elections employed longitudinal data but not election districts. His analysis of selected congressional elections employed election districts but not longitudinal data. Thus, neither analysis provides an appropriate empirical test.

It is possible to test the conventional and defection models with data from U.S. presidential elections.² For this analysis we present the correlation between level of turnout and vote for the Democratic party for the thirteen presidential elections from 1932 through 1980 (fewer elections for Alaska, Hawaii, and the District of Columbia).³

The conventional model predicts positive correlations between turnout and Democratic vote. The defection model predicts that the correlations will be more strongly positive in Republican states and more strongly negative in Democratic states. We distinguish Republican from Democratic states by two measures: (1) the average Democratic proportion of the presidential vote and (2) the proportion of presidential elections carried by Democratic candidates. Table 1 presents orderings of correlations by these two measures

of partisan strength.

Table 1 shows a positive correlation between turnout and Democratic proportion of the two-party presidential vote in 29 election districts and a negative correlation in 22 districts. Most of the largest negative correlations are exhibited by the most Democratic states. These findings meet the predictions of the defection model. However, there are numerous examples of strongly Republican states with negative correlations and strongly Democratic states with positive correlations. Furthermore, state partisanship does not seem to account very well for the overall ordering of correlations.

Scatterplots of these data (not shown) clarify the relationship between the correlations and state partisanship. The strongest support for the predictions of the defection model appears where partisanship is defined in terms of the average Democratic vote. However, that support is due primarily to a group of outliers. Seven southern states exhibit extremely large negative correlations and extremely high average Democratic proportion of the vote. If these outliers are omitted, the relationship between partisanship and correlation predicted by the defection model all but disappears. The scatterplot between correlations and partisanship defined in terms of proportion of Democratic victories does not exhibit the negative relationship predicted by the defection model. The two appear to be unrelated. However, this scatterplot does highlight another sense in which most of the southern states are anomalous. The seven southern states that were severe outliers on average Democratic vote are not severe outliers on Democratic proportion of victories. For these southern states partisanship defined in terms of average vote is quite different from partisanship in terms of winning elections.

The southern states are anomalous in yet another way—the elections in which high turnout occurred. For most states,

**Table 1. Correlations between Turnout and State Partisan Strength
in Presidential Elections, 1932-1980**

Ordered by Average Democratic Vote			Ordered by Proportion of Democratic Victories		
State	Democratic Vote	Correlation	State	Democratic Victories	Correlation
NE	.42	.50	VT	.15	-.17
KS	.42	.28	AK	.17	-.15
VT	.43	-.17	IN	.23	.58
ID	.45	.21	KS	.23	.28
AK	.45	-.15	ME	.23	.10
WY	.46	.42	ND	.23	.25
NH	.46	.11	NE	.23	.50
SD	.46	.11	SD	.23	.11
ND	.46	.25	CO	.31	.52
IN	.46	.58	IA	.31	.11
ME	.47	.10	NH	.31	.11
CO	.47	.52	AZ	.38	.29
IA	.48	.11	CA	.38	.44
UT	.48	.26	OH	.38	.05
NJ	.49	.08	OR	.38	-.45
AZ	.49	.29	VA	.38	-.58
OH	.49	.05	WY	.38	.42
IL	.50	.48	CT	.46	.22
CT	.50	.22	DE	.46	.25
PA	.50	.34	ID	.46	.21
OK	.50	-.32	MI	.46	-.50
NY	.50	-.15	MT	.46	.54
VA	.51	-.58	NJ	.46	.08
MI	.51	-.50	NY	.46	-.15
NM	.51	.30	OK	.46	-.32
NV	.51	.47	UT	.46	.26
MT	.51	.54	WI	.46	-.12
OR	.51	-.45	FL	.54	-.70
CA	.51	.44	IL	.54	.48
WI	.52	-.12	NM	.54	.30
KY	.52	.24	NV	.54	.47
MD	.52	.32	PA	.54	.34
DE	.53	.25	TN	.54	-.71
MO	.53	-.01	WA	.54	.10
WA	.53	.10	KY	.62	.24
TN	.54	-.71	LA	.69	-.73
WV	.54	.22	MD	.69	.32
FL	.54	-.70	SC	.69	-.85
HI	.55	.22	TX	.69	-.84
MN	.55	-.51	AR	.77	-.62
MA	.55	-.15	GA	.77	-.81
NC	.57	-.31	MA	.77	-.15
RI	.58	-.34	MN	.77	-.51
LA	.61	-.73	MO	.77	-.01
TX	.61	-.84	MS	.77	-.60
AR	.62	-.62	NC	.77	-.31
AL	.63	-.78	RI	.77	-.34
MS	.64	-.60	HI	.83	.22
SC	.64	-.85	AL	.85	-.78
GA	.67	-.81	WV	.85	.22
DC	.76	.52	DC	1.00	.52

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the high turnout elections were those when turnout was high nationally: the elections of 1940 and 1952 to 1968. However, the southern states exhibit a different pattern. For them, turnout increases in each successive election. The unique pattern of increasing turnout in southern states is most probably rooted in changing registration and election practices. At the same time, these southern states are experiencing a shift in relative party strength from the Democratic to the Republican party. The strongest apparent support of the defection model seems more likely a function of coincident change over time than evidence of short-term partisan defection in high-turnout elections.

A more appropriate analysis of the conventional and defection models for the presidential elections of 1932 to 1980 shows that the correlation between turnout and two-party proportion of the vote for Democratic candidates is positive in some states and negative in other states. Clearly, the conventional model does not apply to all states. Some predictions of the defection model seem to be met by a group of southern states. However, closer analysis suggests that these southern states are anomalous in terms of national patterns of behavior. The relationship they exhibit between turnout and partisan advantage does not demonstrate the linkage between turnout and partisan defection predicted by the defection model.

Turnout and Partisan Advantage: A New Approach

DeNardo's and many other scholarly models of electoral advantage define advantage in terms of proportion of two-party vote. The underlying assumption is that advantage is continuous in nature: a higher proportion of the vote is more advantageous than a lower proportion of the vote. Use of correlation coefficients

denotes interest in a linear relationship between two continuous variables.

DeNardo identifies political practitioners and journalists as the most visible adherents of the conventional model. Political practitioners are interested in higher vote proportions, to be sure. However, they are primarily interested in winning elections. For them, the most important election outcomes are discrete, not continuous. They define advantage in terms of winning elections.

Increasing vote share over a historical norm may be neither necessary nor sufficient for winning a given election contest. Political practitioners may be interested in models that define electoral advantage as increasing their share of the two-party vote from 30% to 35% or from 70% to 75%. However, they are even more interested in models that define electoral advantage as increasing their share of the vote from 46% to 51%. When political practitioners say that high turnout works to the advantage of the Democratic party, they may well be speaking of advantage as a discrete concept. The findings of correlation analyses are not entirely relevant to a concept of electoral advantage expressed in terms of winning elections.

We undertook an analysis of turnout and partisan electoral advantage defining each concept in discrete, rather than continuous terms. For each state, each election was classified by a fourfold scheme: above-average turnout, Democratic victory; above-average turnout, Republican victory; below-average turnout, Democratic victory; and below-average turnout, Republican victory. Table 2 summarizes five patterns of relationship between discrete measures of turnout and electoral victory.

The 10 states in which Democrats win a majority of above-average turnout elections and Republicans win a majority of below-average turnout elections provide the strongest support of the conventional model. The 6 states in which Republicans

Table 2. Relation Between Turnout and Electoral Victory, 1932-1980

1. Democrats win majority of above-average turnout elections and Republicans win majority of below-average turnout elections: 10 states, 140 electoral votes.					
CA	IL	NV	WY		
CT	NJ	PA			
DE	NM	UT			
2. Republicans win majority of above-average turnout elections and Democrats win majority of below-average turnout elections: 6 states.					
AL	OK				
FL	SC				
MS	TN				
3. Democrats win majority of above-average turnout elections and Democrats win majority of below-average turnout elections: 13 states.					
AR	HI	MA	MO	WV	
DC	KY	MD	NC		
GA	LA	MN	TX		
4. Republicans win majority of above-average turnout elections and Republicans win majority of below-average turnout elections: 16 states.					
AK	IA	ME	NH	RI	VT
AZ	IN	ND	OH	SD	
CO	KS	NE	OR	VA	
5. Each party wins half of both above-average turnout elections and below-average turnout elections: 6 states.					
ID	NY				
MI	WA				
MT	WI				

win a majority of above-average elections and Democrats win a majority of below-average elections seem to support the defection model. However, closer inspection of these 6 states shows that they are southern states in which consistently increasing turnout coincides with partisan realignment. These states do not exhibit the short-term defection in high-turnout years predicted by the defection model.

In 13 states, Democrats win a majority of above-average and below-average turnout elections. In 16 states, Republicans win a majority of above-average and below-average turnout elections. Thus, in 29 states the majority party is advantaged by both high- and low-turnout elections. In 6 states, there is no majority party and no partisan advantage

associated with above- and below-average turnout.

Analysis of turnout and partisan advantage as discrete concepts demonstrates that there is no relationship in most states. However, that the relationship does exist in a minority of states meets the predictions of the conventional model, not the predictions of the defection model.

Turnout and Partisan Advantage: State vs. National Focus

We have focused on partisan advantage in state presidential elections. When we turn to the question of partisan advantage in national presidential elections, we must recognize that not all state contests are of

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equal importance. To assess the relationship between state-level turnout and partisan advantage in the national election, state findings must be weighted by electoral college votes.

We have argued that the southern states that provide the greatest apparent support for the defection model have unique longitudinal patterns of turnout and do not exhibit the short-term partisan defection predicted by DeNardo's model. Nevertheless, if one were to take the positive and negative correlations of Table 1 above as evidence of the substantive relationship between turnout and electoral advantage, it is noteworthy that the 29 states with positive correlations control 276 electoral votes. The 22 states with negative correlations control 262 electoral votes. Even when the strongest case is made for the defection model, correlation analysis suggests that high state turnout gives the Democrats an advantage in winning presidential elections.

The same conclusion results from weighting the state findings for a discrete model of turnout and partisan advantage by electoral votes. The 10 states in Table 2 above that meet the predictions of the conventional model control 140 electoral votes. The 6 states that meet some of the predictions of the defection model control 64 electoral votes. The 13 states where Democrats win a majority of above- and below-average turnout elections control 134 electoral votes. The 16 states where Republicans win a majority of above- and below-average turnout elections control 113 electoral votes. The 6 states where there is no relationship between turnout and partisan advantage control 87 electoral votes. The electoral votes of the normally Democratic states and the states the Democrats win when turnout is above average sum to a majority. The normally Republican states, the states the Republicans win when turnout is above average, and the states that have no partisan advantage associated with turnout

together control a minority of electoral votes. Thus, even when the strongest case is made for the defection model, discrete analysis suggests that high state turnout gives the Democrats an advantage in winning presidential elections.⁴

Turnout and Partisan Advantage in Perspective

Measurement of the empirical relationship between turnout rate and partisan advantage in U.S. presidential elections is surprisingly complex. Longitudinal data from the appropriate election units, states, are required. Turnout, partisan advantage, and the historical partisan allegiance of a state can each be conceived either as a continuous or discrete concept. High national turnout does not always result in high state-level turnout. Partisan advantage in state presidential elections is not necessarily the same as partisan advantage in national contests for the presidency.

Despite the multiplicity of empirical measures, our analyses yield a consistent set of findings. First, for a majority of states neither the predictions of the conventional model nor those of the defection model are met. Second, a larger number of states controlling a larger number of electoral votes meet the predictions of the conventional model than meet the predictions of the defection model. Third, on closer analysis, the states that provide the strongest apparent support for the defection model do not exhibit the pattern of short-term partisan defection predicted by the model.

DeNardo gave his article the subtitle "The Joke's on the Democrats" to highlight his contention that, contrary to conventional wisdom, large turnout works to the disadvantage of Democratic party candidates. More appropriate analyses demonstrate that, although the conventional model is not appropriate to all states, it is not incorrect. For a number of

states controlling a large number of electoral votes, high turnout has been associated with Democratic state victories. There are many senses in which high state and national turnout have helped Democratic candidates win the presidency. It is the defection model that is infrequently, if ever, supported.

The conventional model is unacceptable as a comprehensive description of patterns of turnout and partisan advantage in presidential elections. Yet it appears to be a plausible model to guide election strategy for state and national party officials. However one defines turnout and partisan advantage, Democratic presidential candidates have been unquestionably advantaged by high turnout in a number of states typically viewed as keys to Democratic victory. High turnout is strongly related to Democratic success in states such as Illinois, California, Pennsylvania, Connecticut, and New Jersey. In this narrower strategic sense, the conventional model has been consistently valid since 1932. High turnout alone cannot guarantee Democratic victories in state or national election contests. Nevertheless, there are good reasons for the Democratic party to work for high turnout in a minority of targeted states and in the nation as a whole.

The bivariate conventional model linking high turnout to majority party electoral advantage is simplistic and incomplete. There are good reasons to seek more sophisticated and successful models of aggregate electoral behavior. However, those reasons do not include the unequivocal empirical failure of the conventional model. For some important interests and in some important senses, the conventional model and conventional wisdom are correct.

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Tucker and Vedlitz deserve kudos for providing new evidence about the enigmatic connection between turnout and the two-party vote and for raising political and statistical questions that have broad relevance to students of voting. Unfortunately, their treatment of the evidence only obscures what is a beautiful and intriguing electoral relationship. In this note I want to show that their negative conclusions are not really grounded in data at all but follow instead from an analytical strategy that discards information whenever it appears unfriendly to the conventional wisdom.

Why the Relationship between Turnout and the Vote Is So Elusive

The commonplace notion that a big turnout helps the Democrats rests on a belief that the voting electorate becomes more Democratic as it gets bigger. While it is plausible to believe that turnout affects the partisan composition of the electorate, it is a mistake to think that the story ends there. What intuition fails to grasp is that swings in turnout also disturb the rates of defection across party lines, thereby distorting the conversion of partisan strength inside the electorate into the final vote. Once this basic complication is recognized, the next problem is to see that the combined impact of the two mechanisms varies with the partisan coloration of the eligible population. Without careful statistical controls over the factors that condition how the two processes interact, the true relationship between turnout and the vote disappears in a sea of confounding effects.

The Effect of Turnout on the Composition of the Electorate

First, let's be clear about what the conventional wisdom says. The most com-

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mon (and naive) version simply assumes that a big turnout helps the majority party. A more sophisticated version considers the quality of both parties' political support. If each party's base in the eligible population consists of a loyal core and a less committed periphery, then intuition tells us that the party with the larger periphery should benefit when turnout goes up. Why? because core partisans turn out consistently while peripheral turnout swings with the circumstances of the political campaign. A rising general rate of turnout therefore signals an influx of peripheral voters into the electorate, adding disproportionately to the ranks of the party with the broader peripheral base. Because the Democrats are the party of the "common man"—drawing more support than do Republicans from politically marginal groups, who are least interested in politics, least educated, and least reliable about voting—surely they ought to benefit from a heavy turnout.

The reader may review my article (DeNardo, 1980) for a formal discussion of this reasoning, but one technical point deserves emphasis here. It turns out that the party with the larger *fraction* of peripheral followers—not the larger absolute number—improves its share of the voting electorate when turnout increases. The point is significant because it means that the benefits of rising turnout do not accrue automatically to whichever party is the majority in a particular state or district. Rather, the Democrats will benefit from a heavy turnout even when they are a minority of the eligible population so long as their base contains a larger fraction of peripherals than does the Republicans' base. The naive version of the conventional wisdom is therefore logically untenable while the sophisticated version makes logical sense.

The Defection Effect

Now if everyone who turned out voted a straight party ticket, the case for the old

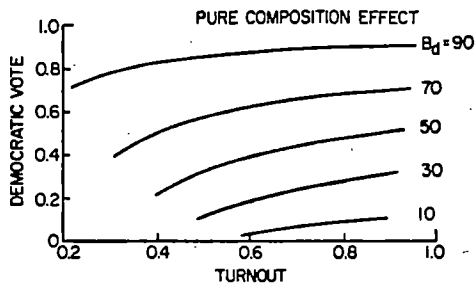
saw would be logically airtight. But, of course, everyone doesn't vote strictly along party lines. We know that cross-over and spit ticket voting happen all the time and that fickle peripheral voters are more easily induced to defect than the loyal core. But if a heavier turnout raises the concentration of peripherals in the electorate, then the normal rates of defection should increase with turnout. The point is crucial because the higher defections will not be politically neutral even when they are visited equally on the two partisan camps. Instead, they will favor the *minority* party (at least on average). Imagine a district in which the Democrats are the overwhelming majority of the voting electorate, say 90%. If defections in each party rose from 0% to 10%, then the Republicans would increase their share of the vote from 10% to 18%. If defections rose to 30%, the two-party vote would become 66% to 34%, and so on.

This reasoning reveals why our intuition is both incomplete and seriously misleading. Higher turnouts may well affect the balance of Democrats and Republicans in the voting electorate as the conventional wisdom assumes. But by bringing more peripheral voters to the polls, they set the stage for rising defections that normally work to the benefit of the minority party.

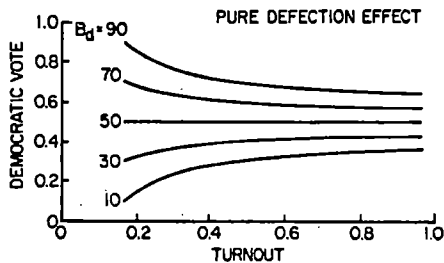
Interactions of the Two Effects

As soon as we accept the idea that peripheral voters defect more readily than do core voters, predictions about the relationship between turnout and the two-party vote must perforce become more complicated than those suggested by the conventional wisdom. While we can predict unambiguously that a big turnout helps the Democrats in Republican strongholds—where both the composition and defection effects work to the Democrats' advantage—the picture is clouded in the Democratic areas where the two

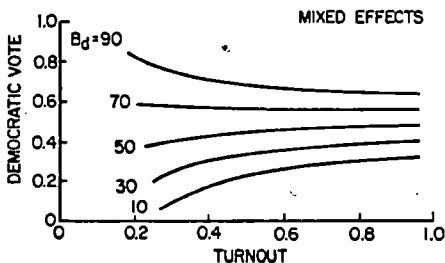
Figure 1. Turnout and the Democratic Share of the Vote



$$\theta_c = .85; P_d = .80; P_r = .30; \rho_c = 0; \delta = 0$$



$$\theta_c = .85; P_d = .80; P_r = .80; \rho_c = 0; \delta = .4$$



$$\theta_c = .85; P_d = .80; P_r = .65; \rho_c = 0; \delta = .4$$

Source: Compiled from Equation 14, DeNardo (1980).

effects pull in opposite directions. There the composition effect favors the Democrats (the party with the larger periphery), while the defection effect favors the Republicans (the minority party).

To understand these complex interactions more fully, consider the graphs in Figure 1, which show how the relationship between turnout and the Democratic

share of the vote varies with the Democratic fraction of the underlying population (B_d), the fraction of peripheral followers in each party's base of support (P_d, P_r), and the difference in the baseline rates of defection between core and peripheral voters (δ). The top panel shows the composition effect in pure form (where everyone votes along party lines and $\delta = 0$). Notice how a big turnout helps the Democrats in all constituencies, whether Democrats are a majority or not, but at different rates according to the size of the Democratic base and the level of turnout in the election. The relationship becomes steeper in all circumstances as the fraction of peripheral followers in the Democratic base (P_d) grows relative to the Republican fraction (P_r). The middle panel illustrates a pure defection effect (when $P_d = P_r$) and the dramatically different pattern of relationship it creates. Now the minority party everywhere enjoys the benefits of a larger turnout, but more so when turnout is low and the majority party is extremely large. Finally, the bottom panel gives a typical example of what happens when both effects operate at once. As always, a big turnout helps the Democrats in Republican areas, but the picture is mixed in the Democratic strongholds. When the Democrats are only a modest majority in the district ($.50 < B_d < .70$), the composition effect governs the sign of the relationship and a big turnout continues to help the Democrats. After the transition point around $B_d = .70$, the defection effect dominates and a big turnout helps the Republicans. The value of B_d where the sign of the relationship reverses depends on the relative strength of the two effects. As the discrepancy between the size of the two parties' peripheries grows bigger and the difference between core and peripheral rates of defection gets smaller, the sign of the relationship remains positive in progressively more Democratic areas, and vice versa.

The reasoning and evidence I presented previously (DeNardo, 1980) strongly suggest that both mechanisms influence the relationship between turnout and the vote in U.S. elections such that a heavy turnout normally helps the minority party. Nor should this finding surprise believers in the conventional wisdom. After all, both effects are logical consequences of the same underlying cause—the political fickleness of the parties' peripheries relative to their cores. If you're willing to believe that core loyalists turn out more reliably than peripherals, why not that they vote more reliably for their party after they turn out?

Tucker and Vedlitz raise no objections to the logical or political validity of my formal reasoning. Instead, they claim that properly chosen evidence does not support it. Given a clear understanding of what my model says, we're now in a position to consider how one might test it empirically.

On the Selection of Relevant Evidence

Figure 1 shows that to estimate without bias the impact of turnout on the two-party vote, one must control for several variables that condition the magnitude and the *direction* of relationship. Above all, one must control for the size of each party's base of support (B_d or B_r) since even the sign of the relationship depends crucially upon it. To select cases for study without regard to the underlying strength of the parties would be tantamount to sampling from different curves in Figure 1, making it impossible to draw useful inferences about the true pattern of relationship. Ideally, one would also wish to hold constant the relative size of the two parties' peripheries (P_d , P_r) and the normal difference between core and peripheral rates of defection (δ). The challenge facing anyone trying to devise empirical tests of the theory is that none of this

information is easy to obtain in U.S. electoral archives. Indeed, the variables P_d , P_r , and δ are unobservable. The problem of achieving statistical control is therefore a delicate one, demanding both good political judgment and detailed theoretical knowledge. Let me explain how I proceeded and then reply to the dismissive comments of my critics.

The evidence analyzed in my article had been assembled previously from rarely studied publications of the states. This unusual collection of information about congressional districts described not only the rates of turnout among registered voters and the Democratic share of the two-party vote (easy to get) but also the partisan division of registered voters in each district (hard to get). The latter information made it possible to solve three critical problems at once. First, one could construct homogeneous subsamples of districts at six different levels of the crucial conditioning variable B_d . Second, one could measure turnout as a fraction of the population that the variable B_d actually described (here the registered electorate). And finally, there were a sufficient number of cases that each of the subsamples could be drawn from a single election year. In all, I analyzed six separate subsamples in each of six years.

Restricting each of the subsamples to cases from a single year provided valuable indirect control over the remaining conditioning variables in the model. Obviously, no public records tally the relative size of the peripheries in the two parties or the base rates of defection among core and peripheral partisans in congressional elections. Nonetheless, it seems plausible that in a *single year* these quantities should remain relatively stable across congressional districts, particularly among districts that had similar fractions of Republicans and Democrats in the registered electorate. These data provide a reasonable hope that the internal structure of the parties and the behavioral patterns of

voters would not fluctuate radically over the districts contained in each subsample. When regression analyses confirmed that the Democrats benefited from rising turnout in all of the Republican subsamples, while in 7 of 10 Democratic samples the Republicans did better (though less so than the Democrats in symmetric circumstances, just as predicted), it seemed justified to conclude on both theoretical and empirical grounds that the conventional wisdom was lacking. A big turnout generally helps the minority party.

None of these considerations seems persuasive to my critics. But they provide no reasons why a longitudinal analysis will provide better statistical control over the conditioning variables in the model. Apparently, Tucker and Vedlitz find it self-evident that to learn the effects of rising turnout in a district, we must observe that particular district over time. Their position is tantamount to denying the utility of all cross-sectional studies, as if to say that one couldn't learn anything from a sample survey about the effect of aging on turnout because no one grows older during the interview. To paraphrase their position: A sample survey model defines old and young in terms of contemporary sample averages. Yet, the conventional model defines these concepts theoretically in terms of their values within a single person. Therefore only a longitudinal study of individuals can be appropriate.

A vast body of statistical theory and experience refutes this position completely. What is required for unbiased inference is statistical control over those parameters in the model that condition the relationship between turnout and the vote. By failing to understand that I have taken every feasible precaution to achieve such control, Tucker and Vedlitz disregard some of the best evidence we have on the problem.⁵

Next consider how Tucker and Vedlitz propose to rectify the "severe flaws" of my approach. Instead of considering

statistical cross-sections, they calculate the product-moment correlation between turnout and the presidential vote in each of the states over the period between 1932 and 1980. Then they investigate the relationship between the correlations and two measures of the partisan balance in the states. Before we study their results, it is important to understand why this procedure is suspect. First, their analysis provides only weak control over the strength of the parties in each state. The trouble, of course, is that the summary statistics describe elections spanning a fifty-year period. Are we to assume that the fraction Democratic in each state remains roughly constant over this time? If not—and who wouldn't harbor doubts—then the cases in their subsamples would not be selected from a single curve in Figure 1 corresponding to a fixed level of B_d . On the contrary, they might even be sampled from curves with different signs.

Next, consider the procedure used to measure B_d . Rather than measuring the fraction of registered voters who belong to each party, as I did, my critics calculate B_d as the average share of the vote (and then the proportion of wins) for the Democrats. It is easy to see why both of their measures will give biased estimates of the parties' strength in the eligible (or registered) electorate. As I have shown, the two-party vote reflects not only the underlying partisanship of the state but the distortions introduced by the effects of turnout and defections. Consider the bottom panel in Figure 1 again, and study what the average vote would be in states with various levels of B_d . In most cases, the vote is closer to 50% than is the actual partisan division of eligible voters (mainly because defections push the vote toward 50/50). The proportions of wins will produce a bias of another kind, describing as it does an S-shaped function of B_d . Clearly, when either party gets to be 60% of the electorate or more, it will win almost

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every election that is held. Thus, neither of my critics' procedures provides an unbiased measure of partisanship in the population for which turnouts are calculated.

Finally, it should be clear that samples spanning 50 years will provide virtually no control over the peripherality of the parties' support or the crucial defection variable δ . During the period under study, the U.S. electorate underwent a severe dealignment, the technology of political campaigning changed drastically, party organizations fell into disrepair, the franchise was extended to 18-year-olds, and the electoral rules evolved in ways that favored split ticket voting. Are we to believe that the critical conditioning variables identified by the model must remain stable over 50 years time just because the cases are drawn from single states?

When we add to these difficulties the decision of my critics to summarize the evidence with linear correlation coefficients—even after I had proven the relationships in question to be nonlinear and even though regression coefficients provide more relevant information—students of elections may well understand why their approach is less responsive to the demands of the problem than is my own. The surprise is that they do obtain interesting results despite the shortcomings of their method.

What the Results Mean

In fact, Tucker and Vedlitz's correlations follow exactly the pattern my model predicts. Consider their Table 1, which shows the correlations as a function of the average vote in each state. Tucker and Vedlitz agree that this summary of the evidence provides some support for my reasoning. "However," they say

that support is due primarily to a group of outliers. Seven southern states exhibit extremely large negative correlations and extremely high average Democratic proportion of the vote. If

these outliers are omitted, the relationship between partisanship and correlation predicted by the defection model all but disappears.

The first thing to notice is how Tucker and Vedlitz discard precisely the states that would be most likely to produce a negative relationship (review the bottom panel of Figure 1). Since theory tells us that highly Democratic states will provide the most striking evidence of the defection effect, if it exists, it is disconcerting to see these cases dismissed so casually—and conveniently—as "outliers." But is it true, after setting the southern states aside, that the relationship predicted by my model "all but disappears"? Are there really "numerous cases of strongly Republican states with negative correlations and strongly Democratic states with positive correlations"? Of the 17 Republican states in the sample, exactly 2 show a negative correlation ($-.15$ and $-.17$). And what of the remaining Democratic states? Do they show a consistently positive relationship? On the contrary, if we exclude the only true outlier in the sample (the District of Columbia), then 11 of the 21 remaining Democratic states show a negative relationship (8 of the 11 with negative correlations exceeding $.31$). Were we to include the southern states, then 18 of 28 Democratic states show a negative relationship. But this is *precisely* the pattern predicted by my model. The bottom panel in Figure 1 shows that a mixture of the composition and defection effects should produce consistently positive relationships in Republican areas, consistently negative relationships in heavily Democratic areas, and mixed results in moderately Democratic areas. Tucker and Vedlitz obtain negative correlations in fully 64% of the Democratic states in their sample. Does this pattern conform to the conventional wisdom? Would anything about the conventional wisdom lead them to anticipate that a big turnout has helped the minority party in 73% of the states?

My critics are not impressed easily. They press their case for the conventional

wisdom by throwing more evidence away. First, they discard their percentage of the vote variable in favor of proportion of wins for the Democrats. Now the "seven southern states that were severe outliers on average Democratic vote are not severe outliers on Democratic proportion of victories." Of course not. That's because even states with a mild Democratic majority will produce a high proportion of Democratic victories. In effect, the new measure wastes more information about the underlying division of partisan loyalties in the states by suppressing differences between modestly and heavily skewed electorates.

Finally Tucker and Vedlitz declare that winning is all that matters in electoral politics (notwithstanding much literature to the contrary) and proceed to collapse their continuous variables into dichotomies. By discarding a large fraction of the remaining information in their sample, they succeed in obscuring the last vestiges of the patterns I had predicted. Of course, even if one were strictly interested in winning, it would be a mistake to throw away the continuous information in the sample before estimating the relationship between turnout and the vote.

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Notes

1. See, for example, Campbell and Miller (1957), Campbell, Converse, Miller, and Stokes (1960), DeVries and Tarrance (1972), Key (1966), Milbrath and Goel (1977), and Verba and Nie (1972).

2. We would argue that longitudinal analysis of congressional elections by district is severely complicated by reapportionment and presidential elections. In recent years, congressional districts have been reapportioned every 10 years. The five available elections are an insufficient data base to assess the relationship between turnout and partisan

advantage in congressional elections and to control for the impact of presidential elections on congressional election turnout.

3. DeNardo analyzed the elections of 1932 to 1976. Inclusion of the election of 1980 does not affect the findings reported in this paper.

4. We also questioned whether, in those states in which one party won a majority of above- and below-average turnout elections, the minority party won a higher proportion (although less than half) of above-average turnout than below-average turnout elections. Of the 13 "Democratic majority" states, Republicans won a higher minority of above-average turnout elections in 4 states with a total of 57 electoral votes. Of the 16 "Republican majority" states, Democrats won a higher minority of above average elections in 8 states with a total of 55 electoral votes. Thus, the advantage of above-average turnout to the minority party in a minority of elections is balanced between the Republicans and Democrats. Overall, above-average turnout still works to the advantage of the Democrats.

5. Their argument against the presidential data I consider is gratuitous. Since I explain at length why such evidence does not constitute an appropriate test of my model (DeNardo, 1980, p. 412), to call my treatment of these data a "test" completely misrepresents what I said. The important point to notice is that my arguments against these longitudinal data apply equally well to the data of Tucker and Vedlitz.

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The Flight into Inwardness: An Exposition and Critique of Herbert Marcuse's Theory of Liberative Aesthetics. By Timothy J. Lukes. (Selingsgrove, PA: Susquehanna University Press, 1985. Pp. 178. \$24.00.)

Science and The Revenge of Nature: Marcuse and Habermas. By C. Fred Alford. (Gainesville: University Presses of Florida, 1985. Pp. x + 226. \$24.50.)

Critique, Norm and Utopia: A Study of the Foundations of Critical Theory. By Selya Benhabib. (New York: Columbia University Press, 1986. Pp. xiii + 455. \$35.00.)

These books focus on how the Frankfurt tradition of critical theory can contribute to the discussion of some intensely contested questions in philosophy and political theory: How does the aesthetic imagination relate to political activity? How deeply is the project of modern science intertwined with the dilemmas of modern society? How can we develop a defensible normative foundation for political thinking.

Timothy Lukes looks at the question of aesthetics and politics in Marcuse's work, in particular the claim in his last book that aesthetic imagination offers a permanent bulwark against "one-dimensional society" and a potential source of a new, radical political orientation. Lukes is sharply critical of this turn to aesthetics, this "flight into inwardness." He argues that recourse to aesthetic imagination is in no sense necessarily linked to the subsequent emergence of radical political action. More likely—and historically, more frequently—it leads to political resignation and impotence.

Given the way Lukes presents Marcuse's position, he does a good job of showing its weakness and revealing the perennial danger of "a premature integration of the political and the aesthetic" (pp. 162-63), but for anyone who takes Marcuse seriously (which is not the same as thinking he is right), this sort of refutation prematurely cuts off speculation about his work and how it might possibly inform current thought and action. In this light, I find it

somewhat surprising that Lukes never mentions either the recent work of post-structuralists and feminists or contemporary discussions of new social movements. Many of the same issues Lukes deals with in refuting Marcuse are being extensively debated in such contexts. For example, Lukes traces Marcuse's failures back to his abandonment of various Kantian dualities, such as subject/nature-as-object and reason/sensibility. Many post-structuralists and feminists are relatively close to Marcuse here, and Lukes would have made his adversary tougher and more interesting if he had allowed such thinkers into the discussion as Marcusean advocates. Had they been brought in, Lukes might have explored, in a more provocative way, the implications of Marcuse's emphasis on a new aesthetic sensibility emerging in recent social and political movements, especially among women and radical ecologists.

Fred Alford's book is strong in precisely the way Lukes's is weak. He examines the controversy between Marcuse and Habermas over the nature of science, and asks us to reexamine the stereotypic views of these thinkers in light of the recent ferment in Anglo-American philosophy of science (Kuhn, Feyerabend, Rorty, Hesse, etc.). Although none of these philosophers explicitly endorses Marcuse's idea of a "new science," Alford uses the implications of their work to draw the conclusion that "in some respects Habermas' view of science is less satisfactory than Marcuse's" (p. 2).

Alford's title refers to Horkheimer and Adorno's thesis of the "dialectic of enlightenment," which holds an instrumental (read *scientific*) orientation to the world to be responsible for an attitude of domination whose corrosive effects spread from external nature to internal, human nature, as well as to intersubjective relations. The "revenge of nature" thus refers to external environmental crises as well as social and psychological problems stemming from repressive strategies toward internal nature, in particular those typical of the modern world where the erotic, aesthetic, and playful qualities of human

beings are shunted aside by rational and calculative ones.

Both Habermas and Marcuse are deeply influenced by this set of concerns, and Alford examines their philosophical speculations about science from this perspective. His analysis is sophisticated, thoroughly informed, and judicious. He probes with particular care the many changes and ambiguities within the two theorists' work. The richness of this discussion makes it particularly difficult to summarize in a short review.

Marcuse, as is well known, developed the idea of a "new science," which he hoped would ultimately transform our conception of the relation of man to both inner and outer nature. He looked forward to a time in which a more nonrepressive attitude to internal nature would allow Eros to become a "builder of culture," and in which our relation to external nature would be characterized by "sensuous perception," not an abstractive, instrumental one (Alford, pp. 39-58). Alford does not in any literal sense defend Marcuse's "new science," but rather asserts simply that he admires its "utter radicalism" and implicit recognition of what the theoretical pluralism of the new philosophy of science tells us: that grand philosophical speculation about science "should stem from free and playful thought and should be recognized as such" (pp. 15, 171-73).

Marcuse's speculations about science have typically met with skepticism because they appear to involve a wildly utopian notion of universal reconciliation of man and nature. Habermas's route to solving the problems of the "dialectic of enlightenment" is usually seen as more plausible. Although his precise strategy has evolved, his underlying intention has been to accept, on the one hand, that man's relationship with nature is unavoidably instrumental, but to demonstrate, on the other hand, that this orientation has become unnecessarily imperialistic in modern society, submerging our capacity for normative and aesthetic-expressive rationalization.

What disturbs Alford and other recent critics is that Habermas's conceptual scheme of types of knowledge (theoretical, normative, aesthetic-expressive) is too rigid, in the sense that it arbitrarily sets up limits to the kind of conceptual revision that should result from taking account of "radically new categories of

experience" (p. 43). The last and best chapter of the book tries to establish this claim. Alford tries to undermine Habermas's contention that an instrumental attitude to nature is the only one that is "*theoretically fruitful*" (p. 148). He makes use of anthropological examples of "non-instrumental healing (and harming) methods" to establish the point that, at least with regard to internal nature, we can have experience which is not very easily accommodated within Habermas's conceptual scheme (p. 152 ff). Although I think that Habermas could in fact deal with these examples more easily than Alford would allow, the discussion is nevertheless provocative and focused on key questions.

Benhabib's focus is the normative foundation of social theory and political practice. She states in the preface that she had originally intended to examine the tradition of critical theory, beginning with Hegel's critique of Kant, in order to show that Habermas's "communicative ethics" was an untenable foundation. In the course of this project, however, she became convinced that Habermas's main ideas were correct. The key question then became what further insights could be gained from sources such as Hegel for the task of further developing the insights of communicative ethics.

This initial standpoint gives Benhabib's book a somewhat strange quality, for her exhaustive rethinking of the tradition of critical theory leads her to endorse, one after another, each of the major points Habermas has been making over the last couple decades: that the "work model" of action is inadequate; that the notion of autonomy and the kind of politics that emerges from the peculiar standpoint (adopted by Hegel and successors) of the "collective subject" are flawed; and that the approach of Habermas's predecessors in critical theory was becoming less and less likely to have any leavening influence on mainstream social science.

Although Benhabib thus rediscovers much that was already discovered, one should not conclude that this is a book without much new to say. First, it is useful because of its clear and penetrating discussions of the central philosophical tensions and shifts that have emerged from Hegel to Habermas. Where this book really distinguishes itself, however, is in its incisive discussions of the nature of action and

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of ways to make communicative ethics more defensible. As regards the concept of action, Benhabib, like Habermas, wants to reorient our thinking away from its focus on subjectivity. She argues that action has certain ontological qualities—its linguistic nature and “interpretive indeterminacy”—that can only be adequately grasped by theories emphasizing “radical intersubjectivity and pluralism” (pp. 55, 87ff, 136ff, and ch. 8).

Although Benhabib’s insights run generally parallel here to Habermas’s, she develops them independently enough that she can employ some of them against him in a critical fashion. For example, she worries that his insights about intersubjectivity and pluralism are threatened at those points in his theory where he employs transcendentalist figures of thought and speaks of a reconstruction of the history of the species’ competence for moral thinking. Although I would argue she overplays this threat, Benhabib is clearly correct to be concerned and to draw attention to the proper order of values in communicative ethics. Her insights on these issues are also supplemented by a very good analysis of the problem of moral judgement, and of how communicative ethics must open itself more to what feminists such as Carol Gilligan have been saying about this topic. In sum, Benhabib gives anyone interested in communicative ethics and politics some fresh ideas on how, with Habermas, we can think beyond the philosophy of the subject, without running blindly into the embrace of post-structuralism.

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Constitutional Choices. By Laurence H. Tribe. (Cambridge: Harvard University Press, 1985. Pp. xiv + 458. \$29.95.)

God Save This Honorable Court: How the Choice of Supreme Court Justices Shapes Our History. By Laurence H. Tribe. (New York: Random House, Inc., 1985. Pp. xii + 171. \$17.95.)

Politics, Democracy, and the Supreme Court: Essays on The Frontier of Constitutional Theory. By Arthur S. Miller. (Westport, CT: Greenwood Press, 1985. Pp. viii + 368. \$35.00.)

Tribe and Miller seek a new legal realism, or contextualism, in the making of constitutional choices and reject much of modern constitutional theory and practice for failing to protect rights effectively and to limit the abuse of public and private power. They want the Court to make choices based not on a priori principles that endure, but on 1) political-structure values and rights values in the context of each case and 2) on impressions about the nature of political, economic, and social facts and inequality.

Constitutional Choices consists of 16 superb—at times brilliant—essays, 12 of which have not appeared elsewhere in their present form, that argue against the present trend of grand constitutional theory toward a segmentation into groups of scholars who advocate that the legitimacy of judicial review can only be sustained by staying close to the Framers’ intentions (Robert Bork), by proceduralism (reinforcing the structures of representation and stopping racial prejudice, (e.g., John Hart Ely), or by creating rights (Michael Perry).

Tribe describes the themes that cut across the essays: the domination of constitutional theory and practice by “the dangerous allure of proceduralism,” “the paralyzing seduction of neutrality,” “the morally anesthetizing imagery of the ‘natural’ in gender, race, and economic inequality relations,” “the hidden . . . tilt of various constitutional doctrines towards the perpetuation of unjust hierarchies of race, gender and class,” and the potential of various forms of constitutional argument to deflect judicial responsibility from crucial substantive choices onto external circumstances or actors. These essays, which should be considered necessary reading for scholars and students of constitutional law, cover a full range of issues; congressional limitations on Supreme Court jurisdiction, the legislative veto, standing, state sovereignty, interstate bank mergers, freedom of speech, the compensation and contract clauses, affirmative action, gender categories in the law, and state action.

The strength of the book is that it actually shows us one of the foremost constitutional scholars and practitioners at work making constitutional choices—that is, considering the relationship in the Constitution between structural values and rights values in the light of the realities of economic and social power. His analyses result in a most perceptive critique of

constitutional theory and the practice of the Burger Court, showing that neither rights values nor representation-reinforcing values alone can inform constitutional choices; nor are they inherently more neutral. He argues that both values must be viewed in tension in all areas of constitutional law. To base one's constitutional choices on only one of these values, or on Bork's interpretivism, creates a priori areas of public law that are by definition either out of bounds or in bounds for the Supreme Court or political institutions. Modern constitutional theory fails to keep alive the problematic nature of judicial and political power.

Tribe would like the Court to make affirmative choices about economic, social, and political realities rather than use what he considers to be falsely neutral categories in the law. If such affirmative choices are not made, then constitutional choices will replicate or increase inequalities through law. In the area of First Amendment jurisprudence, Tribe argues that efforts to secure content-neutral categories; to make distinctions between speech and action; to create different rules for obscenity, commercial and libelous speech, and public and private forums; and to secure equal versus guaranteed access rules—that is, the concern for neutrality over all other values—lead to grossly inadequate protections for the speech of the poor, the politically weak in society, and those with unpopular, less-conventional viewpoints. He wants the court to make substantive choices based on the resources available to individuals, groups, and institutions so it can affirmatively protect all who seek to influence the marketplace of ideas. Also, there has been too much “cost-benefit” balancing of direct effects of speech activities with the diffuse but crucial speech values and with the favoring of allocational efficiency (merely more speech), rather than distributional equity.

Similar arguments are made about equal-protection categories. Tribe argues that because only women can bear children and men now are not required to give a part of their body to sustain the life of a child, the court should affirmatively protect women's right to choose abortion by requiring Medicaid payments for abortions. Rights of privacy, Ely's proceduralism, and Bork's interpretivism do not go far enough in providing equal pro-

tection of the law given this physical reality. Also, Tribe opposes society's viewing of the sexual mores of men and women, as groups, as different and “natural” (as in statutory rape cases) because this perpetuates stereotypes about both genders.

In the area of separation of powers, Tribe's contextualism is seen in his rejection of Lochneresque categories, formalisms, and principles used by the Burger court and constitutional scholars because they hide the complexities of institutional power. For example, he rejects the distinctions among judicial, congressional, and presidential power used by the Burger court to outlaw legislative vetoes and to bankrupt the power of the courts. He opposes Supreme Court review of congressional decisions about whether states have retitled a proposed amendment because the amendment process is the way to overturn Court decisions. Tribe also opposes congressional efforts to limit the jurisdiction of the Supreme Court and lower federal courts because such efforts would violate Supreme Court powers under Article III, and substantive rights values in the Bill of Rights.

While Tribe's criticism of present constitutional theory and practice is superb, he has gone overboard in his contextualism. It leads to a rejection of abstract theory itself—to nominalism—which may result in outcomes he opposes: less active discussions about what process and rights values to choose and less consideration of the problematic nature of all uses of power.

The problem in his approach is that there is no unifying theory for deciding such questions as when to grant power to one branch of government over another, when to protect one group or individual instead of another, or when to grant individualized determinations by government. The standards are inductively determined by looking at specific factual situations in light of views concerning (1) political, economic, and social inequalities in our society, (2) general political structure (3) substantive values in the First Amendment; (4) the Equal Protection Clause; (5) and the doctrine of separation of powers that are discovered as cases are decided.

Tribe has made a good case for the lack of neutrality of categories in free speech cases that involve speech conduct, content-neutral mechanisms by government, and the looking

at different types of speech with different standards. However, he has not shown that to allow courts and scholars to abandon the search for neutrality, to read basic social and economic facts, and to make ad hoc affirmative rights protection choices is a better way to proceed. The rejection of a priori principled constitutional theory in favor of an inductive, ad hoc process of rights and political structure choices is an open invitation to jurists whose readings of the nature of social and economic equality are different to come up with decisions that may be hurtful to the groups Tribe seeks to protect. Neutral categories (even if not as truly neutral as Tribe would like) may be more protective than a deconstructed contextualism of First Amendment and equal-protection values, for they set limits on the constitutional choices of jurists with more conservative economic and social philosophies.

Tribe's opposition to the use of formalisms in deciding the nature of individual rights or the powers of government undercuts one of constitutional law's most important devices to limit politics and protects rights. The long-term effect of the denial of abstract principles, formality in constitutional law, and the Framers' intentions in favor of institutional innovation, while implicitly supporting the Framers' values such as the need for the independence and accountability of each branch under the doctrines of the separation of powers is to denude constitutional theory of its ability to be prescriptive and educational. It could lead to a non-Euclideanism—a relativism like that which plagued pluralist scholarship of American institutions and constitutional law in the 1950s and 1960s. Relativism then led to a delegitimation of the checking power of constitutional law; to the development of theory without critical principles because its principles came from "what is" and to the failure to create and justify benchmarks for testing whether society met process and rights values in the Constitution.

Ironically, Tribe's argument against modern constitutional theory and in support of doing constitutional law shows the need for more systematic constitutional theory and has within it some of the criteria for such theory. Tribe's important demonstration of the impossibility of separating process and rights values in making constitutional choices could

be a central premise of the new theory. It might draw on empirical studies of abuses of power and rights on the national, state, local, family, and individual levels, and of the use of power by the Court versus its use by political institutions. It might be informed by normative theories of rights. It would be a theory with moving categories of process and rights that would suggest how to make tough choices, how the overall scheme of constitutional categories influences the choices made within a specific category, and how the categories resonate with inequalities in social, economic, and political life.

In *God Save This Honorable Court*, Tribe offers well-known anecdotes from history to argue against the conventional wisdom that presidents are surprised about what their Supreme Court nominees do once they get on the Court; that the appointments of specific presidents, even when few in number, have not made a significant difference in the direction of constitutional law; and that the Senate has in the past been spineless in consenting to presidential nominations to the Court.

More interesting are Tribe's arguments for the Senate's taking a more active role in judicial selection. These include the Senate's greater diversity of viewpoint, compared with that of the president, due to ethnic, class, race, sectional, and party diversity; the greater number of electoral snapshots of public opinion in Congress and the Senate; and the future-looking direction of the Senate and House versus the past-looking views of (lame-duck) presidents.

Tribe urges the Senate to support nominees with complex judicial philosophies and views on both institutional relationships and substantive rights values in the Constitution. They should understand the constitutional questions raised, for example, by the abortion rights case, *Roe v. Wade*, rather than be asked to pass a litmus test of support or opposition to that case. The Senate is asked to make judgments about whether a nominee is within "the outer limits" or boundaries of constitutional or judicial philosophy. For example, the nominee should favor the incorporation doctrine and major cases opposing racial discrimination. Tribe is most concerned about securing ideological diversity on the Supreme Court. He advocates that the Senate should consider the judicial philosophy of a nominee in light of the

mix of philosophies already on the Court.

In this book, Tribe's contextualism is evident in his failure to consider theories and systematic empirical studies on legislative behavior. Large representative bodies, like the Senate, with ends-oriented members surrounded by single-issue interest groups, tend to favor compromise. Lowest-common-denominator nominees who are not objectionable to any significant subgroup would be chosen, rather than women or men of strong judicial philosophy with leadership qualities, judicial temperaments, and qualities of mind capable of meeting Tribe's goal of countering the dominant judicial philosophies already present on the Court.

Arthur S. Miller offers 11 essays, only one of which is original to the volume, that are highly critical of modern constitutional theory for its lack of political theory and doctrine, which could make it a strong molder of change in society. Law made by lawyers, rather than by political theorists, allows politics, presidents, and the powerful to dominate society and fails to provide a utopian vision of

the future. The potential for change through judicial review is limited because doctrine assumes there is accountability in the political system and concern for human dignity. The problem lies in archaic distinctions such as that between public and private spheres and the separation of powers. Miller's call for new doctrine to curtail presidents' unlimited powers in foreign affairs and war, to create a fundamental right to employment in this age of technological change, to establish democracy in corporations, and to protect the world environment. Although his critique offers a refreshing look at the moderate nature of reform in our pluralist society, Miller fails to provide us with an alternative political or constitutional theory, an internal critique of doctrine (as Tribe does), or a systematic analysis of the law, political institutions, or society that would help us achieve some of his laudable goals.

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POLITICAL
THEORY

A Weber-Marx Dialogue. Edited by Robert J. Antonio and Ronald M. Glassman. (Lawrence: University Press of Kansas, 1985. Pp. xxi + 334. \$29.95.)

Any book entitled *A Weber-Marx Dialogue* is striking in two respects. First, dialogue is not a word that comes to mind when thinking of Marxist-Weberian relations. Second, Weber's name precedes Marx's in the title. These two things tell us much about the authors of this collection of essays: they are not Marxists. With Marxists, the word *critique* invariably precedes Weber's name, and Marx's name invariably comes first. After all (and this is the conventional wisdom, and not only the Marxist view), Weber was Marx's epigone, a thinker who defined himself over and against his better. Marx was a turning point in intellectual history; Weber an interesting, but in a way inevitable, twist in the road.

The purpose of this collection appears to be, therefore, two-fold: first, to elevate Weber, the thinker, to the level of historical significance occupied by Marx, and second, to end the shouting match by showing that Weber was not merely, or even primarily, Marx's antagonist. Hence, this is not intended as a dialogue in the sense that Marxists are chatting with Weberians, but rather in the sense that non-Marxists are showing the bases on which such a dialogue is possible and desirable. Thus, we have a very complicated maneuver underway here—a prolegomena to any future dialogue, not the dialogue itself.

It is, of course, extremely dangerous to make generalizations about any book containing essays by 13 authors as diverse as Guenther Roth, Wolfgang Mommsen, and Stephen Turner. Each, obviously, has his own agenda, and all have different interests, but all of the essays are bound together by certain recurrent themes. Among the issues addressed are the problem of determination in Marx and Weber; the idealist/materialist controversy; and capitalism, socialism, and their relations to bureaucratization—themes one would expect. What makes these essays interesting is that the

authors' responses to these problems are not altogether expected. Steven Turner argues that Weber did not reject Marx's categories or methods altogether, but that "the elements of the Marxian account are retained, as members of the supporting cast in a story Weber regarded as more significant. . ." (p. 185). Working from the other direction, Douglas Kellner argues that Weber had great influence on twentieth-century Marxists—particularly the Frankfurt School—that, far from being acknowledged, was hidden behind a mass of anti-Weberian polemics. Franco Ferrarotti goes so far as to argue that to regard Weber's *Spirit of Capitalism* as an anti-Marxist work distorts it greatly.

We see in these three essays an underlying unity: all argue that the Marx-Weber distinction is not so obvious as one might think, and therefore, that a unified or at least commingled sociology is in fact possible. At the very least, discourse between the two sociologies is not made impossible by any methodological bars. Why, then, has this not happened? Some of the culprits are named in Kellner's essay, and in Johannes Weiss's fascinating survey of Eastern European literature on Weber. Weiss describes the way in which Marxists, with the interesting exception of the Poles, have either ignored Weber or savaged him, accusing him of neo-Kantianism, subjectivism, and moral agnosticism. Thus, the argument emerges that it has been the Marxists that have made dialogue impossible—in the West because of the Frankfurt School's covert and willfully unacknowledged borrowings, and in the East, because of the doctrinaire rigidity of Leninism.

The theme of possible unity is continued in an interesting way in Ira J. Cohen's essay, which criticizes both Marx and Weber for the same failing. Neither, according to Cohen, appreciated the vigor and validity of democracy, but both, for different reasons, tended toward anti-democratic sentiments. Thus, the failure of modern social science to develop a coherent sociology of modern democratic society is laid equally at the doors of Marxism and Weberianism; this is the unity of failure.

Stephen L. Esquith, in the book's final essay, tries to put all this into perspective by arguing that there was a powerful general unity between the works of Marx and Weber, but that, in spite of some general agreements, it would be a mistake "to conclude that . . . all their political differences have been exaggerated." (p. 314) It is not surprising that there were profound methodological similarities in Marx and Weber. After all, both were brilliant pupils of Hegel, working within his developmental paradigm (a point made by Scaff and Arnold). Yet, whatever their methodological affinities, it is too easy to forget the enormous political difference between the two.

This should give us pause about any Weber-Marx dialogue. Whatever Weber's politics were, they were not the politics of a revolutionary socialist. Whatever methodological similarities Weber and Marx might have had, we cannot but believe that their political choices were more important than their methodological choices. We social scientists, in our self-absorption, frequently forget the relative importance of method and politics. Previous literature on this question focused obsessively on methodological questions, and ignored the political. To a great extent, this collection of essays does not suffer from this defect, and for that it is to be praised. While trying to show us a methodological convergence, the authors do not use this insight to lay claim to a new theory of political convergence.

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Making Sense of Marx. By Jon Elster. (Cambridge: Cambridge University Press, 1985. Pp. xv + 556. \$49.50, cloth; \$15.95, paper.)

What of Marx remains valid today? This separation of the chaff from the wheat is the goal that Elster has set for himself in this new book. The task is not an easy one: it requires, besides mastery of Marx's voluminous production, knowledge of the most up-to-date theories concerning all the phenomena that Marx examined, which range from philosophy to sociology, through economics, political science, and history. Elster's achievement on each one of these fronts is remarkable.

Making Sense of Marx comprises a method-

ological introduction, two substantive parts, and a conclusion in which Elster provides a critical evaluation of Marx's work. The first part deals with issues relevant to philosophy and economics. Marx's views on human nature, social relations, alienation, the philosophy of history, exploitation, freedom, coercion, and justice are presented and critically discussed, together with his theories about technical change, the labor theory of value, the falling rate of profit, and capitalist crises. The most valid of Marx's views today, Elster argues, are those concerning technical change and exploitation. I will not summarize Elster's points on technical change (he has written a whole book on the subject). With respect to exploitation, he argues that the concept not only provides the grounds for normative criticism of capitalism, but also constitutes an explanation for social action. On the other hand, the labor theory of value has a congenital and fatal flaw: it assumes homogeneous labour.

The second part focuses on Marx's theory of economic history and the political issues of class struggle, the state, and ideologies. In the chapter concerning the Marxian theory of economic history, Elster examines the relationship between forces and relations of production and Cohen's argument about the centrality of functional explanation in Marx.

In the next chapter he examines the other candidate for the motive force of history: class struggle. Elster examines Marxian texts very closely in order to reconstruct a definition of class, then departs from them in order to examine what he considers the most important problem in the social sciences: why people cooperate. (p. 366) The question is raised from the concept of *class consciousness*, which Elster defines as the possibility of overcoming the free-rider problem. In the last section of the chapter, Elster focuses mainly on the political writings of Marx and examines the questions of class struggle and class coalitions, which he considers important Marxian contributions.

The last two chapters of this part of the book examine questions concerning two important aspects of class societies: the state and ideologies. What is the relationship of the state and ideologies to the dominant class? Is the state the instrument of the dominant class, or is it autonomous? Is the dominant ideology the ideology of the dominant class? The last ques-

tion provides an additional opportunity for further development of issues relevant to the nature of explanation in the social sciences and of endogenous belief formation.

So far I have summarized the book's systematic presentation and criticism of Marx's thoughts. It would be wrong to conclude from such a summary that the book is simply one more approach to Marx's contributions. If Elster can be criticized, it is not because he has been too servile to Marx, but because he holds strongly his own beliefs and tries to see Marx through them.

In fact, the book is original in several ways. It is concerned not with textual exegesis only (although it does not hesitate to expand in this direction when it is required), but with the internal coherence of Marx's theories, and with the comparison between those theories and reality. It does not deal with the ideas of other writers belonging to the Marxist tradition, Engels, Lenin, Trotsky, Luxemburg, and Gramsci are absent from the book. Instead, references to Cohen and Roemer (the other two editors of the *Studies in Marxism and Social Theory* series) are very frequent. Another original aspect is the author's attempt to strike a delicate balance between defending and criticizing Marx's arguments. No doubt the mix will not please everybody. Questions like, "Then what remains out of Marx?" or "Then why refer to Marx at all?" will be heard from one or the other end of the ideological spectrum. Finally, the book attempts a very interesting synthesis between Marx's thought and rational actor approaches in the social sciences. For all these reasons, the reader has a feeling of participation in the first steps of the creation of a new subfield: the intersection between Marxism and rational choice.

Let us analyze this point. Elster is a methodological individualist (a principle rejected by many Marxists). He believes that social phenomena are in principle explicable in terms of the properties, goals, and beliefs of individuals. Where explanations based on microfoundations are not possible (as in the problem of collective action) a black-box explanation may be temporarily necessary, but "methodological collectivism can never be a desideratum" (p. 6).

How do these methodological lenses apply to the work of Marx? Elster rejects Marxian functional explanations because to be valid, a

feedback causal loop from the consequences to the causes has to be specified. In the absence of such a loop, explanations are either incomplete or false. This kind of "teleological" reasoning is very characteristic of Marx's philosophy of history, the theory of development of productive forces, and the theory of the political and ideological superstructures.

Based on this reasoning, Elster criticizes large parts of Marx's work as being either uninteresting or wrong. What remains from such a radical criticism? Elster argues that Marx has offered a fertile framework for analysis of social phenomena. It is a three-tiered scheme: first, there is a causal explanation of desires and beliefs of the actors; second, there is an intentional explanation of actions in terms of these desires and beliefs; third, there is a causal explanation of the aggregate outcome of these actions (note that the outcome might not have been intended by the actors). This third part of the explanation was pioneered by Marx, and it has become so standard in the social sciences that "few would think of referring to it as 'the Marxist method'" (p. 3).

According to Elster, the second part of the explanation—the intentional part—was not very well developed by Marx, whose analyses either do not include strategic interactions between actors, or are pre-game theory in nature. The reason is that Marx was relying very much on functional explanations. However, Elster argues, there are analyses of specific situations (in "The Eighteenth Brumaire of Louis Nardeon") or whole books (*The German Ideology*) written on methodological, individualistic premises.

Finally, the first part of causal explanations, which Elster calls "sub-intentional," was also developed by Marx. Again, the Marxist theory of ideologies lacks microfoundations, but in Marxian economic analyses, different agents get false beliefs because they generalize from locally valid views that hold only with a *ceteris paribus* clause. For Elster, this fallacy of composition "is perhaps the most powerful part of the Marxist methodology" (p. 19). An example is the falling tendency of the rate of profit in capitalism. Each capitalist can improve his situation by introducing labor-saving innovations, but if all capitalists introduce labor-saving innovations, the rate of profit falls. It seems to me that the situation can be accurately described as a Prisoners' Dilemma game be-

tween capitalists. Elster, however, develops the argument of endogenous belief formation, and shows instances of this phenomenon throughout the book.

In conclusion, *Making Sense of Marx* deals with Marxism and rational choice. As surprising as it may seem with respect to the intellectual history of the two research programs, it seems reasonable after second thought. After all, Marx himself was a rationalist and an English political economist, as well as a Hegelian and a functionalist sociologist. Elster's book makes the point that there is no intrinsic reason for the two traditions to be so far away from each other. On the contrary, they have much to gain from a mutual osmosis of their research agendas. *Making Sense of Marx* addresses itself not just to the intersection of people interested in Marx and in rational choice, but to their union, and to people who like to read books full of interesting ideas.

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Representation and Responsibility: Exploring Legislative Ethics. Edited by Bruce Jennings and Daniel Callahan. (New York: Plenum Press, 1985. Pp. xvi + 332. \$39.50.)

Empirical political scientists who study legislatures have been hesitant to address questions of legislative ethics and morality head-on. Journalists, reformers, and an occasional political theorist have provided most explorations of the topic. Ethics has been avoided, both because political scientists have had bad experiences when they have ventured into advocating reform and because treatments of ethics have frequently concentrated on very narrow questions. *Representation and Responsibility* is an important volume that attempts to move the analysis of ethics to a level broader than a focus on criminality, and it does so in a way that self-consciously attempts to wed the empirical and the normative.

Representation and Responsibility is a collection of essays developed for a series of meetings at the Hastings Center in Ethics in 1982 and 1983. These papers set out

to explore the broad range of ethical responsibilities that arise in legislative life and in the

practice of political representation and to place those responsibilities in the context of recent changes in the electoral system and the legislative environment on both the federal and the state level. (p. xiii)

As with most collections, the usefulness of specific contributions varies considerably. On the whole, the collection is at its best when individual essays link questions of ethics to larger issues of representation. Not all of the essays make the link successfully, however. It is especially troublesome that the authors of the empirical and practical chapters talk past the authors of the theoretical essays on representation and ethics.

The most helpful theoretical essay is "The Theory of Legislative Ethics," by Amy Gutmann and Dennis Thompson. They develop an analytical framework for discussing ethics by starting where they should: with representation. Theories of ethics, as with theories of representation, are "essentially contestable." Gutmann and Thompson aid our understanding by describing an analytical framework that stands up regardless of one's representational standard:

Legislative ethics cannot tell the representative whether to act as a trustee or delegate. . . . It follows that legislative ethics will grant considerable discretion to legislators in their choices of role and their decisions on policy. . . . The primary focus of legislative ethics thus becomes the legislative process itself, and the chief ethical duties of legislators concern making legislative discretion conform to political democracy. (pp. 173-74)

They argue that ethical behavior can be discussed in terms of autonomy, accountability, and responsibility. That is, legislative activity is said to be ethical to the extent that improper influences are minimized and information about legislative behavior is maximized, and that corporate responsibility for the state of the legislature is also maximized. One can argue over what a "proper" legislative influence is, for instance, but Gutmann and Thompson at least define the salient characteristics of ethical systems and provide a starting point and common standard for contention.

Few of the empirical essays on actual legislative behavior rely on a framework as fine as the one Gutmann and Thompson provide. This probably betrays the limited scope of the

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legislative research of the last four decades, during which normative questions have been eschewed. Roger Davidson, in his enlightening essay, "Socialization and Ethics in Congress," notes that norms, the most normative concept of legislative research, "did not . . . address larger questions of ethical behavior for elected officials" (p. 117). Whether such ethical norms were simply lacking, were assumed, or were overlooked by political scientists remains unclear. Yet in this volume, the absence of information on ethical norms hinders the empirical and practical writers from engaging fully in the theoretical debate.

Gary Jacobson's essay, "Political Action Committees, Electoral Politics, and Congressional Ethics," demonstrates the keenest coupling of a larger theoretical understanding of ethics with the actual behavior of American legislators. Jacobson's comments also prove to be the most controversial of the volume. Analytically, Jacobson complements Gutmann and Thompson by recognizing that "the most important ethical problems [political action committees] raise are systemic" (p. 42). With Jacobson, as with Gutmann and Thompson, the question of campaign giving turns not on whether money creates an influence, but on the type of influence it creates. If the present system is unethical, it is because narrow particular interests have improper influence. As Jacobson points out, certain proposed campaign finance reforms would only shift the locus of improper influence (e.g., from special interest groups to incumbent politicians). This leads him to reject most common proposals for campaign finance reform and to note that greater responsibility would only come by strengthening parties—a meager hope.

Representation and Responsibility strikes out in some fruitful directions toward the systematic study of legislative ethics. Those hoping for a unified look at the subject, however, will need to look elsewhere. Those hoping to take up the research agenda on legislative ethics will find this a good place to start.

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The Flight From Ambiguity: Essays in Social and Cultural Theory. By Donald N. Levine. (Chicago: University of Chicago Press, 1985. Pp. x + 248. \$25.00.)

Professor Levine feels strongly that, for the sake of progress in the social sciences, not to mention the health of humanity, a balance need be struck between precision and ambiguity. He is concerned that our modern existence, having fallen under the spell of Hobbes and Condorcet, lends ever-diminishing value to ambiguous thought and language. His book is an attempt to trace the pitfalls of this evolution, and to stimulate a new tolerance for ambiguity. In his own discipline, sociology, his hero is Georg Simmel, while his antagonist is Emile Durkheim.

Levine became disillusioned with the unambiguous commitment to straightforwardness early in his career. In the 1950s, his lengthy study of the Amhara people of Ethiopia engendered an appreciation of the beauty, complexity, and multivocality of their mode of communication, especially when it came in the form of the enigmatic poetry called "wax and gold." In one of his opening chapters, Levine discusses the multidimensional character of these poems, and argues effectively that the sophisticated, subjective interpretations to which they are open foster a personal autonomy missing in our technological environment. For the sake of "clarity and precision," modernity imposes simple, conformist, tyrannical forms of communication on its constituents, and thus erodes the personal universe.

Levine admits that ambiguity can be an excuse for muddled thinking, or an obfuscation of exploitative motivations, but he also argues that wax and gold has its place in the social sciences, despite the efforts of those like Durkheim to eliminate it. Levine argues that ambiguity must be maintained in the scientific study of society because concepts need to be appreciated for their multiple meanings in order to be understood properly. Here is where Levine discusses theories on the relationship of rationality and freedom in modern society. He highlights Simmel and Toennies as proponents of the argument that increased rationality leads to increased freedom, but also sees them as pioneers in multiplying the meanings of rationality and freedom. He is especially taken by

their distinction between objective and subjective forms of rationality.

Levine's discussion turns to Weber, who more than any other commentator, appreciated and refined the multiple meanings of rationality (if not freedom). Not everyone has understood Weber's complexity, however, because many are attracted to his more blatant statements about the diametrical opposition of rationality and freedom. According to Levine, it is not rationality *per se* that Weber sees as erosive of freedom, but only some manifestations of rationality—particularly the "objective" rationalities in the spheres of economics and politics. Levine, claiming to understand the more complicated (ambiguous) position, argues that Weber recognizes certain kinds of rationality that lead to certain kinds of freedom. For instance, when freedom is divided into "situational" freedom, which involves "external constraints on the movements of actors," and "autonomy," "where actors choose their own ends of action" (p. 165), it can be seen that modern capitalist economies increase situational freedom by using "an open labor market where workers are formally free to sell or withhold their services" (p. 166). Likewise, when rationality is divided into that which is concerned with instrumentalities and that which considers "ends," one sees that a good deal of freedom exists in choosing one of "a variety of technically suitable courses of action, even when a particular end is specified" (p. 168). The obvious problem here, which is not adequately addressed by Levine, is that situational freedom and autonomy are not equally valuable or basic; the same can be said for instrumental rationality and the choosing of ends. Having the "freedom" to choose among an ever-growing number of options while being constrained by a predetermined and non-negotiable end seems to be the same kind of freedom allowed Japanese auto workers when they decide whether to put bumpers or headlamps on new Toyotas. In the end, Weber's concepts of freedom and rationality may not be as ambiguous as Levine would like them to be.

In any case, if we accept that Weber has been simplified and misunderstood regarding the equation of rationality with the curtailing of freedom, it is not all the fault of Weber scholars. Weber himself seems to have resisted the ambiguities Levine discovers in him. There

were "idiosyncrasies of his own temperament" (p. 170) that injected undue pessimism into what might have been a more neutral analysis. In a partly biographical chapter on Weber and Freud, Levine argues that "protracted emotional crises" (p. 186) drove the two contemporaries "to exaggerate the extent of repressiveness in modern culture" (p. 197), limiting their ability to articulate and explore their ideas fully. Levine also discusses the intellectual parochialism under which Weber and Freud (and Durkheim) suffered, contracted during their efforts to demarcate independent scholarly discipline. Simmel, on the other hand, who had no interest in acolytes, or academic entrepreneurship, was not so shackled.

Levine assembles his advantages for a concluding synthesis. He has the example of Simmel to strengthen his tolerance for ambiguity. He represents a discipline now firmly entrenched in academia and not in need of intellectually limiting defenses. He does not seem to be undergoing a debilitating emotional crisis, and he has, since Simmel, a multitude of ever-more complicated and specialized treatments of rationality to go on (especially that of Weber). Mustering these forces, Levine "disambiguates" the concept of rationality by articulating and relating its various meanings. He does not claim to have solved the problem of understanding rationality; he only claims that his appreciation of the concept's ambiguity has allowed him to explain the concept better, and prepare it for yet more ambiguous manifestations.

This is not a good book. In very facile ways it links personal "crises" to scholarly positions (concerning Freud, Weber, and Durkheim especially), and thus degrades the independent value of the analyses. It is an eclectic book, bordering on dilettantism. Kant and Hegel are portrayed very simply, jammed between poorly connected discussions of Ethiopia and anomie. The book lauds ambiguity, yet is packed with charts and classification systems, and it criticizes the univocality of modern science without appreciating modern treatments that recognize the scientific engagement as very complex and multivocal. Furthermore, Levine can hide behind his own confusing and disjointed argument by claiming it too represents the scarce commodity: "Surely it is appropriate for a book on ambiguity to harbor

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a multiplicity of meanings" (p. x).

Yet this is a very good book. It isolates the pitfalls of intellectual specialization and stimulates cross-disciplinary considerations; indeed, the book itself is refreshing in its employment of a variety of disciplinary perspectives. It is a balanced book in that, despite its appreciation of ambiguity, there is an attempt to "disambiguate" concepts—to synthesize ambiguous representations and make sense out of them. The book chastises those who would read and understand the classic literature of sociology simply, and it effectively shows the often forgotten elegance of writers like Georg Simmel.

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Aristotle on Equality and Justice: His Political Argument. By W. von Leyden. (New York: St. Martin's Press, 1985. Pp. ix + 145. \$25.00.)

In his preface von Leyden suggests that this, his most recent, book is intended

to contribute to the discussion of the Aristotelian as well as modern questions of (a) how to render the principle of equality, no less than that of inequality, compatible with the idea of fairness, and (b) how to combine the facts of individual as well as social diversity in civil life with the demands for political justice and cohesion. (p. vii)

In an effort to accomplish these goals, the author uses the first half of his book to discuss Aristotle's understanding of equality as the concept is developed in his political, physical, and metaphysical treatises. The second half of the book contains a general report on Aristotle's analysis of revolution and law, and concludes with the author's summary and evaluation.

As readers of Aristotle know, the question of equality is at times raised in the context of a disagreement between the democratic and oligarchic conceptions of distributive justice. According to the democratic argument, one's equality in respect to his or her free birth should be understood to imply an equivalent equality in respect to all things whatsoever, whereas the oligarchs insist that their superiority in one respect—be it wealth or noble birth

—necessarily required a recognition of their superiority in general. Aristotle's "correction" of these two different but equally partial interpretations of justice is his principle of proportionate equality. This principle justified a fair and reasonable inequality of treatment, to the extent that only equals were to be treated equally and unequals unequally. Although von Leyden prefers an Aristotelian theory of proportionate equality, which distinguishes among the worth of various human capacities and achievements, to such an undifferentiated egalitarianism as found in Bentham or Justinian, he nonetheless acknowledges the difficulty of establishing precisely in what respect equals are to be considered equal and unequals unequal. Aristotle argued that the appropriate criterion for differentiating among various functions, and thereby justifying the unequal treatment of citizens in certain cases, is to be found in reference to one's contribution to the common good of society. As some contribute more to the morally good life, so too should they receive proportionately more in return. While accepting this criterion, von Leyden is aware of its difficulties. Although it may not be too difficult to distinguish the level of contribution within a single profession (e.g., to recognize the difference between a good and a poor soldier), it is much more problematic to compare various social vocations that are different in kind (e.g., the contribution of a good farmer as compared to that of a good artisan).

Given the essentially contestable nature of all claims for either equal (democratic) or preferential (oligarchic) treatment, von Leyden argues that there are ultimately only two ways in which such claims finally can be settled: either the resort to revolutionary action or an appeal to law. Accordingly, the final chapter of his book examines Aristotle's treatment of these themes. Von Leyden's discussion of revolution repeats rather faithfully Aristotle's own analysis. The primary task is conceived as an attempt to achieve a proper balance between the interests of the rich and those of the poor, and the solution is presented in terms of establishing the influence of an essentially moderate middle class. Perhaps the most interesting section of this book is found in von Leyden's discussion of Aristotle's theory of law in general and his arguments concerning the natural law in particular. This discussion gathers together several disparate sources and

attempts to present a rather full treatment. It is surprising, however, that the author did not provide a more careful analysis of prudence (*phronesis*) or a more detailed discussion of the nature of the morally mature man (*Spoudaios*). Both concepts are absolutely central to any consideration of Aristotle's understanding of the moral order. In general, von Leyden's book repeats the standard analysis of Aristotle's understanding of equality, revolution, and law. Unfortunately, however, it does not attempt much more than this. Indeed, it is not clear to this reviewer what the purpose of the book actually is. As noted, von Leyden indicates certain intentions in his preface, yet these intentions appear to remain somewhat external to the actual analysis within the text, and fail thereby to unify or structure the argument being made. As a discussion of Aristotle's political theory, von Leyden's book lacks the detail and care found in such commentators as R. G. Mulgan, Ernest Barker, or Günther Bien. As a contribution to the contemporary discussion of equality, law, and revolution, its arguments are underdeveloped and indirect at best.

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The Intellectual Development of Karl Mannheim. By Dolin Loader. (Cambridge: Cambridge University Press, 1985. Pp. ix + 261. \$34.50.)

Karl Mannheim. By David Kettler, Volker Meja, and Nico Stehr. (New York: Ellis Horwood Limited and Tavistock Publications Limited, 1984. Pp. 169. \$11.95, cloth; \$4.95, paper.)

As the authors of *Karl Mannheim* observe, Mannheim never gained a place among the major thinkers within the social sciences. He was an early casualty in the war between positivist and antipositivist social scientists, and "it is only as these simplistic oppositions lose plausibility that the undeniable presence of Mannheim at the starting points for the most compelling questions [of social thought] can be acknowledged" (Kettler, Meja, and Stehr, p. 12). If, as it appears, those battle lines have receded and social science has entered into a period of critical self-assessment, then

perhaps Mannheim's time of recognition has come. For unlike Aristotle, Locke, or Marx, whose answers must be reckoned with even by those who believe these individuals asked the wrong questions, Mannheim's significance as a thinker depends upon a general willingness to accept the importance of asking oneself probing and troublesome questions in a situation where reassuring answers are not forthcoming. It is the "sensibility and a distinctive openness" (Kettler et al., p. 13) in Mannheim's approach to social problems that makes him such an attractive thinker.

Mannheim's style of writing is itself a reflection of this approach. He did not write books with an Aristotelian beginning, middle, and end, leading to an artificial closure of the argument. Rather, all of his writings—including *Ideology and Utopia* and *Man and Society in an Age of Reconstruction*—are essays or collections of essays. This "essayistic-experimental attitude" (Loader, p. 2) allowed Mannheim to explore a particular problem in depth without worrying about how his arguments could be forced into a linear or progressive alignment. Thus, "to trace his thinking requires some patience and a certain tolerance for essayistic experimentation" (Kettler et al., p. 13). Moreover, Mannheim's belief that intellectuals have an obligation to "think through the problems of social life in terms of the tensions between social strata," as he put it in *Ideology and Utopia: An Introduction to the Sociology of Knowledge* (New York: Harcourt, Brace & Co., 1968, pp. 45-6) virtually guaranteed that he would draw fire from partisans on all sides whenever an exploration of these problems led him—as they invariably did—into the thicket of political controversy. This preoccupation with finding some means for overcoming social conflict by thinking it through to its roots necessarily gives a "political cast to [Mannheim's] thought" (Kettler et al., p. 15). Loader advances a somewhat different view of Mannheim, and I will return to this point later.

Mannheim's earliest writings were focused upon the cultural crisis characteristic of modern industrial society. The organic unity of Western culture had dissolved into atomistic fragmentation and, Mannheim believed, the role of the intellectual was to discover the causes of this crisis and to work toward a restoration of cultural unity (Loader, pp.

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10-30). This endeavor might lead one toward a consideration of the rise of science or democracy, or of the socioeconomic causes of conflict, or any of the other issues that comprise the problems that still constitute contemporary social science. Mannheim entered this complex of problems through the door of neo-Kantian philosophy as a critic of the application of positivist methodology to cultural phenomena (Kettler et al., p. 17). Indeed, his discussion of the methodological and epistemological presuppositions underlying an interpretive approach to culture in his essay "On the Interpretation of *Waltanschauung*" (1921) has lost none of its thought-provoking brilliance. Mannheim believed a philosophy of history could supply the values necessary to sustain both this methodology and a cultural synthesis until a deeper examination of the historical and sociological dimensions of culture in the mid-1920s convinced him that the aperture of philosophy provided too narrow a perspective for understanding—let alone resolving—the crisis.

Responding to the dissonant influences of Max Weber, Lukacs, Troeltsch, and Alfred Weber, Mannheim developed a sociology of knowledge approach stressing the role played by social groups in the formation of a particular culture (Kettler et al., pp. 38, 55; Loader, pp. 48ff.). Naturally, this formulation led him toward a consideration of the problem of ideology, which, he believed, lay at "the heart of the problem of political thought in general" (*Ideology and Utopia*, p. 124). Yet, it is precisely in Mannheim's most famous work that the tensions within his thought are most exposed. Not surprisingly, the two books under review differ most sharply in their respective interpretations of *Idology and Utopia*. For Loader, Mannheim is following the model of Max Weber in his attempt to develop a scientific approach to political conflict (pp. 94, 96, 187, 189). As Weber had maintained, the social scientist—including Mannheim—only analyzes social problems; politicians decide what to do about them (Loader, p. 98). *Ideology and Utopia*, Loader argues, is Mannheim's attempt to come to terms not with Lukacs or Marxism, but with Weberian sociology (p. 96). Hence, the discussion of the social role of the intelligentsia in that work ought to be read in the context of Mannheim's application to that group of the scientific ethos of social science as stated by

Weber (pp. 119 ff.).

The authors of *Karl Mannheim*, however, give greater weight than Loader does to the fact that Mannheim employs Marx's terminology and assigns a considerable importance to class conflict as a determinant of value positions in *Ideology and Utopia* (Kettler et al., pp. 58, 92, 101). Moreover, Mannheim's critique of the limitations of bourgeois thought and the role he assigns to party schools seems to echo the influence of Lukacs's *History and Class Consciousness*. No one is suggesting that Mannheim followed Lukacs in his political commitment to Marxism, or that he directly challenged Weber's ethos for social science. What makes an interpretation of Mannheim's position in *Ideology and Utopia* so problematic is precisely the difficulty of determining whether there is some middle ground between these two perspectives.

In any event, Kettler, Meja, and Stehr rightly stress the significance of Mannheim's insistence that ideologies are cognitive structures—i.e., "productive of knowledge" (p. 61)—as a positive contribution to our understanding of political thought. This insight is still very far from gaining universal acceptance within social science. In part, this is due to the failure to appreciate the importance of Mannheim's observation that "ideology" is a distinctly historical phenomenon, as characteristic of Western society as the processes of rationalization or bureaucratization to which Weber drew attention. Even if this were granted, one would still have to demonstrate how very disparate areas of cultural consciousness are organized into an ideology, how the various elements that comprise this world view change differentially over time, and how the presence in society of competing world views relates to both these points. Such an undertaking requires a highly sophisticated concept of ideology, which certainly cannot be reduced to its functional role as a means of advancing the interests of a particular social group. In *Ideology and Utopia*, Mannheim argues that it is unlikely that such a concept of ideology will be formulated unless its historical-sociological status is first recognized, and that since no instrumentalist or positivistic methodology could produce this concept of ideology, the sociology of knowledge must employ both interpretive/phenomenological and causal/scientific methods in its treatment of ideology. How

such a synthesis of methodologies is possible is a question whose legitimacy and thoughtful consideration has been set aside by the long-standing positivist/antipositivist debate within social science, but in the present period of the subsidence of the intensity of this debate, a rereading of *Ideology and Utopia* might prove to be a helpful first step toward supplying an answer to this question.

Like many others of his generation, Mannheim became a refugee from Nazism, settling in England. It seems fair to say that a noticeable decline in the intellectual quality of Mannheim's thought was the price of his adjustment to his new environment. Not that his writings on planning and democracy are devoid of valuable insights; on the contrary, *Man and Society* is, in its own way, a pioneering work, and still forms the starting point for much of the contemporary discussion of democratic planning. Nevertheless, Mannheim's suggestion of the psychoanalyst as the model for his social planner (Kettler et al., pp. 80-81, 147); his uncritical acceptance of the existence of a consensus on democratic values in England and America, and of the ability of parliamentary institutions to resolve all political conflicts in modern society (Kettler et al., pp. 27, 145; Loader, pp. 145, 154, 172); his easily misunderstood use of *manipulation* as a synonym for *planning* (Kettler et al., pp. 81-82; Loader, p. 144); and the missionary zeal he displayed in his quixotic attempts to reform the consciousness of his English contemporaries (Kettler et al., p. 130; Loader, pp. 174ff.) hardly exhibit the best qualities of his mind. As Kettler, Meja, and Stehr observe, Mannheim's failure to continue to ask critical questions concerning the foundations of his own position is the most disappointing feature of his post-1933 writings (p. 148).

Loader is less inclined to accept this view of Mannheim. In providing "a comprehensive interpretation of Mannheim's entire career," and in placing Mannheim in close intellectual proximity to Weber, Loader schematizes Mannheim's intellectual development into a "series of types, stages, and categories" (pp. 3-5, 186). *Intellectual Development* presents a "typological structure, which has been imposed upon the heterogeneity of Mannheim's thought" (Loader, p. 182); that is, the application to Mannheim's thought of Weberian ideal types as one-sided exaggerations

forms part of the argument that Mannheim is, really, a Weberian thinker (Loader, pp. 179-80). Loader's argument is interesting, and does "cast a new light upon [Mannheim's] work" (p. 186). Nevertheless, I do not find his reading of *Ideology and Utopia* convincing. Loader's simplistic formula—that if Mannheim is not Lukacs, he must be Weber (pp. 101ff.)—is hardly helpful, and could easily be reversed. The constant use by Loader of the analytic and the valuative as sharply separable categories of sociological research draws upon a Weberian meaning that is, at times, more a hindrance than an aid to understanding Mannheim's approach. Readers of *Intellectual Development* should see pp. 178-89 for Loader's clarification of his procedure before they approach the text. Not only did Mannheim reject what he called Weber's "extreme nominalism," his reliance upon the individual as the unit of sociological analysis, and the sharpness of his separation of theory from evaluation in his conception of social science methodology, but as Kettler and colleagues observe, he "fundamentally alters the meaning" of the Weberian categories even when he employs them (p. 54).

Although my view of Mannheim is closer to that presented in *Karl Mannheim*, this should not be read as an attack on the plausibility of Loader's argument. A reading of Mannheim from the standpoint of Weber, especially when it is executed with a great deal of sensitivity, is a valuable contribution to the understanding of a thinker whose intellectual allegiances vacillate between Marx and Weber. Both these works are intelligent and sympathetic interpretations of Mannheim's thought, and like him, they raise important and yet unresolved questions concerning the nature of social science that deserve serious consideration.

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Marxism and Morality. By Steven Lukes.
(New York: Oxford University Press, 1985.
Pp. xiii + 163. \$15.95.)

Steven Lukes's book breathes new life into a dying debate: that concerning the relationship between Marxism and morality. Others

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(Cohen, Geras, Gilbert, Husami, Miller, Wood, and Young) have thoroughly explored available philosophical positions, but none from Lukes's practical perspective. Lukes bluntly asks whether Marxist theory accounts for the moral disasters of Marxist practice. He answers yes, and argues that socialists should sever some of their links to Marxist ethics, for Marxism is morally "disabled" and hence incapable of countering immoral measures pursued in its name. This gives the debate on Marxism and morality not only new vitality, but also new urgency.

Lukes begins by arguing that Marxists' views on morality are paradoxical: they attack morality as ideology, yet adopt a higher, human morality (chap. 2). Lukes resolves this paradox by showing how Marxists distinguish a morality of *Recht* from a morality of emancipation. They reject *Recht* (justice and rights) and the conditions requiring it (scarcity and conflict); emancipation involves the transcendence of both. Socialist society is characterized by cooperative abundance and harmonious interests, making *Recht* superfluous (chap. 3). Problems remain after this resolution, however. Lukes illuminates them by examining *Recht* (chap. 4) and emancipation (chap. 5) in depth. He argues that Marx (and his heirs) conceives *Recht* and the conditions necessitating it too narrowly. Rights need not only protect egoism, they can also defend individuals from arbitrary power. Similarly, justice involves more than the unequal treatment of unique individuals by a common standard; it provides general rules of fairness essential to social life. Since conflicting interests (though not class interests) and related questions regarding distribution, organization, and decision making postdate capitalism, Lukes argues that socialism requires *Recht*. Without a morality of *Recht*, Marxists cannot respond to injustices or to violations of rights; they cannot define impermissible means and inappropriate ends. In a final fascinating chapter, Lukes discusses debates over means and ends in the Bolshevik revolution (Luxemburg, Kautsky, Lukacs, Lenin), in the 1930s (Trotsky, Dewey), and in post-liberation France (Sartre, Camus, Merleau-Ponty, Koestler).

Lukes concludes that Marxists' exclusive morality of emancipation has two defects for guiding action. First, it generates illusions by refusing to specify, and claiming to foresee, the

future socialist society. This precludes the clarification of ends by which to judge means, and hence consideration of alternate means. Second, these illusions further Marxists' moral blindness. Failure to clarify what socialism means in practice absolves revolutionaries of moral responsibility and denies them moral direction. It also prevents consideration of the relation between emancipation and *Recht*. Still, Lukes praises Marxists' vision of freedom, which is richer than the liberal view and reveals the ideological functions of *Recht*.

The insightful analysis and incisive questions throughout this book make it valuable for scholars and students of Marxism. As a self-proclaimed exercise in "socialist free-thought," it chastens hostile critics of Marxism and challenges sympathetic ones. Lukes honestly confronts Marxists' moral disasters, arguing convincingly that they cannot afford to abandon *Recht*. But can they afford to adopt it? If socialist ends cannot be achieved by democratic means, are any other means justified by those ends? Lukes suggests not. Only political movements sensitive to *Recht* can create a socialist society worth having. But can they create it? And if so, how? In the end, Lukes only asks, "Can a theory of justice and of rights be developed which incorporates the insight and vision of Marxism's conception of freedom?" (p. 149). I wish he had answered, but that is perhaps unfair. He promises only a diagnosis, not a cure. He challenges others to answer, guided by the clarity and urgency of his questions.

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Marx's Critique of Political Economy: Intellectual Sources and Evolution, Vol. 1, 1844 to 1860; Vol. 2, 1861 to 1863. By Allen Oakley. (Boston: Routledge & Kegan Paul, 1984. Pp. 225. Vol. 1, \$30.00; Pp. 302, Vol. 2, \$32.50.)

In his recent, comprehensive, two-volume study, Allen Oakley provides a masterful examination of Marx's critique of political economy. The first volume devotes roughly equal attention to the works of the 1840s and 1850s; that is, to Marx's "early" and "middle" writings, from the *Paris Manuscripts* to the

Grundrisse and *Contributions to the Critique of Political Economy*. The second volume is devoted exclusively to that portion of Marx's critical manuscripts of the early 1860s published under the title *Theories of Surplus Value*.

Karl Marx's views and works on political economy prior to *Capital* have been a major source of commentary and controversy for several decades. Publication of Marx's 1844 *Paris Manuscripts* in the 1930s and its subsequent translation into English in the 1950s stimulated debate concerning the position of these manuscripts—notably Marx's critique of the phenomenon of alienation—in his life works. It was common in the 1950s and 1960s to postulate a tension or "break" between Marx's early writings of the 1840s and his more scientific analyses of capitalism's "law of motion," presented in *Capital*. The case for continuity was boosted by the English translation, in the 1970s, of Marx's massive unpublished notebooks of 1857–58, *Grundrisse*. This work, while continuing such early themes as alienation, linked them with Marx's emerging ideas on capital, value, and exploitation, subsequently published in more fully developed form as *Capital*.

On this controversy, Oakley takes a prudent middle ground. Marx's critical theory, he states, comprised a "complex mixture of continuity and discontinuity because of its organic and dialectical nature" (vol. 1, p. 2). Sifting out shifts in emphasis within elements of continuity can only be done by carefully studying Marx's life works as a whole. Reading *Capital* in isolation and out of bibliographic context provides a limited and misleading understanding of Marx's thought.

Oakley's chapter on Marx's 1844 *Manuscripts* usefully illustrates this view. While not neglecting alienation, the typical centerpiece of expositions of this work, Oakley deftly shows how this concept also provides (although in embryonic form and without the benefit of the mature Marx's theory of value) strategic elements of Marx's subsequently developed category of exploitation. Thus, Oakley shows that Marx's concept, if not his theoretical explanation, of exploitation has its intellectual roots in his early writings of the 1840s.

Another prominent debate surrounding Marx's early writings concerns the influence on Marx of his predecessors in classical political

economy, notably Ricardo. One familiar tradition in Marx scholarship emphasizes the impact on Marx of the classical political economists, and the continuity between them. Another, more recent, school of thought challenges this interpretation, and focuses on Marx's special, essentially non-Ricardian contribution.

On this issue, also, Oakley charts a judicious course between the extremes of considering Marx a mere "minor post-Ricardian" and asserting a fundamental dichotomy between classical and Marxian perspectives. Marx relied "very heavily" on classical political economy in constructing his own critical theory of capitalism, but "he went to great lengths in order to differentiate and distance his critical theory from the work of his antecedents" (vol. 1, pp. ix–x). As Oakley explains, this differentiation proceeded through two main stages. In Marx's writings of the 1840s, he essentially accepted classical political economy as a "faithful abstract reflection" of the actual capitalist society (vol. 1, p. 131). Increasingly, however, Marx came to the conclusion that classical political economy failed to appreciate fully the tensions and contradictions behind the apparent liberty and equality of capitalism's market exchange processes and regime of free contract. Consequently, he shifted from a critique of capitalism via classical political economy to a critique of the limitations of classical political economy as well. This, in turn, required formulation of his own critical alternative analysis of capitalism, culminating in *Grundrisse*.

Although Oakley's careful and insightful study sheds valuable light on these and other debates, his stated intent is the more modest one of expositional clarity. That he achieves more than this in the process is a pleasant bonus. He certainly succeeds in his stated task of clear exposition of the intellectual influences on and evolution of the "critico-theoretical core" of Marx's thought.

Oakley's equally painstaking explication (in vol. 2) of the manuscripts subsequently published as *Theories of Surplus Value* is especially valuable because these manuscripts have been relatively neglected by Marx scholars, and because they constitute Marx's clearest and fullest attempt to differentiate and distance his work from that of his predecessors. As in his earlier writings, Marx thrived on

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critical analysis and evaluation of others' ideas. Through critique of his predecessors' views of surplus value, Marx engaged in "self-clarification," and thereby advanced the sophistication "of his own critical theory of how capitalism functioned" (vol. 2, p. 6). Thus, *Theories of Surplus Value* is both more and less than ruminations on the history of political economy in the traditional sense: more, because he used critique as a means to advance his own thinking; less, because the scope of his manuscripts was narrowly proscribed by his own particular interest—surplus value—and his own particular criterion for judging the quality of his antecedents' works, namely, the labor theory of value.

Apart from the introductory and concluding chapters, Oakley organizes his discussion around two overarching themes: the key issue of value and distribution theory, and the high theme of capitalism's "law of motion."

As for the first of these two topics, the fundamental dividing line, of course, is Marx's assessment of the contributions of David Ricardo, whom Marx clearly regarded as both the classic representative and apogee of classical political economy. All other views of exchange value and distribution are either "pre-" or "post-Ricardian."

Oakley provides both order and insight in his summaries of Marx's interpretations of surplus value in the writings of his predecessors. From Marx's perspective, mercantilist writers explained economic surpluses (of production above necessary consumption of the working population) as derived purely from exchange; that is, from the sale of commodities above their values (vol. 2, p. 33). Physiocrats such as Quesnay located—quite properly in Marx's view—the origin of surpluses in production rather than exchange, but restricted their argument to the physical realm in general and to agriculture in particular (vol. 2, pp. 37–38). Adam Smith sharpened the analysis by generalizing from the creation of surplus to production as a whole, attributing a special role to labor, and interpreting surplus in terms of value, rather than merely in physical terms. However, he muddied the waters by presenting a conflicting view, in which "natural prices" are themselves nothing more than the summation of wages, rents, and profits at their "natural rates" (vol. 2, chap. 3). According to Marx, Ricardo's great achievement (relative to

Smith) was to penetrate to capitalism's "inner," more fundamental, relationships through a labor-embodied theory of value. Yet even Ricardo failed to articulate fully the relationships between value and distribution, by not clearly differentiating between fixed and variable capital, surpluses and profits (and thereby between the rate of surplus value and the rate of profits), and labor and "labor power," and by his ingenious—but from Marx's standpoint digressionary—focus on land rent (vol. 2, chaps. 4–5). After Ricardo, Marx's assessment of the quality of political economy (and his patience) decreased. Still, Oakley helpfully guides his readers through four chapters of commentary on Ricardo's seminal work.

In a final section, Oakley turns to more "dynamic" aspects of *Theories of Surplus Value*. These chapters also provide him an opportunity to elucidate Marx's own emerging analyses of capitalism's "law of motion." Rather quickly (too much so for this reviewer's taste), Oakley traverses reproduction schemes—both his own and Quesnay's—(chap. 10), capital accumulation and the falling rate of profit (chap. 11), and crises (chap. 12). The last of these three discussions, although abbreviated, is the most interesting. Here, Oakley comments insightfully, in rapid succession, on Marx's critique of Say's Law (that is, the proposition that the production of output generates income, which thereby constitutes sufficient demand for the output, thus circumventing crises and depressions); partial and general overproduction; Ricardo, James and John Stuart Mill, Malthus, and Sismondi; and the crucial distinction between the possibility and causation of crises.

Oakley's study is an important, needed, and unique contribution to Marx scholarship. He creates order and clarity out of three massive volumes of Marx's posthumously published manuscripts. He shows how Marx differentiated his own work from that of his predecessors, clarified and advanced his own thinking, and thereby prepared the way for embarking on the critical investigations he drafted in *Grundrisse* and subsequently published in *Capital*. Although obviously sympathetic to Marx's project, Oakley carefully notes limitations and inconsistencies in the argument. The final section, on dynamic elements in Marx's thought, could easily have been doubled in

size, and suffers by comparison with the fuller and more leisurely examination of theories of value and distribution. All in all, however, Oakley's explication of Marx's *Theories of Surplus Value* is a valuable contribution and a most useful guide to a neglected and strategic part of Marx's voluminous writings.

JOHN E. ELLIOTT

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Disorganized Capitalism: Contemporary Transformations of Work and Politics. By Claus Offe. Edited by John Keane. (Cambridge: The MIT Press, 1985. Pp. 366. \$12.50.)

Claus Offe has established himself as one of the most important contemporary thinkers associated with critical theory and the Frankfurt School. This volume contains reprints, some newly revised, of ten of Offe's recent essays on the contradictions of the contemporary capitalist states. Despite their various subject matters, each of these essays poses a challenge to the notion of "organized capitalism." This idea, first propounded by Rudolf Hilferding in 1910 and developed in the work of German democratic-socialist theorists in the 1920s, notes the passing of classical market capitalism and its replacement with "formally organized collectivities of economic action (corporate firms, cartels) and interest representation (trade unions, business associations)" (p. 6). Instead of resolving the problems of classical capitalism, Offe analyzes the inherent tensions and contradictions that plague these corporatist-collectivist structures of late capitalism.

The first five essays ("The Political Economy of the Labour Market," "The Future of the Labour Market," "Three Perspectives on the Problem of Unemployment," "The Growth of the Service Sector," and "Work: The Key Sociological Category?") focus on recent developments in the sociopolitical status of labor in late capitalism. Throughout these essays, Offe notes the problems of treating labor in terms of "the labour market." This notion of the market misperceives labor as just one commodity among many others. The second five essays ("Interest Diversity and Trade Union Unity," "Two Logics of Collec-

tive Action," "The Attribution of Public Status to Interest Groups," "Legitimation Through Majority Rule?" and "The Divergent Rationalities of Administrative Action") explore the failures and inadequacies of the mediating links between social power and political authority. In the most general terms, the politicization of social and economic structures does not resolve the problems of classical capitalism, but merely transfers them to the institutions of the state.

Like most of his theoretical work, Offe's *Disorganized Capitalism* is poised between Marxist and Weberian perspectives. The emphasis on contradictions and inherent tensions clearly reflects the Marxist tradition of the critique of existing theories and practices. Furthermore, Offe attests that his work has an explicitly political, delegitimizing goal:

If these assumptions [about the integrating tendencies of welfare-capitalist democracies] become implausible when exposed to a reality test, this will in turn contribute, . . . to the disorganization of not only our mode of thinking about modern capitalism but also, and consequently, to the political disorganization of the patterns of organization of welfare-capitalist democracies themselves. (p. 9)

However, no facile conclusions about the imminent or automatic collapse of late capitalism should be drawn from Offe's critique. Following Weber, Offe contends that the contradictions and inherent tensions of late capitalism may continue to be managed, if not resolved, within the parameters of the existing capitalist state.

Offe's writing style is forbidding. These essays are highly abstract and complex theoretical exercises. The relatively few empirical-historical references provide precious moments of relief from this theoretical assault. Yet Offe's work continues to take its proper place in the first rank of research on the modern state. A "Chronological Bibliography of Claus Offe's Work" is appended to this volume.

JOHN BOKINA

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Giving Desert Its Due: Social Justice and Legal Theory. By Wojciech Sadurski. (Dordrecht, Holland: D. Reidel Publishing Company, 1985. Pp. xvii + 329. \$47.00.)

It is impossible in this brief space to give this deserving book its due. Ranging over topics such as legal justice, ethical justification, the proper distribution of wealth, respect for human needs, preferential treatment, and the ethical basis of punishment, Sadurski attempts to demonstrate that essentially one principle of justice can and should be applied in the numerous settings in which questions of justice arise. The unifying principle is that social arrangements should aim toward an "equilibrium" of the benefits and burdens experienced by individuals as they strive to implement their life plans. Sadurski's idea is not to equate benefit and burden, as is done in interpretations of punishment as a restoration of the statue quo ante. Rather, he wants, as he often puts it, for society to "balance" benefits and burdens so that one truly gets what one deserves. This emphasis on receiving the proper rewards for one's efforts introduces a dynamic element to this conception that, perhaps, justifies the additional connotations of the notion of equilibrium.

The substantive breadth of this book is complemented by a certain narrowness in its definition of justice. For Sadurski, justice basically concerns distributive issues only. For example, he does not consider the aggregate amount of freedom in a society to be, in itself, an issue of justice. On the positive side, this narrowness promotes conceptual clarity and the consistent application of a coherent conception of justice across its many spheres (to borrow Michael Walzer's phrase). On the negative side, one is left in the dark, or at least the twilight, about how to trade off justice with, say, freedom or utility.

There are some additional areas that need clarification. Sadurski, for example, proposes that balancing should occur between the social benefits a person produces and the burdens experienced in producing them. Specifically, people should not be rewarded for their personal attributes, but only for the burdens they encounter in using them. Yet the notion of burdens employed by Sadurski is ambivalent. When basic human needs go unsatisfied, this counts as a burden even though it is only

counterfactual—that is, of the form "so-and-so could have done X if he or she were not in need of Y." No actual effort need be expended. Once basic human needs are satisfied, however, burdens, as I understand Sadurski, are those encountered through actual effort (p. 116). In this labor theory of desert, advantages due to undeserved resources are negated, but potential that remains untested due to non-basic burdens is apparently undeserving. Sadurski's heart is clearly with the latter restriction on the notion of burden, but he recognizes a requirement to satisfy basic needs so as to put individuals in a position to produce socially valued benefits. It is not enough, though, to postulate that deprivations of the opportunity to produce would simply be unjust. Injustice, after all, is what we are trying to define.

It is interesting in this respect that Sadurski argues that the allocation of jobs need not conform to the basic principle of justice (pp. 153–56). Jobs should be awarded on the basis of merit and not actual past sacrifice. Here we see a very active and narrowly construed notion of burden and a curiously uncritical acceptance of the ideas by which we have supposedly chosen to allocate jobs (this from an advocate of preferential treatment). He also suggests that a job is not really an unalloyed benefit, but encompasses burdens as well. It is arguable, however, that in a free market, a job is more of a benefit than a burden.

With regard to the general notion of benefits, there is a possible incompleteness in this theory of justice, even taken on its own terms. In rewarding people for their socially beneficial efforts, we need to rank different kinds of effort and compare them with standardized measures of reward (p. 143). Those very comparisons are, in turn, typically institutionalized through markets and other social or political arrangements (see pp. 127–28). Arguably, they must be institutionalized in some sense to become socially meaningful. Can Sadurski rest with his admonition to balance burdens and benefits, or must he assess the alternative institutional mechanisms in which these elements are first defined? Is he thus led back to the idea that the basic structure of society is at least part of the subject of justice? This book provokes and merits questions such as these.

ROBERT GRAFSTEIN

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Maurice Merleau-Ponty: Between Phenomenology and Structuralism. By James Schmidt. (New York: St. Martin's Press, 1985. Pp. x + 214. \$30.00, cloth; \$10.95, paper.)

The world of political philosophy today is divided into a number of subsets (at times appearing to be armed camps). When a reader sitting in one subset meets a book from another, the result is often bewilderment—perhaps even hostility—followed, one hopes, by an attempt to understand and come to terms with a different vocabulary and unfamiliar concepts before returning to the comfort of familiar surroundings. This is to say, a specialist in contemporary continental—specifically French—political philosophy, which I am not, would read the book under review differently than I did.

For the nonspecialist who is somewhat familiar with the vocabulary and concepts involved but does not live with them every day, this is a valuable book. James Schmidt takes the reader step-by-step through Maurice Merleau-Ponty's intellectual life, from his earliest writings to his last. His intellectual evolution and his periodic revolutions in stance are recounted fully and fairly. Anyone wanting a reasonably sophisticated introduction to Merleau-Ponty will find one here.

The problem, which appears at times to bother Schmidt, is whether anyone but a specialist should care. The first paragraph of the book gives a number of very good reasons why we should care:

By the time of his death, Merleau-Ponty was regarded as 'the greatest of French phenomenologists'; yet his last writings questioned the very coherence of Husserl's project. He built a case for Marx's philosophy of history which is among the most subtle in the Marxian tradition; and he produced one of most powerful and merciless critiques Marxism has ever received. He was the first French philosopher to appreciate the importance of Saussure's linguistics; but even his admirers admit that the things he purported to find in Saussure are simply not there to be found. He was the author of a corpus of works whose relevance for contemporary social theory has been matched only by the striking indifference which they all too often have received. (p. 1)

This is a strong statement of significance, yet the Merleau-Ponty that emerges at the end of the book was at best a transitional figure (there

is nothing wrong with that) and probably a quite minor one. What happened?

First, to avoid any overstatement of the case, being a minor figure worth attention in a league including Sartre, Husserl, and Saussure is no small achievement, and Merleau-Ponty was attempting to meet on equal terms thinkers of the caliber of Descartes and Marx. In this company it is difficult to avoid appearing minor.

Second, Schmidt is intensely aware of and bothered by the fact that Merleau-Ponty blatantly misrepresented his sources, particularly Saussure. Schmidt rather harps on this failure to observe the canons of scholarship, but how important is it? As Schmidt points out, this fact has been rather ignored by Merleau-Ponty scholars; therefore, it is essential that Schmidt warn the reader (and there is undoubtedly a good article waiting to be written on the subject), but once we are warned, it is not really all that important. Aristotle misrepresented Plato; it is, unfortunately, part of the tradition of philosophy to progress by climbing on the misrepresented shoulders of past thinkers.

Third, Merleau-Ponty took on so many of the greats on so many grounds that it is at times difficult to follow him. This Schmidt does admirably, but Merleau-Ponty's changing concerns convey the feeling that he never really finished anything. The sheer breadth of Merleau-Ponty's interests gives an impression that he lacks depth. Schmidt argues against this impression, but in a book of just over 200 pages in which the text occupies only 167 pages, he simply doesn't have the space to do so successfully.

The appearance given by both Merleau-Ponty and Schmidt of a constantly dissatisfied intelligence periodically grasping at straws in an effort to find a stable point should not obscure the fact that the problems that bothered Merleau-Ponty were very real problems in the positions he criticized. Merleau-Ponty found what he thought to be fundamental flaws in phenomenology, Marxism, and structuralism, and forced their exponents to change or abandon certain arguments.

Besides, perhaps a constantly dissatisfied intelligence is the most appropriate model for the last quarter of the twentieth century. As scholars, shouldn't we be more concerned with the thinker who asks the fundamental questions rather than with those who think they have found the definitive answers? While

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Schmidt doesn't pose the problem this way, he concludes with a metaphor intended to emphasize that change is the only constant in philosophy, and suggests that Merleau-Ponty will have contributed to the next step. Be that as it may, anyone interested in the intellectual history of the twentieth century will find *Maurice Merleau-Ponty* worth exploring.

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The Challenge of Social Control: Citizenship and Institution Building in Modern Society. Essays in Honor of Morris Janowitz. Edited by Gerald D. Suttles and Mayer N. Zald. (Norwood, NJ: Ablex Publishing Corporation, 1985. Pp. vi + 250. \$34.50.)

This is a collection of essays that were delivered at a conference honoring Morris Janowitz at the University of Chicago in 1982. The essays focus on topics that Janowitz has researched and written about during his illustrious career: the fragmentation of political institutions, the welfare state, social movements, the all-volunteer army, the legitimization of political authority, and urban institutions. Janowitz is the best known political sociologist to embrace institutional analysis, and the essays of these colleagues and students all have the master's mark on them.

The volume is divided into five parts. Part 1 contains an overview of Janowitz's ideas and career by Gerald Suttles and an essay by Don Levine on sociological theory. Levine's article is relevant in light of the *Heritage of Sociology* series published by the University of Chicago Press and edited by Morris Janowitz. Part 2 presents essays by Thomas S. Smith and Allan Silver. Smith addresses the issue of how individuals react to the absence of an integrated cultural order and of leadership in work groups. Silver describes the role that trust plays in the economic, social, and political arenas. Both essays extend Janowitz's work on social control. For Janowitz, *social control* is the capacity of a social system to govern itself according to a set of higher social or moral principles. Governments are viewed as proactive forces in society in which these values

are crystallized and given concrete expression. The problem of generating trust in governments is not something that Janowitz discusses at length, and Silver highlights some issues that all polities face. For example, it is relatively easy for trust to emerge in face-to-face recurring interaction, but it is more difficult to establish a trusting relationship between citizens and impersonal bureaucracies. Silver also focuses on an issue that Janowitz has only touched upon: What happens when values aren't articulated or leaders don't lead? Silver suggests that people withdraw into personal dyads for meaning, abandoning the public forum.

The third and fourth parts of the volume are of more interest to political scientists. Heinz Eulau and Vera McCluggage address one of Janowitz's most persistent concerns: the fragmentation of governmental institutions. In particular, they look at congressional committees. This essay highlights the complex processes that produce the proliferation—not fragmentation—of committees. Although the article wallows in functional imagery, it presents a sound critique of Janowitz's lament. Dwaine Marvick addresses the quality of electoral politics, another problem that has preoccupied Janowitz. The article is brief and suggests that elections can be scored on the basis of how manipulative activists were and how attentive citizens were. The essay by Roberta Garner and Mayer N. Zald is an extended theoretical treatise on the different dimensions of social movement sectors. It also looks at systematic constraints that can explain variation in these dimensions across societies. The authors compare the social movement sectors of the U.S. and Italy to illustrate their points. Clearly the article is more Zald than Janowitz, and will be more interesting to students of social movements than to the Janowitz discipleship.

Kirstin Gronbjerg, Charles Moskos, and David Segel look at the welfare state and military institutions. I view these three articles in a similar light because they all have a component of action or public policy. While he may object to the label "applied sociologist," Janowitz is clearly a man of his times. Moskos and Segel both address issues related to the all-volunteer army and national service. Moskos outlines proposals to implement the all-volunteer force; Segel reviews the theoretical and historical conflict between sociological and economic

conceptions of what the all-volunteer army should be. Gronbjerg traces the growth of the welfare state and the institutionalization of a mass-society ideology of citizenship rights. She then wrestles with the implications of a long time Janowitz truism—that the welfare state and the extension of citizenship rights are only possible in a growth economy. With tax revolts and Reagan's effort to bolster the military establishment only complicating matters, she speculates on what may follow: disenfranchisement, citizen withdrawal, increased inequalities, and weakened governmental legitimacy. Also—in true Janowitz style—she suggests ways out of the dilemma, ranging from reductions in transfer payments and loans to the middle class to the institutionalization of a voluntary national service.

The final section of the volume focuses on urban structure and also has a policy focus. After reviewing where people and jobs are moving in the contemporary metro setting, John Kasarda suggests that planners should cease trying to reverse powerful trends that are primarily market driven, and develop policies that are consistent with these trends. Jack Ladinsky and Charles Susmilch, William Kornblum, and Albert Hunter move to the neighborhood level and focus on institution building; unfortunately, none of the efforts cited are very encouraging. Ladinsky and Susmilch find that consumers do not use community-based institutions to resolve their difficulties with faulty products and services; Kornblum's case study of a New York City high school depicts utter chaos and despair; and Hunter's theoretical piece on crime control highlights the inability of various levels of social life in the city to coordinate their efforts and interests.

As a whole, the volume is successful as a testimony to Morris Janowitz—to how he inspires others to explore political, urban, and social concerns for their own sakes and come up with ideas for social change. Janowitz is not a grand theorist, but rather does his work in the middle range, identifying phenomena or processes that others ignore and exploring how these interface with the rest of society. Janowitz is an active participant in implementing change that he believes is necessary—especially in military and urban affairs—but he is not a revolutionary. He sees change as an incremental process. All the articles reflect this orienta-

tion and, to my mind, should serve as models for future scholars and policy makers alike.

JOSEPH GALASKIEWICZ

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A Rationalist Methodology for the Social Sciences. By David Sylvan and Barry Glassner. (Oxford: Basil Blackwell Publisher, 1985. Pp. vi + 188. \$29.95.)

The problem addressed is how to make rationalist theories better, that is, more eloquent and orderly: "Are tools available for more rigorous and systematic inquiry in the tradition of these rationalist theorists?" (p. 9).

The answer is a tentative yes. A case is made using Simmel's conflict-cohesion hypothesis (external conflict-internal group integration). Other major examples are French structuralism (Levi-Strauss) and new social realism (Giddens). Simmel's hypothesis is formalized and analyzed with a logic designed in a computer program (LOGLISP).

The world of social analysts is divided into rationalists and empiricists:

Rationalists seek to make coherent sense of the social world rather than to manipulate empirical phenomena through experimental theories and other methods. In rationalist theories explanation aims at logical possibilities, not statistical probabilities; the concern is with combination, not causality. (p. 1)

Another way of expressing this distinction is in terms of macrotheorists and micro-observers.

Rational theorists, however, do not have a "methodology." Without something like the empiricists have, it is difficult to sustain a profession—we may degenerate into a collection of associated individuals, each relying on our personal insights and languages. The authors offer formal modeling as a methodology that, they argue, is at least not incompatible with the thinking of rationalist theories. Something more than discussion can be done. The reasons given for creating a methodology for European structuralists are lack of clarity and consistency.

There are many fine points scattered throughout the book, including criticisms of major controversial thinkers such as Althusser

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and Poulantzas. One is that existing languages, of which logics are one type, are not suitable for the social world made up of contours, bumps, and contradictions. The development of catastrophe theory in the early 1970s awaits convincing examples in the social sciences. The authors' arguments open up another set of possibilities for the 1980s.

The predicament of the social sciences is that most of its empirical research has produced few noticeable theoretical yields; yet its theoretical creations do not translate well into coherent statements and research protocols. Some 50 years ago, F. A. von Hayek assessed the European (German) historical school's pursuit of applying the methods of the natural sciences as one reason for the collapse of economic theory. He predicted that such an effort "is bound to degenerate into mere record and description and a total skepticism concerning any laws at all" (F. A. von Hayek, ed., *Collectivist Economic Planning*. London: Routledge, 1935, p. 9).

The main road to theory is thinking, rather than organizing to build theory upon observations. If formal modeling will help thinking—and I believe it can—then perhaps the best way is to put both capacities into the heads of the creators of theories. This book, however, accepts the largely American tradition that methodologies constitute a separable "field," whether in observation or speculation.

HENRY TEUNE

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Behavioral Decision Making. Edited by George Wright. (New York: Plenum Press, 1985. Pp. 407. \$47.50.)

The subject of decision making is parceled out among so many disciplines and subfields that nobody can keep up with all the scholarship on the subject. Political scientists, economists, management scientists, sociologists, psychologists—all have staked claims to a piece of the action. In this book, as the code word *behavioral* indicates, we get an opportunity to see what psychologists and decision analysts have been up to recently in their empirical research on the ways in which people make choices. The papers in the collection are

concerned primarily with understanding how and why individuals depart from the principles embodied in "expected utility" models of decision making—i.e., why people do not do the things that decision analysis shows they should do—and with exploring whether and how decision aids can improve decision outcomes. The authors review a good deal of recent research on decision analysis in fields as various as architectural design and insurance underwriting.

The sections on individual and small-group decision making are the strength of the book, and illustrate the interesting questions that psychologists address and the often ingenious research methods that they use to study them. They test the effects of such variables as task complexity, time pressure, and the way in which a problem is framed. The paper by Amos Tversky and Daniel Kahneman gives a number of illustrations of how small differences in the conceptualization of a problem lead to significant differences in choices.

In the section on organizational decision making, the editor turns the pages over to two sociologists, who create an island in the book quite different from anything that comes before or after. Roger King draws on political science and sociology to address issues of power—what it is and how to measure it. His chapter reviews the difficulties of conceptualizing power in the light of "nondecisions" and "false consciousness." The paper is a sophisticated assay of theories of power, but since it barely alludes to decision making, it seems to have strayed into the wrong company. On the other hand, the excellent paper by Weeks and Whimster confronts the subject of behavioral decision making directly. They present a devastating critique, in fact several critiques, of the rational and individualistic assumptions that underlie it. They cite Tom Burns's conceptualization of an organization as an interaction of three different systems—a working organization, a political system, and a career structure. When individuals calculate the utilities of organizational choices, their referent can be political influence or career prospects just as well as the goals of the working organization. Weeks and Whimster also discuss Habermas's critique of rationality and his alternative formulation of communicative rationality. This chapter, although in contention with the papers around it, fits the book

well and is likely to be of interest to political scientists.

The latter part of the book analyzes the capabilities of decision theory. Several papers have something to say about the role of decision aids in improving decision making. In a section on judgmental forecasting, a paper by Hillel J. Einhorn and Robin M. Hogarth discusses the ways in which causal judgments affect the use of formal statistical models. An appealing paper by Allan H. Murphy and Barbara G. Brown assesses the accuracy of objective and subjective methods of weather forecasting. Objective methods are those that use numerical data and/or statistical models; subjective forecasts depend on the judgment of meteorologists. The paper concludes, you will be happy to hear, that both objective and subjective forecasts are generally accurate and have improved in accuracy over the period from 1971 to 1982. Subjective forecasts tend to be slightly better for predicting precipitation and temperature in the near term; objective forecasts are better for predicting the amount of clouds.

The final section of the book looks for areas in which decision theory may have impact in the future. Jack Dowie's paper suggests that university education is an arena in which decision theory approaches might be applied—for example, by allowing students to answer multiple choice tests probabilistically. That is, students would be asked not only to give an

answer, but also to indicate their level of certainty that the answer is right.

Of the 21 contributors to the book, 11 are British, including Wright himself, who is with the Decision Analysis Group, Department of Psychology, City of London Polytechnic. This gives a pleasant flavor to the writing and to some of the illustrative examples, although most of the literature cited is American. Wright is proud of the fact that 14 of the 17 papers were commissioned expressly for the volume and appear here for the first time. However, even commissioning does not necessarily produce a volume that has a coherent structure. The sections of the book sometimes appear to be arbitrary clusters of papers rather than a set of logically related ideas. Furthermore, the book lacks an index.

If you want to learn about recent work in decision theory approaches to decision making, this is an engaging and particularly wide-ranging set of articles. (The editor suggests that a novice might want to begin with a more elementary treatise.) If you come to the book looking for insights that will help explain the decision-making behavior of legislators or the outcome of public policy debates, it will take creative reading and leaps of inference to extract them.

CAROL H. WEISS

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AMERICAN POLITICS

Setting Municipal Priorities, 1986. Edited by Charles Brecher and Raymond D. Horton. (New York and London: New York University Press, 1985. Pp. xxiii + 476. \$60.00, cloth; \$30.00, paper.)

Union Power and New York: Victor Gotbaum and District Council 37. By Jewel Bellush and Bernard Bellush. (New York: Praeger Publishers, 1984. Pp. xxii + 474. \$33.75.)

Setting Municipal Priorities, 1986 is more a government yearbook than a scholarly monograph. It is the seventh in a series dedicated to

surveying New York City's administrative operations, edited by Charles Brecher of New York University's Graduate School of Public Administration and Raymond Horton of Columbia's Graduate School of Business. The contributors are largely professors from the aforementioned universities and policy analysts from nonprofit research foundations. Each chapter deals with a specific public policy area, with particular emphasis on the delivery of services, but with coverage of the local economic condition and outlook as well as revenue and spending policies. The authors

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largely employ a common format, and the chapters are of uniformly high quality, even conceding the difficulties associated with multiple authorship. The use of statistical data and their integration into the discussion is exemplary, and the "future policy options" proposed are usually thoughtful and fair minded. It is curious that such a work succeeded in incurring the wrath of Mayor Edward Koch, who has questioned the scholarly objectivity of the research (*New York Times*, November 9, 1985).

Although each policy area reviewed comes with recommendations for programmatic improvements, the contributors are stronger in describing problems than in proposing solutions. The analysis is vastly informative, replete with statistical tables and abounding in basic information presented in an unbiased fashion—analytical, but unfortunately not very political. Too many of the recommendations are managerial and technical, without addressing themselves to the political realities underlying policy choices. There is no Ed Koch, Victor Gotbaum, Al Shanker, or Bobby Wagner in *Setting Municipal Priorities*; nor is there any overall set of priorities for the city government to pursue. On the other hand, most of the specific city services are examined with great care. For example, John Kaiser's chapter on sanitation documents the dramatic productivity improvements that have been realized in a notoriously demoralized department, and the chapter on criminal justice by Esther Fuchs and John Palmer Smith thoughtfully analyzes commonplaces about crime, criminals, and courts with good use of data and a strong grasp of the literature. In sum, the quality of the policy research in *Setting Municipal Priorities* is very high. If one wants to get a firm grasp of the background and issues of the policy problems confronting New York City, this is the place to start.

Union Power and New York, essentially a life and times of Victor Gotbaum and of District Council 37 (the AFSCME local in New York City), is a very different sort of work: qualitative, personal, sometimes emotional, and definitely not disengaged. Indeed, it is essentially a memoir of the union written in a tone of easy familiarity with its leaders, demonstrating extensive knowledge of its history, and exhibiting clear sympathy for its programs and goals. For the Bellushs, this book is

clearly a labor of love, and just as clearly reveals a depth of research in the archival materials that is quite impressive. On the other hand, the authors sometimes indulge themselves in value-laden references that give "thanks for Gotbaum's heroic leadership" (p. 99), or cite the "limitless devotion to the rank and file" of DC 37 Associate Director Lillian Roberts (p. 211). This is not to imply that *Union Power and New York* is biased or lacking in overall objectivity; it would, however, have benefited from a less critical approach and greater analytical rigor.

It is frequently noted that DC 37 has been in the forefront of both public and private sector unions in providing economic and educational benefits for its members and in supporting important social welfare legislation benefiting the public at large. These are accomplishments with which the authors are in agreement, and what the Professors Bellush have done very well is to give a detailed account of the rise of Victor Gotbaum as a labor leader and DC 37 as a public-employee labor union. If the book is short on hypotheses, it is replete with information about the union's management and political activities. It also conveys a vivid sense of the passionate commitment to the principles of social egalitarianism and ideological liberalism that has been such an important part of the style of New York City politics.

As the leader of the largest public-employee local in the country, Victor Gotbaum has exerted a major influence on the politics of both New York City and the state of New York. Certainly, Gotbaum's most visible and far-reaching accomplishment was the role that he played in forging the alliance of unions, banks, businesses, and city and state governments that kept the City from lurching over the brink of bankruptcy during the period 1975-77. This herculean task would have been an extraordinary feat of leadership for someone from a secure political base. Gotbaum, however, had to support budgetary austerity measures that diminished his membership's benefits, even while he insisted that the entire burden was not to be borne by public employees and the public's services. The Bellushs sum up the dual orientation that has characterized DC 37 as follows:

Philosophically, Gotbaum gave the Council an independent, progressive orientation on a wide

range of issues. District Council 37 has defended the rights of the poor and aged, supported the ecological movement, and spoken out forcefully in the struggle for civil rights and human dignity. (p. 453)

At the same time, Gotbaum also knew "that he had to fulfill his major functions as a union leader which was to negotiate good contracts on the fundamental day-to-day concerns of the members" (p. 454).

Whatever one may think of DC 37's ideology and politics, there is no question that *Union Power and New York* conveys a sense of the peculiarities of the City's politics that is missing in the more detached *Setting Municipal Priorities*. Both are to be recommended for different reasons to the reader who wants to know what makes New York unique among American cities.

KEVIN V. MULCAHY

Louisiana State University

Farm Policy: The Politics of Soil, Surpluses and Subsidies. By Nancy A. Blanpied. (Washington, D.C.: Congressional Quarterly Inc., 1984. Pp. ix + 179. \$9.95, paper.)

Made in Washington: Food Policy and the Political Expedient. By Clarence D. Palmby. (Danville, IL: The Interstate Printers and Publishers, 1985. Pp. xi + 226. \$14.95.)

Alternative Agriculture and Food Policies and the 1985 Farm Bill. Edited by Gordon C. Rausser and Kenneth R. Farrell. (Berkeley: Giannini Foundation of Agricultural Economics, University of California—Berkeley and the National Center for Food and Agricultural Policy, Resources for the Future, 1984. Pp. xix + 425. \$18.00.)

U.S. Agricultural Policy: The 1985 Farm Legislation. Edited by Bruce L. Gardner. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1985. Pp. viii + 385. \$12.95, paper.)

The plight of the farmer has been a recurrent theme throughout all of American history. There have been periods of farmer prosperity, to be sure, and not the least of these was during the 1970s. On-farm income moved up to levels which made the farmer about equal in

economic status with urbanites. Federal costs for agricultural commodity programs (excluding, of course, food stamps, research, etc.) had shrivelled in that period to an almost inconsequential \$600 million. Then came "the terrible 80s," and once again the media were overwhelmed with truly heartbreaking stories of farm foreclosures and even murder and suicide. Moreover, public costs of agricultural programs rocketed to frightening heights—an estimated \$24.6 billion by 1986—and all the while the farm crisis was exacerbated.

Congress hardly needed to be confronted with another crisis. Indeed, the farm economy during the 1970s was a major respite from the nation's economic ills. The value of farm exports almost exploded from some \$6.7 billion in 1970 to almost \$32 billion by 1979, and this remarkable increase was certainly helpful in mitigating the adverse effects of the national balance-of-trade deficit. However, for numerous reasons which cannot be examined here (high interest rates, an almost explosive rate of inflation followed by an overvalued dollar, and severe budget deficits) the 1980s brought the farm issue back to a prominent place on the congressional agenda. Moreover, the 1981 Farm Act expired in 1985, and to revert to the legislation of 1949 would have burdened Congress with even more onerous budget costs, and in the long-run would have seriously aggravated an already depressed farm economy.

Congress had to act, and eventually—by December 1985—the Food Security Act of 1985 was passed. To a major extent (although somewhat less so with the study by Clarence Palmby), all of these books were scholarly efforts to influence this bill; that is, they sought to affect who was to get what, at what costs, and with what effects on the U.S. Treasury, the farmer, agribusiness interests, and the rest of us.

Farm Policy, Nancy A. Blanpied's study for the Congressional Quarterly, can be recommended for use by those teaching introductory undergraduate courses in American government who wish to have their students acquire a general understanding of U.S. food and agricultural politics and policies. The presentation is heavily descriptive, as it should be at that level of instruction. The discussion of "agricultural issues" (part 1) is presented in an even-handed and sufficiently comprehensive man-

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ner. Particular praise is due the chapters on soil erosion, farm migrant labor, agricultural research, and farm pesticides. One might, however, have expected a chapter on nutrition and food-based welfare programs. Part 2 deals with the development of U.S. agricultural and food policies since the end of World War II, and these chapters are also handled in a readable, competent form and style, although the bibliographic references are less than impressive.

Made in Washington, by Clarence Palmby, is essentially an autobiography, interlaced with a fair amount of personal ideology and policy proposals. Palmby has had a moderately distinguished public career in American agriculture—executive vice-president of the U.S. Feed Grains Council from 1961 to 1969, assistant U.S. secretary of agriculture in the early 1970s, and vice-president of public affairs for the Continental Grain Company thereafter for nearly a decade. We are indebted to him. Political science literature needs more first-hand, candid, analytical recounting from public officials who have also served prominently in quasi-public and agribusiness companies. His account of the Russian wheat sale in 1972, in which he was “bruised” by “false and unfounded” interpretations (pp. 141–42), is an important and largely persuasive addition to our scanty knowledge of what happened. Also, Palmby’s description and defense of the origin and role of the U.S. Feed Grains Council in building U.S. farm export markets needs to be understood by students of U.S. trade policy. His discussion of past international wheat agreements and aborted efforts to negotiate a more comprehensive wheat agreement in the early 1970s should be useful to those who will be studying the General Agreement on Tariffs and Trade (GATT) negotiations—both past and (likely) of the near future.

Although I’m guessing, it seems fairly certain that Palmby also was motivated to write his memoirs in an effort to influence the writing of the 1985 farm bill with his personal input. He strongly disapproves of the theme song, “Farm prices are made in Washington.” Although his own predilections cannot be examined here, he does “recommend an end to CCC [Commodity Credit Corporation] loan rates on corn, sorghum and other feed grains” (p. 213). There are substantial reasons why we might disagree with his policy proposals, but

we are indebted to Palmby for several case studies concerning important agricultural policies over the last 30 years.

The last two studies are certainly the most important in terms of the depth and sophistication of their analyses. They can be reviewed simultaneously because both volumes really comprise the proceedings of conferences at which, by and large, the same set of agricultural economists examined, analyzed, and elucidated the condition of the American agricultural economy and what should be done to improve it. Both conferences were designed to provide professional expertise, in the somewhat naive anticipation that this would be heeded by Congress as it proceeded to construct what came to be called the *Food Security Act of 1985*. As Ronald Knutson observes in *Alternative Agriculture*, “Farm bills are written by politicians—not by economists.” However, he then proceeds to make the important claim that “the economists’ role is to identify policy options and evaluate their consequences” (p. 418).

It is not feasible to try to review all of these papers, which have now become chapters followed by discussions. In *Alternative Agriculture*, political scientists would profit from a reading of D. Gale Johnson’s paper, “The Performance of Past Policies: A Critique” (chap. 2). Alex McCalla’s review of the preceding analyses of farm commodity policies (pp. 302–11) and Stanley Johnson’s discussion of the alternative policy proposals (pp. 403–18) will lead the interested reader back to some of those papers for a more thorough examination. For some useful insights into the politics of American agriculture, political scientists should see Knutson’s brief foray into political realism (pp. 418–22).

The centerpiece of *U.S. Agricultural Policy* is a paper by several agricultural economists at Iowa State University and the University of Missouri who jointly constitute a Food and Agricultural Policy Research Institute. Their influential and readable paper (pp. 119–94) is definitely recommended; indeed, all of part 2 (“Policy Options for 1985”) could be designated for inclusion on the “required” list, especially John Schnittker’s “synthesis” (pp. 201–4). Schnittker was a prominent United States Department of Agriculture (USDA) official in the Kennedy-Johnson administrations, and his policy insights are consistently impres-

sive, a delightful intertwining of economic analysis and political realism.

Let us conclude with comments. The first concerns the rise in influence and prestige of agricultural economists over, say, the last 50 years or so. Bruce Gardner presents their case succinctly: "I believe that the opportunity exists to shape policy through analysis; for Congress, however motivated, wants to know the consequences of its actions (although it may not want them all published)" (Gardner, p. 15). To claim that agricultural economists "shape" policy seems to be something of an exaggeration; perhaps "expound on" would be a more accurate choice of verb. In any case, one could certainly argue that the members of the American Agricultural Economics Association have been extensively involved, over the last half-century, in efforts to influence policy makers to give credence to their analyses and legal sanctions to their prescriptions.

The second comment tends to modify the first. The research and scholarship of political scientists such as Theodore Lowi, Randall Ripley, and Grace Franklin (among numerous others) proved substantially more useful in predicting what actually came to be the principal provisions of the Food Security Act of 1985 than the valuable, but largely neglected, advice and counsel offered in the two conference proceedings reviewed here. The new farm law is another classic example of distributive politics; the iron triangle concept has not lost all of its analytical power, by any means. If political scientists and economists desire to become really influential in the rooms and corridors wherein policymakers negotiate and decide, we need to unite our disciplines in a much more effective form and manner. This fusion is underway, admittedly, but there is still very much to be done.

ROSS B. TALBOT

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Judges, Bureaucrats, and the Question of Independence. By Donna Price Cofer. (Westport, CT: Greenwood Press, 1985. Pp. xvii + 245. \$35.00.)

This book about the role of administrative law judges (ALJs) in the Social Security Disability program moves along uneasily on two

levels. First, it describes the battles in the continuing war among the administrators, politicians, and judges concerned with administering the program. Second, it criticizes the administrative process, primarily on the ground that ALJ independence has been threatened. The author sums up her point of view with the argument that

If ALJs are not afforded adequate protection against bureaucratic, and therefore political, intrusions into their role, their objectivity will be—or will at least appear to be—compromised. (p. 35)

The book succeeds well in its first goal of describing the conflicts that have marked the last decade or so of the Social Security program. The first three chapters give us a general history of the program, of the decision-making process in government benefit programs, and of the role of ALJs. This provides the background for chapters 4 and 5, which thoroughly explain the conflicts from 1960 to the present. We learn about the Social Security Administration's growing concern with how the program was being administered, specifically with the ALJs' productivity, inconsistency, and high pro-claimant decision rates. We are told how the agency brought pressure on ALJs by withholding travel and staff perquisites, and how ALJs fought back in court to maintain their independence.

The author does not conceal the complexity of these battles. The agency's concern with productivity produced the mechanically applied "grid" for determining disability, which reduced inconsistency and improved productivity, but increased pro-claimant decisions. The agency's equating of pro-claimant decisions with decision-making errors is also criticized. Chapter 6 provides a detailed report on the results of a questionnaire sent to ALJs to obtain their views about these problems, and chapter 7 concludes with proposals for reform, including improvement of the state agency process, experimenting with more adverse ALJ proceedings, and adoption of a Social Security court.

The book is less successful in its second objective of criticizing the decision-making process. The rhetoric of the critique is that ALJ independence, and therefore claimants' rights, are threatened by a bureaucratic, and therefore heartless and political, administration. The

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characters in this morality play are too one-dimensional. Indeed, the strength of the book is its detailed description of the conflict among the actors, which is more complicated than the ALJ-vs.-bureaucracy rhetoric would suggest. It is not that the author's perspective is without support. The decision-making process at the state level is too routinized and the pressure from the agency on ALJs is excessive. However, the significance of the threat to the ALJs is not fully captured by viewing it as a simple dispute between the defenders of individual rights and intruding political bureaucrats. Put differently, the issues are less those of a morality play pitting individual against government, and more those of high political drama pitting those with different political views against each other, compounded by the fact that the ALJs' sense of professional self-worth is implicated by the definition of their role.

A somewhat richer view of these conflicts would point out that the Social Security Disability program is poised not-so-delicately at the conjunction of law, politics, and administration. It is not just that the definition of disability is unclear. The idea is itself the product of deep, even irreconcilable tensions arising from the public's conception of malingering, and from the uncertain distinction among the risks covered by unemployment, old age, and disability insurance. The disability program builds these tensions into its administration by placing independent judges at the end of the review process, as is true of the Tax Court in the federal income tax system. When this more complex perspective is adopted, the agency appears less heartless and politically intrusive, and more like any other agency trying to respond to shifts in political climate, often under congressional pressure. The clash between the agency and the ALJs becomes a dispute about interpretation, similar to what we might encounter if the Tax Court were inside the Internal Revenue Service rather than an independent court. The ALJs' problems are the result of housing them inside an agency, where political conflicts are inevitable.

WILLIAM D. POPKIN

Indiana University

The State Politics of Judicial and Congressional Reform: Legitimizing Criminal Justice Policies. By Thomas Carlyle Dalton. (Westport, CT: Greenwood Press, 1985. Pp. xxi + 234. \$35.00.)

The goal of this study is to understand what factors affect the degree of success with which criminal justice policies formulated at the federal level are implemented by states and, based upon these findings, to speculate more generally about political legitimacy and accountability in the United States. Toward this end, Dalton compares responses in four states (California, Colorado, Massachusetts, and Washington) to Supreme Court decisions concerning the rights of the accused and to executive and congressional policies concerning the quality of and access to criminal records.

The book's potential strength is its description of what transpires when the federal government is dependent upon the state for implementing policy. Indeed, the most satisfying parts of the book discuss how state courts, legislatures, and administrative officers grapple with federal policy—either to enforce it, subvert it, or simply understand it—and the motives of the various actors. Although one suspects that he makes too much of perceived distinctions in the political traditions of the four states to explain differences in behavior, Dalton does contribute to our understanding of the complexity that characterizes state implementation of federal policy. Furthermore, the discussion of how state and federal courts respond to one another and to other political institutions in their respective environments is thought provoking. We are provided with accounts indicating that the constraints faced by the state courts in executing federal policy vary, depending upon how the courts are perceived by others in their respective environments.

Unfortunately, the book does not live up to its fullest potential, for several reasons: careless usage; lack of clarity in the presentation and incorrect information; and unsubstantiated and overly broad claims. These flaws seriously undercut the author's credibility.

Interest groups and *elites* are referred to throughout the text, but the author makes little effort to define those attributes which might help the reader to identify an individual as

either an elite or a member of an interest group. We are told that governors, attorneys general, judges, local elected officials, and state and local administrators are among the elite, but we are not told what characteristics distinguish them from nonelites. The difficulty created by this omission becomes important as we are introduced to a plethora of undefined elites, including political elites, national and state elites, policy subsystems elites, institutional elites, and administrative and professional elites. Sometimes individuals identified as elites function as interest group members; other times they are the target of interest group activity. We are told even less about Dalton's conception of interest group theory. This is problematic when, for example, he seems to equate the existence of an interest group (the ACLU in Colorado) with proof of its strength (p. 162).

In many ways, the most frustrating aspects of the book are the rather sweeping generalizations and cryptic remarks that are not warranted by the evidence presented. The first and longest chapter reviews the shortcomings of several major theoretical perspectives—occasionally with less-than-generous treatment of their proponents—and leaves the reader with high expectations about the utility of Dalton's version of "legitimacy theory [which] is uniquely suited to explain the outcomes of judicial and Congressional reforms in the administration of criminal justice" (p. 5). Yet in chapter 1, Dalton never offers more than what "appear to be several elements or conditions with which we could forge a separate theory of legitimation" (p. 36). Rather, we are asked to wait until the final chapter for a "reformulation" or description of legitimacy theory. This stylistic characteristic of making assertions but asking the reader to wait for a more thorough discussion and justification at a later point in the text is pervasive. Unfortunately, our patience is not very well rewarded. The closest we get to an "explanatory framework" is a variant of the systems approach, which owes much to David Easton, among others. Little if anything about the book is novel from a theoretical perspective, and Dalton seems to make too much of what he does offer. For example, he makes the unexceptional claim that, "one of the significant conclusions emerging from Chapter 6 is that federal policy originators can influence but are

unable to control the state implementation of national policies regulating criminal justice processes" (p. 177).

Sentences or phrases are offered without appropriate context or are not justified by the material presented. For example, we are told the Massachusetts Supreme Judicial Court must continue "to conceal its underlying but long suppressed ideological principles" (p. 135), whatever they may be, and that "ambiguity can serve some important practical purposes, shielding the court from unpleasant political realities" (p. 140). The Colorado press, as an interest group, "held criminal records legislation hostage" (p. 141). Finally, much of the evidence presented to justify the interpreted impact of *Miranda* and *Mapp* on the states tends to be anecdotal interview data from anonymous officials.

While one might wish to argue that none of these errors in themselves damage the entire enterprise, together they create a penumbra of doubt about the care that went into this effort.

FREDRIC A. WALDSTEIN

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Boston*

Political Communication in America. By Robert E. Denton, Jr. and Gary C. Woodward. (New York: Praeger Publishers, 1985. Pp. xvii + 364. \$15.95, paper.)

When Information Counts: Grading the Media. Edited by Bernard Rubin. (Lexington, MA: D.C. Heath and Co., 1985. Pp. xiii + 244. \$23.50.)

These two volumes join a growing list of works that take seriously the mutual exchange between communication and politics in contemporary society. Although differing considerably in origins and purposes, they complement each other in overall content and emphases.

As professors of communication at Northern Illinois University and Trenton State College, Denton and Woodward represent an expanding population of communication scholars addressing the cross-disciplinary concerns that bring together students of politics no matter what their academic training or appointment—i.e., those in political science, speech communication, journalism, sociology, and

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cognate disciplines. Noting that "the communication analyst is always a guest (if not an intruder) on someone else's turf" (p. vii), the authors have written a textbook summarizing the roles and functions of communication in U.S. politics.

Because they believe communication to be prior to all other fields of inquiry, it is not surprising that the authors adopt what some observers may well regard as an imperious definition of their subject matter. Political communication, they write, is "*public discussion about the allocation of public resources (money), official authority (who decides), and official sanctions (what is to be rewarded or punished)*" (pp. 15-16). It appears, however, that they quickly draw back from asserting that communication is all of politics—i.e., that communication is the authoritative allocation of values—by asserting that political communication is a "valuable mediating agent" (p. 18) that makes it possible to draw up short of violence, promoting instead compromise, public acceptance, and orderly change.

Augmenting that definition with a framework borrowed from symbolic interactionists, the authors proceed to trace the role of communication in a variety of political settings—electoral campaigns, the presidency, bureaucracy, Congress, and mass media. There are, of course, threats to open political discourse—coercion, deception, mystification, etc.—and so the authors close with a warning that unfettered political talk, despite its inefficiency, is essential to free government. That this line of argument frequently has been repeated before, the authors freely admit. So freely, in fact, that one of the overriding characteristics of the book for those who might employ it as a course text is that it offers a compendium of virtually everyone, practitioners as well as scholars, who have ever written on the subject. Whether undergraduates reading the text will be as drawn to such graybeards is another matter.

Denton and Woodward end their text decrying the corruption of political communication. Rubin's edited volume is devoted primarily to that topic alone. The 13 contributions comprising this work examine the role communication plays in shaping the power and impact of all manner of political actors—democrats, demagogues, and dictators. The essays grew out of a series of panel discussions organized

by the Associates in Research for Public Reporting. As a result, the individual selections mesh around common themes in a fashion superior to that characteristic of most edited volumes.

Rubin, professor of governmental affairs and communication at Boston University, has organized selections around three themes. The first directs attention to why we might have legitimate concerns regarding whether political information actually serves democratic ends. The second theme emphasizes areas of endangered free speech, and the third focuses primarily upon the news media role in covering, explaining, and perhaps exacerbating problems. The topics of some articles repeat points made many times over—for example, the use of buzzwords in news coverage, George Orwell's prophecies, the role of cameras in the courtroom, and how television covers international crises. More rewarding are Rubin's critical updating of Walter Lippmann's notion of stereotypes in the electronic age, Ralph Rosnow and Marianthi Georgoudi's systematic analysis of the part played in politics by small talk and idle gossip, and Walter Clemens's critique of the intellectual foundations of the Reagan policy toward the Soviets.

Two of the audiences for this volume, according to Rubin, are identical with those addressed by Denton and Woodward: first, college and university students, and second, their professors. Given the vertical, issue-directed and case-centered orientation of the selections, one suspects that Rubin's edited work will appeal far more to professorial inclinations, whereas the horizontal, highlighting style of Denton and Woodward is better suited to the yellow felt pens of likely undergraduate readers. Taken together, the two works reintroduce what are fast becoming the staples of political communication. It remains to be seen whether more is to be said or, as with so many cross-disciplinary fields in the social sciences, this one has already peaked.

DAN NIMMO

University of Oklahoma

Government and Sport: The Public Policy Issues. Edited by Arthur T. Johnson and James H. Frey. (Totowa, NJ: Rowman & Allanheld, 1985. Pp. x + 275. \$38.50.)

In the past it was common to find the sports pages of newspapers filled with stories about which teams won and lost and which players performed well, and with predictions for upcoming games or seasons. These stories would serve as the focus of debate for sports fans. Now, however, a glance at the sports news and eavesdropping on conversations among fans will produce a different mix of topics: discussions of strikes, antitrust, television contracts, law suits, Title IX, boycotts, graduation rates, franchise movements, and a host of other policy issues. In this changed environment it is hardly surprising that considerable scholarly interest is now being given to issues in professional and amateur sports. The volume *Government and Sports* by sociologist James H. Frey and political scientist Arthur T. Johnson includes research from a variety of disciplines on a range of issues confronting sports today. Holding this diverse collection together is the linking of each issue to the role that government now plays in the regulation of professional and amateur sports, and the alternatives to government involvement in each of the issues under consideration.

Johnson and Frey organize the volume's 13 articles into three sections. The first is comprised of those that address policy issues and athletes. It includes pieces on labor negotiations between owners and players, the status of the amateur athlete as an employee, the impact of Title IX on women's sports, regulation of sports agents, and policies dealing with sports violence. By contrast, the second section addresses issues of sports administration such as the legal status of NCAA rulings, the application of antitrust laws in professional sports, sports broadcasting policy, and the effect of tax policy on sports. The final section focuses on what the editors label "public interest" and sports—actually a catchall section for three unrelated issues of sports and public policy: gambling, franchise relocation, and foreign policy in sports.

In their introduction, Johnson and Frey claim that the purpose of the book is to illustrate how public policy affects sports in the United States. Accordingly, each essay is

designed to answer three questions: (1) "What is the proper role of government relative to sports?" (2) "Is this specific sports issue an appropriate issue for government to resolve?" and (3) "Is government action capable of bringing about a resolution of the issue before them?" (p. 14). To a significant extent, all of the essays attempt to meet these goals, but the real value of this collection is in the substantive information one can gain about the policy issues involved. The collection is as up-to-date as any such effort could be expected to be. Each issue is carefully defined and presented in a fashion that does not assume the reader possesses great prior expertise. Yet the authors fully explore each issue's complexities. Of particular value are the extensive citations at the end of each chapter, because much of the material cited would not necessarily come to the attention of political scientists. In sum, these essays correspond to good law review articles.

It is unlikely that readers will find all the essays of great interest unless they are interested in sports policies in general. It is far more likely that those engaged in policy research will find the discussion of particular issues in relation to sports—tax policy, antitrust policy, civil rights, etc.—to be of specific use to their research. Moreover, many of the contributors are not social scientists. What links there are to broader policy theory come primarily from the editors' introduction and conclusion. Frey and Johnson do try to evaluate the potential for government regulation for each of the sports issues examined in the book in terms of James Q. Wilson's ideas on the concentration of costs and benefits. Do not, however, expect to find significant argument given to theoretical underpinnings.

There are three articles that will be of particular interest to faculty members concerned with the conduct of intercollegiate athletics. Allen Sack and Bruce Kidd analyze the arguments and court cases dealing with the employment status of scholarship athletes. They contend that scholarship athletes are employees and should be entitled to the rights that employees enjoy. In a study of the impact of Title IX, Linda Jean Carpenter finds that its implementation has produced several unintended consequences. Brian Porto offers an enlightening analysis of the shortcomings of the NCAA and the alternatives available to

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other participants in the development of intercollegiate athletics policy to address the situation.

Government and Sport offers something of value to a broad range of readers. In this sense, the book's diversity is one of its major strengths. This diversity is also weakness, however, because it is unlikely that many readers will be interested in more than a few of the substantive issues covered in these essays.

BRUCE I. OPPENHEIMER

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Protest: Studies of Collective Behavior and Social Movements. By John Lofland. (New Brunswick, NJ: Transaction Books, 1985. Pp. xii + 349. \$24.95.)

This book is a collection of papers and previously published articles by a sociologist well known for his perspicacious and original analyses of the Unification Church (UC) in its infancy. These and his more theoretical articles on contemporary religious movements remain the best chapters in what is essentially a potpourri of pieces, some of which have nothing to do with protest, and not much more to do with collective behavior and social movements.

It is unfortunate that Lofland did not use his prior writings as a framework from which to produce an integrated book. Nonetheless, his analyses of the UC and other cult organizations, though not as transferable to political movements as he would have his readers believe, do provide much food for thought. The most insightful section for political scientists is that on conversion, which contains two studies of the UC and one theoretical chapter. Conversion is a process rarely analyzed by political scientists other than in its most extreme form—brainwashing. Yet some form of conversion is necessary for every political organization seeking to recruit supporters to nontraditional points of view. Lofland provides both a definition and a complex model of conversion—illustrating six different types—along with case studies that illuminate some of them.

His section on organization tries to identify types of social movement organizations, but his data are almost entirely based on religious

movement organizations. Since religious groups fulfill different functions than political groups, and are rarely concerned with capturing state power or changing public policy, the applicability of his analyses to the political realm is limited. Nonetheless, this chapter is helpful in understanding why some political groups, usually extremist ones, do resemble religious movement organizations.

The section on protest actions is the weakest. Its major emphasis is on interest group activities such as lobbying and networking. While one could argue that these are forms of protest, it does stretch the word beyond its usual meaning. Since the types of actions to which the term *protest* is usually applied are given only cursory attention, Lofland's analyses of lobbying appears idiosyncratic rather than part of a larger framework.

JO FREEMAN

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Social Research in the Judicial Process: Cases, Readings and Text. Edited by Wallace D. Loh. (New York: Russell Sage Foundation, 1985. Pp. xxix + 736. \$37.50.)

Social Science in Law: Cases and Material. Edited by John Monahan and Laurens Walker. (Mineola, NY: The Foundation Press, 1985. Pp. lix + 521. \$32.50.)

These texts contain a wealth of empirical research and judicial decisions illustrating the relationship—or the lack thereof—between social science studies and legal policies. Fortunately, the books approach this relationship from different perspectives, and thus overlap only minimally. Loh's book is probably of more interest to political scientists, because it is primarily aimed at the social science student and because it reprints considerably more research than case law. Readers are assumed to know little law, and are given an elementary introduction to the legal process and the case method. Monahan and Walker's text is explicitly written for law students and professionals, and begins with a primer on social science research methods. Nonetheless, much of their material can be useful for social scientists.

Loh focuses largely on the major public law controversies. The famous dispute over the use

of sociological studies in *Brown v. Board of Education* (1954) is covered, as is the ongoing debate over the degree to which social science has informed particular busing decisions or illuminated what the racial integration of schools has accomplished. In the criminal justice area, studies of the impact of *Miranda v. Arizona* (1966) are analyzed, as are those of the exclusionary rule in search and seizure. Research into the efficacy of capital punishment is also included.

Over a quarter of Loh's text focuses on jury research—e.g., selection procedures, how decision rules affect the outcome, the effect of exposure to pretrial publicity, and jurors' reactions to the nature and order of the presentation of evidence or the demeanor of witnesses—and this may be of less interest to political scientists. Juries are a frequent object of social science research, especially among social psychologists, probably because jury situations can be simulated fairly easily. However, the results of jury studies make a more valuable theoretical contribution to research on small-group behavior, and a more practical contribution to lawyers operating regularly in the criminal justice system, than they do to either a theoretical or practical understanding of the formation or implementation of substantive legal policies.

Loh uses three conceptual approaches to the research and policy relationship. The first looks at research that occurs prior to an important judicial decision and informs (or could inform) the judges' minds. The "Brandeis Brief" and Kenneth Clark's famous black and white doll experiment, cited in *Brown v. Board of Education*, are examples. The questions crucial to this approach are when, why, and to what extent such research influences the making of judicial policy. Generalized answers are difficult, of course, but Loh offers few suggestions or insights. The second approach focuses on research that presumably interacts with changing judicial policy over time. The Coleman report and research into public attitudes toward capital punishment are examples. This is probably the approach most in need of exploration, because such interaction is necessary if social science research can be said to have anything more than a sporadic influence on judicial policy making. Unfortunately it is still difficult to draw much in the way of conclusions about the whys and wherefores of

such influence. Impact studies—how court decisions affect other governmental agencies—constitute the third approach. Loh limits his focus to the police, and does an excellent job of reviewing the literature and offering insights and tentative conclusions.

Loh's in-depth examination of a few major public law controversies largely embodied in Supreme Court decisions is complemented by Monahan and Walker's eclectic format, which offers something for everyone. Their material goes well beyond public law, exploring such areas as the impact of malpractice decisions, trends in child custody cases, and how research is used in trademark violation suits. Sometimes, in fact, the research they reprint or cite relates to practical problems rather than court decisions. Monahan and Walker discuss the validity and utility of hijacker and rapist profiles, methods of deterring shoplifting, and the susceptibility of youth to pornography, among other things. Three major focuses organize their text: research on legal issues, research concentrating on the behavior of actors, and research about institutions and interactions in the legal process.

Both texts should prove to be valuable resources to those interested in the judicial process or in the more general area of law and society. Their simultaneous appearance is as welcome as it is fortuitous. For a long time, no one has seen fit to put together a rich compendium of law and social science material. Now we have two.

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New Communication Technologies in Politics.

Edited by Robert G. Meadow. (Washington, D.C.: Annenberg School of Communications, 1985. Pp. 145. \$15.00, paper.)

In early 1983, the Natural Gas Supply Association (NGSA) contracted with Matt Reese, a prominent consultant in targeted communications, to generate a grass roots campaign on behalf of decontrol of natural gas prices. Everyone realized that the task would be difficult; most of the congressmen on the committees that would take up this legislation represented constituencies that had little to gain from decontrol. Reese's strategy was sim-

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ple. Identify and contact those citizens in each of the 15 targeted districts who should be most sympathetic to a decontrol appeal, and then recruit them to drum up a letter-writing campaign among their neighbors. The budget for this first foray in what NGSa foresaw to be an extended public relations offensive was \$1,000,000—about \$70,000 for each targeted district. Revealing an irony of modern politics, a representative of the gas lobby confessed to a reporter, "None of these grass roots efforts comes cheaply."

Several types of respondents were identified in surveys to be susceptible to a gas producers' appeal. One whom Reese labelled the *Mid-America Blues* group was distinctive in its hostility to "politicians in Washington." Apparently, they were also held to be sufficiently distinctive demographically to justify targeting with census data. The only apparent problem in recruiting them behind the NGSa cause was their equal hostility to the oil and gas industry. Reese's recommendation: in appealing to these citizens, "Don't even hint at a connection to the gas companies if you can avoid it." Hence, the Alliance for Energy Security was born. Behind this alias, the NGSa fashioned the following message, which it sent to thousands of citizens: "Why do we go to Algeria for something we've got in [target's state]? . . . Because those guys in Washington thought they could fix prices on natural gas." These mailings were followed up with phone calls soliciting their participation in a letter-writing campaign.

By early August, over 38,000 kits of flyers and "constituent" postcards were in the hands of volunteers who were instructed to distribute ten letters each on a designated Saturday. By most indicators, the campaign failed to meet the sponsors' expectations. On the morning the volunteers were to hit the pavement, the *Washington Post* ran a front-page story unmasking NGSa. One congressman subsequently reported that some of those who had sent in the inspired postcards later wrote to say they had been duped. Another targeted representative recorded only 1,125 NGSa postcards, which thus cost about \$60 each.

Whether one finds in this story cause for alarm or reassurance in either the vigilance of the press or the inertia of the American public, it illustrates the extent to which political actors in Washington are employing modern tech-

nology to mobilize public support. *New Communication Technologies in Politics* introduces us to some of the individuals and techniques that are changing the way campaigns are waged in this country. An outgrowth of an Annenberg conference of leading communications consultants, the book is, unfortunately, ill suited to do much more than make introductions. There is too little assessment of effects on voters or candidates and no consideration of implications for democratic politics. With practitioners writing the essays, one does not find, understandably, troublesome stories such as that of the NGSa campaign. Instead, the consultants offer inflated claims for the power of this new technology and the good it achieves. Reese, for example, cites the NGSa campaign in asserting that his targeting "approach which had worked so well with candidates for office and referenda issues yielded almost incredible results when applied to get grass roots response to lobby members of Congress" (pp. 106-7). Hmm.

The only serious attempt to validate the proprietary methodologies presented in these essays is offered by Kevin L. Kramer and Edward J. Schneider in describing Custom Targets, the trade name of Market Opinion Research's (MOR) demographic targeting system. Using standard benchmark surveys rather than Claritas's preformed consumer groupings as the basis for targeting, MOR's concept appears clearly superior to that used by Reese and others. Still, Kramer and Schneider are not fully persuasive that their approach works as well as they say, or is cost effective. They show that targeted precincts did deliver a larger vote for their side, but is it because targeted voters were more sympathetic in the first place? After all, receptivity to the message is what qualified them as targets. One wonders, as must campaign managers, whether the money spent for a high-priced targeting consultant could have been put to better use in sending out more messages, even if it meant doing so less efficiently.

Despite its inherent limitations, *New Communication Technologies in Politics* offers a useful introduction to this new field of political consulting. As targeting and communications technology continue to improve, the wares offered by these consultants will become increasingly attractive to candidates and others who wish to mobilize public opinion. As cam-

paigns become better endowed, more candidates will find these services affordable. Most importantly, as the brand loyalty of voters continues to weaken, candidates will become ever more dependent on methods developed by advertising and marketing research to identify their message and to locate their supporters. For these reasons, even a book with such modest aspirations as this deserves our attention.

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Religion in American Public Life. By A. James Reichley. (Washington, D.C.: The Brookings Institution, 1985. Pp. x + 402. \$31.95, cloth; \$11.95, paper.)

The First Liberty: Religion and the American Republic. By William Lee Miller. (New York: Alfred A. Knopf, 1986. Pp. 373. \$25.00.)

"The First Amendment is no more neutral on the general value of religion than it is on the general value of the free exchange of ideas or an independent press" (p. 166). The purpose of the amendment, James Reichley goes on to assert, was partly to reserve to states the more delicate questions related to the practice and even the promotion of religion. For this reason, Reichley finds the Supreme Court's establishment cases ill conceived. Although he accepts the first two recent tests of establishment—i.e., purpose and primary effect—he thinks the Court's willingness to address these issues "was neither constitutionally necessary nor consistent with American tradition" (p. 156). Reichley attributes part of the Court's problem to a judicial "non-interpretivism" that forgets the words of the Constitution.

Religion in American Public Life begins with a review of religion from the first settlement to the founding. Next is a critical account of relevant Supreme Court decisions, followed by an extensive, effectively detailed, and richly documented analysis of the political role of religion up to the present. A final chapter discusses appropriate political roles for churches.

Reichley, a senior fellow at the Brookings Institution, combines an explicit respect for

theistic-humanistic religion with concern for democracy. In America, better-educated people commonly see formal religion as a threat to democratic values. Religion is considered untrue, provincial, and either too conservative, as in the abortion controversy, or too liberal, as in utopian solutions to nuclear problems. Against this prevalent negative opinion about religion in public life, Reichley argues that the same religions that can be dangerous are, nevertheless, essential for the very survival of democracy.

The problem for Reichley is the difficulty of reconciling cultivation of respect for persons with dedication to politics. Reichley challenges the assumption that the presence of persons with reasonable good will will suffice. There are too few such people, and if they are put in charge they will create, perhaps, a benevolent aristocracy of some sort, but never democracy. Respect for common people and their government requires, says Reichley, an independent source of authority and influence that helps people to do right and restrains their leaders. The public role of religion in America has been, at its best, to provide this foundation for liberty and order. Reichley's contribution is to discuss with great intelligence and sensitivity the role of religion within a radical and even pessimistic understanding of American democracy.

The First Liberty is a bicentennial celebration of the constitutional provisions for religious liberty. When the American people agreed that the supreme law of the land should not favor a particular religion or require a religious test for public office, they set religion free to play an expanding, and generally positive, role in American life. Miller concludes his review of the founding, colonial antecedents, and recent developments in politics and law, with a prediction that we can expect Jewish and Roman Catholic communities, especially, to make significant new contributions to our politics.

Miller has written a distinctive book designed to capture the attention of tired or otherwise inattentive readers. He uses a personal style without endnotes, and only an occasional bibliographical reference. This deliberate informality recaptures the intense feelings involved in an appropriate appreciation for the historic achievement of religious liberty. Focusing on dramatic episodes (e.g.,

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disestablishment in Virginia), unusual writings (e.g., Roger Williams's *The Bloody Tenent, of Persecution, for Cause of Conscience*), and great leadership (e.g., James Madison's work), Miller rescues this story from historical oblivion.

Miller gives dissenting Protestantism credit for establishing religious liberty and promoting voluntary public service, but then blames this tradition for much of the individualism and political indifference in contemporary America. The Supreme Court, in its recent decisions on establishment, receives a less caustic judgment. Miller weighs the complexity of current decisions involving church and state and their attendant debates. Remaining reasonably neutral, he counsels calm reflection from diverse perspectives. History and past law provide for him few normative principles for unravelling the complex ties of church and state.

Trained in religious social ethics, Miller has long been active in interfaith and interdisciplinary discussions of religion in America. A former political science professor, he is now Chairman of the Department of Rhetoric and Communications at the University of Virginia.

These volumes on American politics and religion join those by Booth Fowler, Charles Dunn, and others soon to be published, adding depth to our understanding of this no-longer-ignored, much-to-be-studied subject. Miller, who is more liberal, and Reichley, who is more radical, have given us two excellent books.

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Factions in House Committees. By Glenn R. Parker and Suzanne L. Parker. (Knoxville: The University of Tennessee Press, 1985. Pp. xx + 312. \$26.95.)

Committees remain at the very center of congressional deliberation and decision making. Until 1971, knowledge of committee processes largely reflected the reports of astute "inside observers" (e.g., Fenno and Price); in that year, however, as the Legislative Reorganization Act of 1970 mandated, committees began to record and make available their roll-call votes. Glenn and Suzanne Parker, almost alone among students of Con-

gress, have seized this opportunity to chart the character and continuity of factions in House committees. Using factor-analytic and correlational techniques to analyze the substantive (amendment) votes in 19 panels, they compare the factional alignments in the 93rd and 94th Congresses (1973-1976) with those in the 95th and 96th (1977-1980). They seek to describe the lines of committee division in terms of party, ideology, executive influence, and constituency pressure, and to account for changes in these divisions over time. In doing so, they have produced a most valuable study.

The bulk of the authors' analysis extends their pioneering paper ("Factions in Committees: The U.S. House of Representatives," *APSR* 73 [1979]:85-102). They find partisanship and ideology to be the most significant correlates of committee voting alignments. On some panels (e.g., Budget), the two factors coincide, pitting highly polarized factions of conservative Republicans and liberal Democrats against one another. On other bodies (e.g., Judiciary) ideology diverges from partisanship, producing more diffuse blocs of conservatives and liberals. Executive branch forces influence committee cleavages over and above partisanship and ideology on five committees (e.g., Appropriations). When the executive is concerned and employs its persuasive resources, committee factionalism reflects those efforts; because these efforts "crosscut" other forces, the factions on these committees are seldom tightly polarized.

Constituency factors also are significant supplements to party and ideology on three committees. Where interest groups "are interested in many issues falling within the committee's jurisdiction," they "tend to be sustained and pervasive forces within the committee's environment" (p. 158), and factions reflect support for and opposition to group policy preferences. The Agriculture and Banking committees are cases in point. For the most part, factional patterns endure across the 1970s. Committee jurisdictions change little, committee agendas are relatively fixed, and the types of members who serve remain constant, as do their voting dispositions; in consequence, voting alignments are stable. The few changes that do occur flow from the appearance of extensive membership turnover, and a new administration in the White House. On balance, however, voting factions in the

Nixon-Ford and Carter presidencies look very much alike.

In a truly ingenious analysis (chap. 7), the Parkers explore the relationship between committee cleavage and coalition formation. They hypothesize that "the more partisan loyalties of committee members are crosscut by other environmental influences (specifically ideology, administration, and constituent-group interests), the greater will be the proportion of larger-than-minimum" winning coalitions on the committee (p. 247). "Polarized-partisan" panels—those on which environmental influences reinforce partisanship—do indeed tend to form minimum-winning coalitions of between 50% and 60% of the members. "Non-partisan" committees, by contrast, tend to develop "universal" coalitions (of more than 70% of the members), and "mixed-partisan" committees, on which there exist significant crosscutting environmental forces, display winning coalitions of intermediate size. During the Carter years, coalitions tended to be smaller, suggesting that divided government requires the president to reach across party lines to secure opposition support, thus producing more universalistic voting alignments.

It is possible to quibble about some methodological choices the authors make. Their decision to examine only members who served on a committee continuously during each pair of congressional terms serves to reduce the pool of legislators in their analysis, as does their decision to eliminate those who voted on fewer than 70% of the substantive roll calls in committee. Thus, to cite an extreme case, their analysis of the Agriculture Committee in the 93rd and 94th Congresses contains only 18 of the nearly 60 members who served on the panel between 1973 and 1976. As a result, they may overestimate the stability of factions, because they consider only those representatives—the high-participant, continuing members—most likely to behave consistently over time. Conversely, the authors, because they exclude the relatively inactive, noncontinuing members who may behave more erratically, may underestimate the diversity of committee factions.

The Parkers provide graphs—"two-dimensional representations"—that "generally" locate members of the committee with respect to "the major cleavages within Congress and its committee system—party and

ideology" (p. 32, emphasis added). This implies that in some instances other factors—presumably executive, constituency, or policy influences—constitute the fundamental dimensions of committee cleavage. The failure to label the axes of these graphs prevents identification of these latter circumstances. Similarly, the authors make much of polarization of committee factions, and while visual inspection of the spatial location of members gives some clues about the degree of polarization, a statistical measure of dispersion might have made their contrasts between "tight" and "dispersed" clusters more readily interpretable.

A quantitative study of roll-call votes nicely illustrates the difficulties of specifying cause and effect. The authors, who repeatedly acknowledge the problem, often resort to inference and speculation to disentangle the impact of executive and constituency forces from that of party and ideology. They rely on personal data on committee members, possession of leadership (including subcommittee) positions, and the existence of pertinent policy divisions in the panel as evidence that a particular bloc is sensitive to administration or constituency-group pressures. They also draw on previous nonquantitative work to identify committees susceptible to such influences. These sources permit plausible, but not always altogether persuasive, arguments for executive or constituency impact on particular committee factions. In the last analysis, it is never certain whether a cluster whose members have high presidential support scores is responding to executive leadership or voting out of partisan or ideological compatibility with a president of their own persuasion. There is a moral here, of course: neither quantitative nor more conventional methods alone are likely to provide definitive answers. Taken together, however, as complementary approaches, they can offer fuller, independent, and perhaps more convincing explanations of important phenomena.

It is exactly here that the exceptional value of the Parkers's work becomes crystal clear. They have brought a new, untapped data source to bear on a set of central questions about congressional committee politics. If they have often confirmed the conventional wisdom (from an independent data base), they have also challenged that wisdom in numerous instances. Their study has identified committee

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voting patterns precisely, advanced arguments to explain those patterns, and made a distinguished contribution toward understanding coalition formation. These are no small accomplishments. Moreover, they have, implicitly at least, set a research agenda for the study of committee politics, and if scholars now take up the challenge, as they surely will, it will be because *Factions in House Committees* has led the way.

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Organized Interests and American Democracy.

By Kay Lehman Schlozman and John T. Tierney. (New York: Harper & Row, 1986. Pp. xix + 448. \$12.00, paper.)

Scholarly interest in the nature and role of organized interests in American politics, awakened by a realization that pressure groups are alive and well and, indeed, flourishing, has recently enjoyed a renaissance. Perhaps the best indicator of the subject's panache is the revival of the market for textbooks on interest group politics. After a hiatus of a decade, publishers have brought forth at least four new interest group texts in the last four years. The latest of these is *Organized Interests and American Democracy*, by Kay Lehman Schlozman and John T. Tierney.

In conception, *Organized Interests* is as much a reportage of research results as a comprehensive text. The book marks the culmination of a five-year research program wherein the authors conducted long interviews with 175 organized interests—citizens' groups, professional associations, labor unions, trade associations, and corporations. The results are the centerpiece of the book, both in presentation and in substance. Schlozman and Tierney provide a detailed snapshot of who takes part in the pressure system of the 1980s, what the organizations amount to, and what the organizations do. Their data confirm many casual observations—that pressure activity has increased, for instance—but disconfirm other notions—for example, that public interest groups have become more prominent. Researchers and teachers will find their descriptive information valuable in itself.

The Washington Representatives Survey is

also this textbook's strongest point, for Schlozman and Tierney, without making a big deal of it, have demonstrated one of the things political scientists do, namely, bring systematic evidence to bear on empirical and theoretical puzzles. As the authors note,

The realm of organized interest politics is so vast . . . that it is possible to locate an example to illustrate virtually any reasonable generalization one might put forward. Only by taking a more global view can we get a sense of the relative frequencies within this world of astonishing political diversity. (p. xiii)

The central descriptive theme of the book is that today's Washington pressure activity is "more of the same." The authors find that the number of groups in the capital has burgeoned, and that groups that were already part of the Washington landscape have stepped up their activity. The proportion of organizations representing particular constituencies, however, has stayed remarkably constant, with the notable exceptions of unions, which have declined in prominence, and corporations, whose share has jumped markedly. Moreover, they assert, group tactics remain Congress-centered and no more or less reliant upon such traditional strategems as regular contact with public officials, testimony at hearings, and grassroots lobbying.

For the most part, the authors are appropriately modest about the uses to which their results can be put, although they have a tendency to take their respondents' answers at face value. (What Washington representative, for instance, would denigrate the value of a good reputation as a political resource?) To their credit, however, Schlozman and Tierney have sought to lessen their reliance on the recall of Washington lobbyists by collecting a variety of side evidence, ranging from anecdotes to a census of interest groups active in 1960, to a clever experiment starring Schlozman's infant son. Nor are the authors so insistent upon "more of the same" that they neglect nuances in the sameness. Although citizens' groups have not become more prominent in the universe, they find, the composition of the sector has changed from dominance of civic groups to primacy of environmental and consumer groups.

As one might expect, the empirical and topical sweep of *Organized Interests* has been purchased at a price. The authors have stayed

fairly close to their data, and it does a serviceable job of covering the bases, including a few I had not expected (such as the internal democracy of groups), but it keeps them at arm's length from others. Lacking here, as elsewhere, is a serious treatment of power and influence. Granted, power is a large, digressive topic for a book on organized interests, but students must wrestle with what power and influence are if they are to avoid the promiscuous attributions of influence that so plague many popular and scholarly treatments of interest group politics.

The very scope of *Organized Interests*, moreover, contributes to its least satisfactory aspect. Schlozman and Tierney ably review and evaluate the major theories of group membership, internal dynamics, and organizational maneuverings in elections, Congress, the bureaucracy, and the courts, but they have chosen a theoretical theme that is inadequate for the task of integrating these disparate elements, and the book lacks a sense of how all the pieces fit together.

The authors pose an interpretive dichotomy. On one side is the pluralist model of pressure politics (associated with Truman) that sees government as the very expression of the push and pull of contending interests. On the other side is the revisionist model (associated with Bauer, Pool, and Dexter) that portrays organized interests as actors so put-upon and cross-pressured as to be ineffectual. The degree to which American government is the captive of particular interests is an important question; that, presumably, is why we care enough about organized interests to put so much into studying them. Yet the only defensible conclusion when the dichotomy is so posed is the one that Schlozman and Tierney arrive at—that the reality of pressure politics is far more complex than either the pluralist or revisionist model can accommodate. What we are left with, then, is complexity. In a sense, we are back to square one.

So what might have been better? It is quite unfair, after all, to expect anyone to develop the grand theory of organized interests in American politics. The answer, I think, is to be at once more limiting and more speculative. The task is to draw relational linkages—to treat interest groups as modes of citizen participation, interest groups as policy actors, and interest groups as problems in democratic

theory as related questions, instead of as discrete questions. How, for instance, might the maintenance needs of political organizations structure their dealings with government elites? How might the goals of legislators, bureaucrats, and judges shape their responses to associational demands? What are the normative implications of different answers to these two questions? In short, once we know something about what an organized interest is or does, how does that help us understand something else it is or does?

Texts on interest group politics must be both empirically and theoretically informative, and this book is. They must also show how political scientists reason about how organized interests and policy makers are likely to behave, and about what difference it makes. Students should learn not only about the complexity, but about the if/then thinking that political scientists use to draw pattern from chaos.

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Administrative Discretion and Public Policy Implementation. Edited by Douglas H. Shuvamon and H. Kenneth Hibbeln. (New York: Praeger Publishers, 1986. Pp. x + 308. \$40.95.)

The causes, uses, abuses, and strategies for controlling administrative discretion in public policy implementation have been frequent areas of inquiry and research in the literature on public administration, public policy, and administrative law. This well-written collection of 17 essays provides more grist for the mill. The editors, Shuvamon and Hibbeln, have compiled a collection of essays that address the gamut of issues raised by "administrators possessing discretionary authority" in policy implementation (p. 8).

Shuvamon and Hibbeln organize the 17 essays around six perspectives/sections. Each section contains at least one case-analytic essay and at least one essay of more general relevance. The editors' introduction and prefaces to each of the six sections provide a brief overview of each perspective. These could have been more expansive and provided more theo-

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retical links. The six sections are on (1) the legal context of administrative discretion; (2) the causes of legislative delegation of authority; (3) administrative discretion in regulation and enforcement; (4) the politics/administration dichotomy; (5) the effects of the intergovernmental system on administrative discretion; and (6) traditional and other responses to administrative discretion.

The essays in the first section explore the legal framework of administrative discretion, and would be of most interest to students of administrative law. One of the few theory-rich essays of the volume is found in the section on delegation. In this essay, Angus MacIntyre delineates 18 sources of statutory ambiguity and systematically assesses the difficulty these ambiguities pose in attaining democratic accountability.

Both essays in the third section provide substantiation of the empirical invalidity of any dichotomized formulation of the relationship between politics and administration. William Browne provides data on the policy leadership of city managers in Michigan. T. Zane Reeves recounts the 1970s struggle between ACTION political appointees and career bureaucrats over shifting the mission of the agency from an antipoverty to a volunteerism focus. In one of the two essays of the fourth section, John Scholz applies tit-for-tat contingency theory to "explain how discretionary enforcement can increase cooperative compliance" (p. 145).

The essays of the final two sections add to the extant literature on administrative discretion in policy implementation, and offer insights into the adaptive behavior of administrators, administrative agencies, the judiciary, and legislators. The intergovernmental relations selections reveal the impact of the block grant in decreasing the discretionary authority of federal, state, and local bureaucrats. In the final section on responses to administrative discretion, five essays describe and critique a "comprehensive array of political and legal efforts to assure beneficence and limit the abuses that ensue from the exercise of administrative discretion" (p. 209). Included in this section are case studies on the Interstate Commerce Commission and the Economic Development Administration; an empirical analysis of the impact of the now unconstitutional legislative veto on administrative-legislative interaction; and

more general discussions of such control techniques as the Administrative Procedure Act, Freedom of Information Act, and judicial review.

The richness and breadth of this collection are evident in its diversity of policy arenas (mental health, antipoverty, science, immigration, and economic development, to name a few); variety of types of administrator (federal, state, and local bureaucrats; city managers; street-level bureaucrats); and range of methodological approaches (case studies, descriptive typologies, bivariate statistical analyses, and game theory). In addition, the essays are well balanced. Most of the authors acknowledge that administrative discretion is problematic. However, several also attribute positive outcomes to such discretion. Noteworthy in this regard are the lucid discussions by Douglas F. Morgan, John A. Rohr, and George J. Gordon on how administrative discretion may increase government responsiveness and accountability.

This volume does not offer any tried-and-true strategies of how to structure or confine administrative discretion for better public policy implementation. It is, however, a wealth of very readable, case-specific information on one of the most important and enduring issues of public administration theory and practice.

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Constitutional Reform and Effective Government. By James L. Sundquist. (Washington, D.C.: The Brookings Institution, 1985. Pp. x + 262. \$26.95, cloth; \$9.95, paper.)

In December 1979, C. Douglas Dillon, former secretary of the treasury, under secretary of state, and ambassador to France, declared,

I very much doubt that . . . we can long . . . afford . . . the division of power and responsibility between our Executive and Legislative branches of government. . . . I have no pat answers. But I do know that until we are prepared to examine the basic structure of our federal system . . . rather than indulging in continuous . . . recriminations, our problems will

remain . . . and, in all probability, increase in severity. (Reston, James, December 23, 1979. *Where Are We Going?* *New York Times*, p. 3-13)

This speech led to the organization of the Committee on the Constitutional System (CCS) in 1981. Members combine reverence for the constitutional ideal with a conviction that the uniquely favorable circumstances of the provenance and evolution of our Constitution's particular structures have virtually disappeared.

Now CCS charter member James L. Sundquist publishes a magisterial account of constitutional origins and the history of suggested reforms, and an intensive and detailed analysis of contemporary proposals. With regard to the third topic, he deals (1) with forestalling divided government (the "team ticket" that mandates straight-ticket voting for president and Congress, bonus congressional seats to insure the winning president a majority, etc.); (2) with lengthening the terms of office (especially the 4-8-4: that is, electing the president, the House, and half the Senate—given eight-year terms—every four years); and (3) with replacing failed governments. He then discusses lesser proposals for fostering inter-branch collaboration and altering the checks and balances (including incisive critiques of the presidential item veto and of the possible rehabilitation of the congressional veto devastated by the *Chadha* decision).

In the bicentennial, this book should inform the celebration of the Constitution by insisting on a thorough reexamination of its present adequacy. I dissent, however, from Sundquist's espousal of incrementalism:

The parliamentary system represents only a source of ideas for incremental steps that might bring more unity to the American government, each such step to be considered on its own merits in terms of its adaptability to American traditions and institutions. (p. 15)

Normally, incrementalism is the proper approach to change, but in 1787 the framers radically departed from incrementalism, thereby creating a system which, if it is to be changed fundamentally, has to be approached as comprehensively as the framers did. Charles H. McIlwain, who capped his illustrious career with *Constitutionalism: Ancient and Modern* (Ithaca, New York: Cornell University Press, 1958), writes, "Among all the modern fallacies

that have obscured the true teachings of constitutional history, few are worse than the extreme doctrine of the separation of powers." He could find little historical background for the theory, "except the fancies of eighteenth-century doctrinaires." Excoriating the separation of powers for dissipating government, McIlwain stressed its departure from incrementalism: "Unlike the legal limitations in our bill of rights, it is not the matured result of centuries of trial and error."

Consider the 4-8-4 proposal as an incremental step. This might (a) consolidate elections so that voters could "learn by doing" that they are creating a government *and* an opposition, and (b) give the government enough time to develop and apply its program. However, it would not cope with failed governments for which a vote of "no confidence" is the obvious solution. Yet if we favor keeping a popularly-elected president to help insure a unified executive and a governing majority, can the "people's choice" be voted down by legislators chosen in single-member districts? Can this dilemma be resolved by educating voters to perceive the president as a leader of a governing majority rather than as the embodiment of the sovereign people? Another problem: Both history and logic tell us that only one house of the legislature can properly be vested with the vote of confidence—What to do about the Senate? Meanwhile, the vote of confidence alone would unbalance the Constitution unless paralleled by vesting the power to dissolve the government in the executive. Careful readers will note that this is not a plea for simple adoption of the parliamentary system, but rather an argument that constitutional reform is ineluctably complex. It cannot be properly approached piecemeal.

History shows the actual drafting of viable constitutions to be the work of relatively few people. Our vigorous democracy would stipulate that this small group be surrounded, informed, and interpreted by Carl Friedrich's "constituent group." Political science can lead in the fearful task of forming the constituent group by focusing intensive study and debate on the Constitution. Can we, however, sustain such prolonged national questioning without undermining that reverence necessary to the maintenance of any regime? Plato's nocturnal council (*Laws*, 10) haunts us. Yet we may be able to have our inquiry and still hold fast to

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our devotion by distinguishing the essence of constitutionalism from the particular structures devised to implement it. Again, McIlwain is a good place to start.

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The Great American Crime Myth. By Kevin N. Wright. (Westport, CT: Greenwood Press, 1985. Pp. x + 227. \$29.95.)

Crime is escalating, and the chances of most Americans being victimized by a violent criminal are extremely high. The fear of crime is a rational response by those people who are the most vulnerable to crime. To combat the crime problem, we need a no-nonsense, punitive strategy, and must allocate even greater resources to the agencies of the criminal justice system. Are these propositions factually correct?

Based on a thorough synthesis of the empirical literature on crime, victimization, fear of crime, and the operational capacities of the criminal justice system, Kevin N. Wright concludes correctly that these propositions are among the 11 myths about crime and criminal justice in America (see p. 9 for a full listing of the myths). Wright's lucid debunking of these myths is coupled with a straightforward explanation of the realities of crime and criminal justice in America.

For example, only 1 in 100,000 Americans are murdered by strangers, and less than 2 people in 1,000 are robbed by strangers and seriously injured. Wright points out that, compared with other life-threatening situations, a citizen's life is safe from victimization by a violent stranger: "You are more likely to die as a result of a motor vehicle crash (sixteen times), a fall (four times), drowning (two times), or a fire or burn (two times)" (p. 48).

When he looks at victimization data, Wright shows that those who are the most vulnerable and the most fearful of crime—the elderly and adult women—are among those groups least likely to experience victimization (pp. 57–62). In fact, like so many social problems, street crime is a dreadful threat to young, black, male inner-city residents. Despite broad-based

fear of street crime, it is a targeted problem.

Why, then, is there such fear of crime among the broader public? Wright points to three sources: politicians' quest for easy votes; the American news media's competitive drive for subscribers and audience shares to boost advertising dollars; and criminal justice administrators' search for bigger budgets. These sources "exploit the public by sensationalizing crime" to pursue their organizational and self interests (p. 191).

Although crime is hyped and most citizens are unlikely to fall prey to a street criminal, this brand of crime is a grave threat to inner-city residents, particularly the young. Will a more punitive approach to crime and the allocation of more resources to the criminal justice establishment bring this threat under control? Wright points out that many of the "get-tough" strategies assume incorrectly that the only motivation for street crime is easy gain, and that increasing the costs of crime will lead people to refrain from such activity (p. 179). He also shows how difficult it is for the criminal justice establishment to implement strategies based on deterrence and incapacitation. For example, despite the enormous growth in resources allocated to law enforcement agencies, the certainty of detecting criminal behavior is far too low to make formal punitive measures the answer to America's crime problem (pp. 112–25).

While employing a different method, Wright arrives at the same conclusion reached by many critical criminologists who concentrate on the historical processes and structural forces that shape America's definitions and responses to crime. Developmental processes in the United States have eroded informal controls and increased the reliance on formal punitive controls. The dependency on formal sanctions and governmental agencies to enforce the social order will be of limited use in reducing the incidence of street crime in a heterogeneous culture that openly promotes exploitation, material achievement, competition, and individual autonomy (pp. 181–202). For some in our society, unethical and illegal business practices evolve as a strategy for achieving these norms. For others, street crime, particularly property-related crime, serves as a vehicle for responding to powerful cultural cues. Wright sees this as inevitable for the predictable future, and claims that the criminal justice

system can do no more than it is already doing to bolster the social order.

Is Wright correct in concluding that we are achieving all that we can with the formal criminal justice system? Closer analysis of the inner workings of criminal justice institutions indicates that we can do more to improve the work environment for thousands of street-level employees, who can make a difference in handling the clients of the criminal justice system. Furthermore, much more can be achieved to reduce the tensions between inner-city residents and the agencies of criminal justice.

Also, Wright fails to consider the benefits of reducing our society's heavy reliance on the criminal justice system and other formal punitive methods for managing social problems. A number of "crime" problems require broader definitions and the redistribution of resources to enable government to play a positive role. Even criminal justice administrators are coming around to this position with regard to the drug problem in America. With hundreds of millions of dollars added to the budgets of law enforcement agencies in recent years to elimi-

nate the drug problem and few results, some criminal justice administrators are openly concerned about taking "a problem that is a medical, social and law enforcement problem, and holding law enforcement responsible for solving the whole problem by itself" (Leslie Maitland, "President's Antidrug Task Forces Are Falling Behind in Organizing," *New York Times*, May 1, 1983, p. 1).

Kevin Wright's book represents an excellent source for framing classroom discussion of criminal justice policy in the U.S. By combining his work with the conservative writings of James Q. Wilson, who argues that punitive measures work and criminal justice agencies are capable of exercising even greater control over the crime problem, students enrolling in the growing number of political science courses devoted to the living law and/or criminal justice would be exposed to the major, contrasting views on these topics.

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COMPARATIVE POLITICS

Politics in Israel. The Second Generation. By Asher Arian. (Chatham, NJ: Chatham House Publishers, Inc., 1985. Pp. xii + 290. \$25.00.)

What Makes Israel Tick? How Domestic Policy-Makers Cope with Constraints. By Ira Sharkansky. (Chicago: Nelson Hall, 1985. Pp. xii + 184. \$22.95, cloth; \$9.95, paper.)

Israel: The Partitioned State. A Political History Since 1900. By Amos Perlmutter. (New York: Charles Scribner's Sons, 1985. Pp. xiv + 398. \$19.95.)

Here are another three books about Israel, and as always, the question is, Why do we need so many? I suppose there is some happy conjunction between authors and publishers. All three of the authors under review are well

known in the United States, and their collected corpus of scholarly output is impressive indeed. All three have American educations, residences, and experiences, and all have lived in and worked in Israel. Their credentials are high. Nevertheless, the three books are not of equal quality or value.

Arian's *Politics in Israel* is probably the best book on Israeli domestic politics that I have read out of several hundred over a period of 38 years. A quick perusal reveals how little the average American with a real interest in Israel knows about the intricacies of politics there. A close reading makes some subtleties apparent that are difficult to glean from the average American university course (or text) on the Middle East.

As Arian points out, "Political scientists who compare political systems find difficulty in fitting Israel into their schema" (p. 1). Israel

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is unique in so many ways. It is a democracy, yet like no other in the world. Certainly it does not resemble that of the United States. It has political parties, but none has ever won an election as we understand the term. Herut is a party; it is also the core of Likud, which is an electoral coalition. In order to govern, Likud is forced into a government coalition. Parties and their coalitions are tied to other institutions; the relationship between the Labor party (and coalition) with the "associational interest group" Histadrut is particularly interesting and profoundly important. The result is that every politician in Israel plays a variety of roles—bureaucratic, associational, ideological, and parliamentary. Burdened with a miserable electoral system (characterized by list-system public relations), a growing ethnic division between Sephardim and Ashkenazim, generational gaps, perennial uncertainties among the electorate, and an ubiquitous military presence, it is a wonder that any Israeli government can survive and accomplish policy objectives. A close examination of the daily crises of the Peres government (itself certainly a peculiar variation of party democracy) should be revealing enough.

Arian's book explains much of this and more. I am most impressed with his analysis of Israeli democracy as an institution, and of the labyrinthine connections and interconnections of oligarchical and hierarchical organizations with party posturing and success. It is an enormously complex system understood by too few outside Israel. Arian's *Politics in Israel* is a superb book, masterfully crafted.

Sharkansky's *What Makes Israel Tick?* (what an unfortunate title) is a useful complement to Arian's book. There is not that much overlap. Sharkansky defines the focus of the book as "policymaking for domestic activities" (p. 4), and in general he keeps to this task very well. The book is occasionally marred by such enthusiastic clichés as the following: "The desert has been made to bloom. Forests have taken root on slopes made barren by earlier inhabitants" (p. 5). It takes nothing from Israeli achievements to acknowledge that deserts bloomed in Palestine before there were Israelis there. Nonetheless, the book makes a positive contribution to our understanding of such disparate subjects as how Jerusalem is governed and how budgets—such as they are—are put together and manipulated.

Although they constitute only a small part of the book, Sharkansky cannot resist sharing with us his views on "warfare" and welfare (see chap. 9). Although these topics do relate to policymaking, and certainly affect domestic politics, the strength of Sharkansky's book, like Arian's, lies in its analysis of how the system works (yes, how it ticks), rather than in verbiage about Israel's defense posture, or peace prospects . . . or wars. Nevertheless, this is a good book—a quality book, as its publisher suggests on the cover.

I really have the least to say about Perlmutter's *Israel: The Partitioned State*. Perhaps I can begin by carping about the title. It suggests that Israel is a state that has been partitioned and that somehow it has been in existence since at least 1900. Since its creation in May 1948, Israel has not endured any partitioning, although Palestine *as an area* has. However, Israel and Palestine are not the same. To suggest that Israel had a history in 1900 is a little absurd. What had a history in the early years was Zionism due to a growing population of Zionist settlers (Yishuv) in Palestine and their relations with a British mandatory administration. These things in the title suggest the orientation of the book. The history of Israel (and of Zionism) and its wars with the Arabs has been written about so exhaustively—and by Perlmutter, for that matter, in other times—that there is no crying need for this book. It does not illuminate as many dark and interesting corners as do both Sharkansky's and Arian's books.

Few books are without value, and Perlmutter's is no exception. If Perlmutter has a specialty, it is the military and its games (Arab as well as Israeli). His views on subjects like these are noteworthy, even if one disagrees with details. His discussion of the Begin period is very good. Indeed, in my opinion, the real value of the book begins with section 4, which treats the period from 1973 to the present. I might add that of the three books, Perlmutter's is certainly the most colorfully written, and consequently, the easiest to read. Yet, I can only admonish readers of this book to exercise critical judgment as they are swept along by its imagery and kaleidoscopic metaphors. Many of Perlmutter's views lend themselves to alternative interpretations, and this work is hardly definitive.

All in all, one very good book, one good

book, and one not-bad one can be added to the bulging shelf of books on Israel.

CARL LEIDEN

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Two Political Worlds: Parties and Voting in British Columbia. By Donald E. Blake. (Vancouver: University of British Columbia Press, 1985. Pp. x + 205. \$15.95.)

Donald Blake has written an exceptional book on the dual nature of the party system of Canada's most western province, British Columbia. Within this province's two-party system, Canada's two major national parties, the Liberals and the Progressive Conservatives, achieve almost no electoral success. The party system at the federal level includes three parties: the two major parties and the New Democratic Party, which is a principal participant in the provincial electoral context. To explore this phenomenon, the book

presents a detailed look at the inhabitants of these two political worlds. It traces the evolution of the two party systems, seeks to identify the dimensions of conflict which structure them, and analyses the behaviour of voters who participate in them. (p. 1)

The book has several significant virtues. At the outset it can be noted that the empirical findings derived from "the first [1979] comprehensive survey of the political attitudes and behaviour of British Columbia voters" (p. ix), supplemented by survey data from national election studies, are integrated within a sound theoretical and contextual framework. This analysis does not ignore the importance of the historical sociopolitical milieu within which political behavior functions. Additionally, the findings deal with three significant questions pertaining to Canadian voting behavior: class voting, western alienation, and divided partisanship. The analysis of class voting goes beyond "a simple dichotomous explanation" (p. 111), to a more sophisticated treatment of class that recognizes the impact of the workplace and its environment in shaping the behavior and attitudes of individual voters. The common assumption that British Columbia's divided loyalties in voting behavior are the result of western alienation is challenged,

and that "alienation is probably an inappropriate label" and that better words would be "discontent, policy grievances, or partisan rivalry" (p. 133) may be more appropriate, adds not only to a better understanding of British Columbia politics, but also to a better understanding of nature of regionalism within the Canadian federal political system. Finally, the book's major assertion—that the divided loyalty of British Columbia voters is not the result of "instability" in party identification, but rather the consequence of divergent "structural, institutional and strategic bases" (p. 173)—is a breath of fresh air that helps us comprehend why voters switch parties between federal and provincial elections.

The weakest part of the book deals with the question of alienation. While Blake examines several dimensions of what he refers to as *alienation*, he never defines the concept precisely. Words such as *allegiance*, *discontent*, and *grievances* are used interchangeably when referring to alienation. This lack of conceptual clarity detracts greatly from Blake's findings on the role of alienation in the province of British Columbia.

All in all, however, this concise book is a first-rate piece of research. It will serve as the model for future studies of the two political worlds of British Columbia politics.

ROBERT KRAUSE

University of Windsor

Political Change and Underdevelopment. By Vicky Randall and Robin Theobald. (Durham, NC: Duke University Press, 1985. Pp. ix + 219. \$30.00, cloth; \$11.95, paper.)

Third World Politics: An Introduction. By Christopher Clapham. (Madison: Wisconsin University Press, 1985. Pp. 197. \$25.00, cloth; \$12.50, paper.)

These two concise volumes both offer assessments of the state of knowledge about politics in the diverse "Third World." Randall and Theobald offer a survey and critique of major theories of political change in Asia, Africa, and Latin America. Clapham offers his own integrative interpretation of the nature of the state and its implications for politics in the Third World. Both books are remarkably wide ranging within a brief span of roughly two hundred

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pages. Both share an eclectic, well-balanced, integrative spirit that takes account of, but is not bound by, various theoretical approaches.

Randall and Theobald's survey begins with the "modernization" theories of the 1950s and 1960s, with their teleological and dualist assumptions. The authors then review the major early challenges to these assumptions: the discovery of the durability and flexibility of "tradition" and the need to give more explicit attention to the requisites for establishing and maintaining political order. A discussion of dependency theory then expands into a consideration of more recent neo-Marxist and related non-Marxist political economy approaches. Interspersed with capsule accounts of major contributors to each of these schools of thought are illustrative applications to specific cases. Throughout, the authors offer their assessments of the strengths and weaknesses of each approach.

Inevitably, informed readers will take exception to some specific judgments or interpretations. For instance, I disagree with the assertion that those analysts of the late 1960s and early 1970s who focused on the problem of political order were also admirers of military government and ignored the risk of the abuse of state power. In general, however, Randall and Theobald do an admirable job of presenting and assessing the evolution of thinking about politics in the Third World over the past 35 years.

Clapham's slim volume, in contrast to Randall and Theobald's, does not seek to assess others' theories. Nor, by his own account, does he attempt to formulate his own theory. Rather, his goal is to provide "a sense of how things fit together" (p. 9)—that is, the common strands in the complex and varied kaleidoscope of Third World politics. He focuses on the nature of the state, and argues that the feature most clearly distinguishing Third World states is the weakness of shared social values and public goals that would provide a legitimate purpose for state power. As a result, the state and its "owners" rule largely in their own interests, and the state, being illegitimate, is fragile. The absence of unifying goals reflects the artificial character of most Third World nations, as do the effects of past (colonial and quasi-colonial) and current penetration by external forces. In the absence of legitimate state goals, neopatrimonialism becomes the

main mechanism for regimes to maintain support. Clapham traces the ramifications of this analysis for the management of the economy, external political relationships, internal political-military relations, and revolutionary tendencies (or the lack thereof).

The book is really an interpretive essay, incorporating many of the themes of recent and not-so-recent analysis, and sprinkled with neatly formulated insights. Conceptually, its major weakness seems to me the failure to consider the strategies available to governments (other than variations on the neopatrimonial theme) for strengthening national coherence and building a consensus on legitimate goals and functions of the state. Clapham's discussion of strategies available to ruling coalitions centers wholly on various groups within the population and on different styles and tactics available to rulers, ignoring the content of policy. The end result is a pessimistic and static aura that, while it captures part of reality, omits other crucial aspects.

Both books are intended as teaching tools. Randall and Theobald succeed more fully, in the sense that their volume can stand on its own, and will be valuable at the introductory and intermediate levels. Clapham's volume is subtitled, *An Introduction*, but it might in fact be difficult for those with little prior knowledge of Third World politics to grasp. In contrast to Randall and Theobald's illustrative case materials, often developed over several pages, Clapham simply refers to cases—a technique meaningful only to those who already are familiar with the cases. Used in combination with more detailed case studies, however, this book could be an excellent teaching tool.

Randall and Theobald suggest, in closing, an agenda for research that bears on the weak point in Clapham's discussion. Both volumes stress the authoritarian nature of most Third World states, and both are thoroughly skeptical about the mechanisms earlier theories thought would produce either democratic evolution or revolution. However, these are not the only possible patterns of progressive change, in the sense of movement toward closer and less exploitative integration between state and society. Randall and Theobald call for research that would distinguish different patterns of authoritarianism and seek to discern conditions and paths of change, particularly progressive change. That agenda grows

out of, and should be informed by, the various approaches their book reviews, but it redirects attention to reflect more fully the realities of much of the Third World. I do not regard it as ethnocentric to argue that such an approach may also better serve democratic values.

JOAN M. NELSON

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The Political Dilemmas of Military Regimes.

Edited by Christopher Clapham and George Philip. (Totowa, NJ: Barnes & Noble Books, 1985. Pp. 282. \$28.50.)

In addition to restoring law and order after a crisis situation, military regimes, once in power, face the added burden of having to remedy societal ills. Military regimes are confronted, however, with the conflict between their own requirements and the needs of a given society. As the authors writing in this edited volume ask, What can military regimes do with their power once in office?

Clapham and Philip set out to identify the political dilemmas facing military regimes. Both authors assert that the choice, although not unique to such governments, is between institutionalization and demilitarization. Types of military governments are classified into four groups: *veto*, *moderator*, *factional*, and *break-through* regimes. This concise theoretical discussion is useful, since it leaves the reader with a solid framework for understanding the role of power and how it is exercised by military regimes. However, the editors also acknowledge that the political conduct of military regimes is more a matter of "circumstances" than deliberation, given their desire to cushion the military from civil society (p. 24).

In his discussion of the veto regime in Greece between 1967 and 1974, Thanos Veremis illustrates the military's dilemma quite well. Seven years in office had created a power vacuum, and the military collapsed under its own weight because it did not know what to do with organized civilians. Bener Karakartal's study of the Turkish military, on the other hand, reveals the existence of a high degree of internal unity, in that the military command structure was differentiated from civil society. In Ankara's case, the military acted as a

moderator influence, encouraging the return to civilian rule as soon as possible. Yet, the election of Prime Minister Ozal, while supporting "the longstanding separation in Turkish political culture between the military and civilian parties" (p. 60), did not mean the elimination of the military's influence, a role more comfortably played behind the scenes.

Authoritarianism in the military is also discussed by Guillermo Makin, who asserts that junta members in Argentina have used varying methods to achieve varying goals with varying results. This experimentation and the lack of progress may have forced Buenos Aires to revert to civilian rule. Moreover, the election of Alfonsín to the presidency was expedited by numerous military blunders, creating the impression of power where none existed. Nevertheless, whereas the South American cases (an overview is presented by George Philip) indicate that relative independence does exist between society and state, this is not the case in Central America. James Dunkerley argues that the military's overall inability to accomplish even minimum political objectives has resulted in failures whose outcomes remain obscure. Indeed, unfolding and bloody Central American revolutions stand in testimony to corrupt and purposeless military regimes.

In contrast to the Central American cases, the sense of purpose is very much evident in several Asiatic states, where the military strives to maintain internal unity. Ulf Sundhaussen and Barry R. Green assert that the Indonesian regime stands as a relative success story, since it "appears to be intent on hanging on to [power] at all costs" (p. 103). Soeharto is credited with an improved economy but, perhaps more importantly, with the political savvy of persuading minority religious parties to abandon any political ambitions. Alternatively, the Soeharto regime has succeeded by offering more political rights to most people, "provided they [were] neither communists, Islamic fundamentalists, nor separatists" (p. 109). Indonesia still faces an uncertain future, given the absence of a strong successor able to take over from Soeharto.

Gowher Rizvi's study of Pakistan and Bangladesh discusses how the military in both countries found itself in an untenable position, having alienated the principal civilian organizations and created a clientelist relationship with various groups. Nevertheless, the power

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of the masses never eroded, leading a figure like General Zia ul-Haq to accept an eventual return to civilian rule. In fact, the recent restoration of civil rights in Pakistan further illustrates the degree to which power remains elusive.

By far the two most interesting studies in this book are those of the breakthrough regimes in Ethiopia (Christopher Clapham) and the Sudan (Peter Woodward). In Ethiopia, the difficult path pursued by the state has led to civil and economic disasters. In the Sudan, factional clientelism has kept the Numairi regime in power for over 15 years. Yet, as Numairi recently discovered, real power has always rested with the military, not fledgling civilian institutions, and his refusal to acknowledge this source of ultimate power led to his removal from office.

This is an important study of several military "establishments" where contrasting values of power guarantee successive coups, whether violent or not. Readers will learn a great deal more than the theoretical framework, since the factual discussions are colorful and discriminating.

JOSEPH A. KETCHICIAN

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Electoral Change in Western Democracies: Patterns and Sources of Electoral Volatility.
Edited by Ivor Crewe and David Denver.
(New York: St. Martin's Press, 1985. Pp. iii + 438. \$29.95.)

After a long period of stability, western nations seem to have been experiencing substantial electoral change in recent years. This text focuses on a salient characteristic of the change: the volatility of western electorates, defined principally as interelection shifts in the vote. The book contains 13 case studies—of the United States, Canada, Australia, Great Britain, Scotland, Ireland, France, West Germany, Austria, Italy, Belgium, The Netherlands, and Denmark—written by leading national specialists, plus an introduction and a conclusion prepared by the editors.

The principal strength of *Electoral Change in Western Democracies* is that it draws together under one cover a set of national case

studies of uniformly high quality. Some of them, such as the excellent summary of electoral politics in Britain by Ivor Crewe, cover familiar ground. Others present new data and interpretations regarding electoral change. Particularly noteworthy among the latter are the chapter by Marsh on the stability of Irish voting patterns; Haepfer's analysis of the emergence of a small candidate-centered floating vote, located among well educated groups, in Austria; Allum and Mannheim's demonstration of the attenuation of the social and communist/anti-communist base of Italian voting; Van Der Eijk and Niemoller's analysis of the growth of ideological voting in The Netherlands; and Borre's development of a model of fragmentation/realignment for Denmark.

A second strength of this volume derives from the common framework developed by the editors to guide the preparation of the national case studies. Each country's chapter addresses changes in electoral volatility since 1945 (employing the Pedersen measure of net volatility); explores sociological, ideological, and government/party performance explanations for these changes; and considers the relationship between electoral volatility and the party system. In the few cases where the appropriate individual-level data are available, gross volatility (i.e., individual vote change) is also examined.

The organization of each national study within a common framework promotes cross-national comparisons and gives this volume great potential for uncovering whatever general patterns may exist in processes of electoral change throughout the western world. The primary generalization, however, is that the western democracies have responded to substantial social change in diverse ways; i.e., no predominant electoral pattern has emerged. Some electorates have exhibited increasing levels of electoral volatility, but others have shown striking vote stability in the face of significant societal change. The common denominator of electoral change instead is what has been lost rather than gained; there is a weakening of the traditional social bases of their party systems as reflected mainly in the declining relationship between social class or religious identification and the vote.

That the potential offered by a common framework is not more fully realized may be

attributable to the particular framework employed and not the inherent uniqueness of each nation. Electoral volatility is not a highly useful concept for summarizing electoral patterns. It is only one aspect of electoral change and probably not the central one because of its sensitivity to electoral arrangements as well as to voter behavior. More importantly, electoral volatility has *not* been increasing in a majority of the nations selected for study. The common framework forces many of the authors into the awkward position of trying to explain a non-event and inhibits the pursuit of more profitable lines of analysis.

The operationalization of volatility also is troublesome. The aggregative Pedersen index is a poor measure of electoral change, especially in multiparty systems (see, e.g., Grunberg on France, Mughan on Belgium, and Van Der Eijk and Niemoller on The Netherlands). This weakness led the editors to include gross volatility in their framework, but the absence of individual-change data limits its application to only a small number of elections—and most of them only by using recall data.

Nonetheless, in spite of limitations in its overarching conceptual framework, *Electoral Change in Western Democracies* is a valuable contribution to the study of comparative politics. The relationships between societal change and citizen-voting behavior are complex in any single nation, and the case studies in this volume clarify these relationships well for their particular country. By adopting a common theoretical approach to each case, moreover, this volume advances our understanding of how voters in different electoral contexts respond to similar forces for change—a considerable achievement in a world that inevitably contains more causal factors than observations.

PAUL ALLEN BECK

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The New Select Committees: A Study of the 1979 Reforms. Edited by Gavin Drewry. (Oxford: Clarendon Press, 1985. Pp. xiv + 410. \$29.95.)

The British Parliament of 1979–83 was described by one leading member as a “reform Parliament.” Under pressure from back-

benchers on both sides of the House, significant structural and procedural changes were enacted. Foremost among these was the appointment of 14 investigative select committees, set up “to examine the expenditure, administration and policy of the principal government departments . . . and associated public bodies.” The committees have established themselves as major features of the parliamentary landscape. Yet what political impact have they had? The question is one much to the fore in the discussion of parliamentary efficacy and reform. It is a question that Gavin Drewry and other members of the British Study of Parliament Group seek to answer in this substantial volume.

The book constitutes a comprehensive study of the committees in the first parliament of their existence. There is a chapter on each of the 14 committees, plus two introductory and five analytical concluding chapters. There is also a chapter on committees in the House of Lords, though the rationale for its inclusion—given the absence of chapters on other Commons’ select committees, such as the Public Accounts Committee—is not clear.

The chapters on the individual committees adopt a broadly similar format, covering membership, inquiries, mode of operation, reports, and impact. Each is clear and readable, incorporating much useful factual data. They nonetheless fall foul of two inherent difficulties. One results from the constraints of space. Each chapter has, of necessity, been kept short. Contributors are consequently unable to provide little more than factual summaries. There are no sustained case studies and little anecdotal material that would allow the reader to get the feel of the committees’ work. Constraints of space also appear to account for some important features and problems of particular committees being skimmed over or not even mentioned. (The chapter on the Foreign Affairs Committee fails to mention the controversy engendered by Conservative disquiet over the operation of its Overseas Development Sub-Committee; only the editor, in a later chapter, touches upon it.) The second problem arises from the nature of the committees themselves. They are multifunctional bodies. From the beginning, they have differed significantly in the emphasis given to particular functions. Many of these functions entail seeking to exert “influence.” How can

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one empirically verify what influence has been exerted? In most cases it is near impossible. The contributors have to fall back on generalized and intuitive assessments of the committees' impact, some doing so in such a generalized way as to be not terribly helpful to the reader.

The concluding chapters constitute the best part of the book. In the penultimate chapter, Philip Giddings skillfully draws out the multi-functional nature of the committees and places the attempt to fulfill those functions in the context of the wider parliamentary and political environment. He argues that the committees are most effective in ensuring greater "accountability" on the part of government: ministers and officials are sensitive to the fact that what they do may be the subject of committee investigation. This point is reinforced by the editor in the final chapter. Both writers argue convincingly that the committees more than justify their cost. Like the institution of which they are a part, the new committees provide good value for money.

PHILIP NORTON

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The Future of the Soviet Economic Planning System. By David A. Dyker. (Armonk, NY: M. E. Sharpe, Inc., 1985. Pp. 172. \$25.00.)

The Soviet economy has increasingly been plagued by shortcomings and inefficiency in the ongoing process of shifting from an extensive to an intensive mode of production. David A. Dyker convincingly explains the seemingly inherent weaknesses of Soviet economic planning, which often result in lower-than-expected output targets. *The Future of the Soviet Economic Planning System* is a short, but highly sophisticated and concise treatise on the historical evolution, practice, and future prospects of centralized planning in the Soviet economy.

Stalin's economic growth strategy of output maximization, though necessary perhaps to achieve rapid industrial growth, resulted in waste of capital because of "gigantomania"; overbidding for investments in order to keep resources flowing; and the tendency toward organizational autarky because of supply

uncertainty. Moreover, Stalin's approach to economic planning firmly implanted two key principles in Soviet planning tactics whose basic premises appear still to be at work today, despite a number of reform attempts. The "ratchet" principle implies that central authorities establish their plan targets by extrapolating from past input and output trends because of insufficient or untrustworthy reports on enterprises' production capacity. This in turn induces enterprise directors to try to achieve even but very modest plan fulfillments. The "Micawber" principle is an element of the crude growth maximization pattern by rewarding plan overfulfillment and punishing even the slightest underfulfillment.

Attempts at reforming the planning system began shortly after Stalin's death. Unlike Khrushchev's "peacemeal, disorganised and disorganising" changes (p. 42), however, only Kosygin's 1965 reforms did result in higher capital productivity, even though labor productivity remained stagnant. Most reforms appear to have failed because the overall system of Soviet planning has remained highly centralized, despite some "marketization" in peripheral sectors, and the principle of *khozrashchet* (business accounting) has only been introduced in low-level enterprises and not in intermediate planning bodies. Furthermore, events in Czechoslovakia alerted Soviet leaders to the political dangers of radical economic reforms, and the tremendous increase in oil, gold, and timber prices sparked the (largely false) hope that imports of Western technology could somehow substitute for economic reform. The post-Brezhnev leadership may, however, have the greatest difficulties in coming to grips with the underlying domestic problem of over-centralization, even though Soviet academics appear to recognize and acknowledge its detrimental effects.

The Soviet economic planning system has been plagued with a high degree of inertia that makes it virtually immune to any radical planning reforms. Three factors in particular impose barriers to fundamental change. First, ideological conservatism among leaders results in the pursuit of quantitative, computational improvements of centralized planning tasks. Second, a powerful party apparatus at the local and intermediate levels is extremely suspicious of, if not hostile towards, any autonomization of the economy. Third, the military

sector has tremendously benefited from the "priority principle," whose very existence dictates a command economy.

The analysis by David A. Dyker and the well-documented evidence of weaknesses and shortcomings in the Soviet economic planning system pose a powerful argument against any high expectations of radical and rapid reforms. The only pitfall this study may contain is an underestimation of Soviet economic achievements within a relatively brief time period, and in the face of numerous setbacks that wreaked havoc upon the existing industrial production capacities. Ignoring Soviet economic successes could, in turn, lead to the erroneous assumption that radical reforms towards "liberalization" are the *sine qua non* for the system's survival. The leadership may well continue to "muddle through," particularly if we keep in mind that the economic planning system has shown a tremendous tenacity in the face of external and internal threats.

Nonetheless, Gorbachev's avowed aim of cracking down on inertia and political privileges and the actions taken so far must be regarded as a serious challenge to the existing system of economic planning and policy implementation. *The Future of the Soviet Economic Planning System* will serve as a benchmark against which the success of Gorbachev's reform movement may be evaluated.

BEAT KERNEN

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Politics Against Markets: The Social Democratic Road to Power. By Gösta Esping-Andersen. (Princeton: Princeton University Press, 1985. Pp. xx + 366. \$32.50.)

Why have working-class, political parties been so successful in Scandinavia? Why have they suffered stagnation or decline in popular support during the past two decades? What, if anything, can be done to rebuild support among changing national constituencies? These are analytically interesting questions, but they are also vitally important to the future of the welfare state. Gösta Esping-Andersen's new book, *Politics Against Markets*, offers answers that are at once thoughtful and thought provoking. Along the way, he provides a sophisticated historical comparison

of relationships among class structure, party systems, and public policies in Denmark, Norway and Sweden. Anyone interested in the future politics of welfare states will find Esping-Andersen's argument both rich and rewarding.

In Esping-Andersen's view, most theories that seek to account for the development of working class parties have "concentrated almost entirely on class structural change, in the belief that the logic of labor movements springs from the nature of the social structure" (p. 4). Esping-Andersen, however, believes such theories to be entirely too deterministic and offers an alternative, which is that the development of social democratic parties "depends on the impact of state policies on the class structure" (p. 26). Although the occupational structure in each nation obviously sets parameters for the possible, policy choices made by governments controlled or influenced by working-class parties can either promote or retard the continued development of such parties. In all three countries, for example, the existence of an independent and politically aware peasantry was an important constraint against development of a unified conservative opposition. More importantly still, farmers and peasants were willing to form alliances with urban social democrats in the 1930s in order to secure economic benefits for themselves. These worker-peasant alliances provided firm political foundations for three decades of welfare state initiatives that, in reforming government social policies, stimulated growing political support for Scandinavian social democrats.

To document his argument, Esping-Andersen leads the reader through three areas of policy development in each nation: social assistance, housing, and macro-economic policy. Valuable as sources of information, these chapters have the additional virtue of demonstrating the considerable political differences between these nordic neighbors. Denmark, where agriculture and skilled crafts remained dominant until the 1960s, moved less rapidly toward universal entitlement programs, pursued housing policies that divided renters and owners, and used tax and incomes policies that maintained, rather than reduced, income inequalities. Under these conditions, bourgeois party support grew and left-wing social democrats withdrew to splinter parties.

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In Norway, social democrat-bourgeois consensus on welfare state programs was fueled by solidaristic housing policies that offered all citizens an opportunity for government loans at favorable rates and by massive government participation in credit markets. Sweden provided the model for transition from a workers' to a peoples' party. Thus it led the way in institutionalized entitlements for all citizens, renewed the national housing stock while offering subsidies to renters that matched tax breaks available to home owners, and developed an "active labor market policy" that encouraged both labor mobility and greater productive efficiency. Social Democrats in both Denmark and Norway were badly split by the European Economic Community (EEC) issue. The Norwegian party, buttressed by solidaristic housing and economic planning policies, was able to recover to some extent while the Danish party has continued to struggle. Swedish social democrats, on the other hand, continue to enjoy support among all population segments, although support has not increased in the past decade.

Documented by painstaking historical research, Esping-Andersen's argument that government policies had decisive effects on social democratic support persuasively challenges more simplistic theories of class causation. His analysis of the present dilemmas of Scandinavian social democracy is equally persuasive. The institutionalized entitlements of Nordic welfare state programs are not only expensive but, the more universal they become, the greater the need to raise additional tax resources to fund them. Since middle-income taxpayers provide most of the tax revenues in each nation, their support is essential for improved welfare programs. As taxes increase, however, middle-class support declines, unless social democrats are able to devise effective programmatic responses. Swedish social democrats found such a response in their pension supplement program of 1959, but the Norwegians have had less success, and the Danish party has had almost no success at all. The future of social democracy requires alliances with middle-income taxpayers, but traditional social democratic policies appear to render such coalitions impossible.

Is there a solution to the social democratic dilemma? Absent any other ideas, Esping-

Andersen suggests that the Swedish proposal for "economic democracy" is the key to party renewal. Developed largely by Landsorganisationen (LO), the central labor confederation, the Swedish proposal for wage-earner investment funds has the potential to structure a new coalition between blue-collar and white-collar workers. For labor, ownership shares in corporate profits could dampen pressure for higher and higher wages, thus relieving social democratic governments of the difficult problem of imposing wage restraints on their strongest supporters. For middle-income taxpayers, the availability of large funds for capital investment could provide a resource to maintain productivity and growth, thus relieving them of the burden of ever-increasing taxes. For the nation as a whole, implementation of the plan could provide more effective tools for macro-economic planning to complement worker-participation schemes already in place. Together, these benefits could secure an electoral base for the social democrats into the foreseeable future. Esping-Andersen acknowledges that the plan has little support beyond the LO membership and strong opposition from the bourgeois parties in Sweden. Nevertheless, he regards the wage-earner fund proposal as "the only existing plan that permits labor to escape from the economic dilemma in which it finds itself" (p. 310).

Although Esping-Andersen is clearly committed to the rejuvenation of social democratic parties in Scandinavia, it is not clear why. In Norway, for example, welfare state programs were a product of consensus among the social democrats and their bourgeois party colleagues, while the Swedish bourgeois parties continued to support such programs during their recent six-year (1976-82) hold on the government. If social policy is the metric, it is difficult to understand why one party should be preferred to others. Or, to take another example, figures reported by Esping-Andersen (pp. 239-41) show that per capita Gross Domestic Product (GDP) and real manufacturing wages grew much more rapidly in Denmark than in Sweden from 1960 to 1979, while inflation was only modestly higher in Denmark (10.7% vs. 10.4% from 1974 to 1979). If economic policy is the metric, strong social democratic control in Sweden was less effective than the market-oriented activities of Danish politicians. Unemployment has been

considerably higher in Denmark than in either Norway or Sweden during the entire post-war period, of course, but Esping-Anderson does not say whether or not he believes full employment to be the supreme criterion of judgment. Indeed, the various performance measures he reports are so varied that use of a single evaluative criterion would make little sense. How, then, should we understand his obvious interest in social democratic renewal?

One way to appreciate the perspective Esping-Anderson brings to his study is to recognize alternative conceptualizations that he does not use. An alternative that comes to mind immediately is the "party systems" framework. Had he employed this framework, Esping-Anderson would have paid much more attention to the bourgeois and radical parties of Denmark, Norway, and Sweden, perhaps enriching his theory of policy-generated support by applying it within, as well as between, nations. Among other benefits, this framework might have enabled him to resolve such puzzles as the continued reluctance of the Swedish bourgeois parties to cooperate, rather than simply dismissing the issue as a matter of social democratic good fortune. However, Esping-Anderson focusses almost entirely on the labor parties, referring to others only when necessary to support his analyses of labor party behavior.

The focus on "labor parties" rather than "party systems" or some other formulation stems from Esping-Anderson's decision to cast his study as a continuation of arguments about socialism begun years ago by Marx, Lenin, Kautsky, Bernstein, and a host of other socialist theorists. Most of these arguments became irrelevant in Scandinavia rather quickly, as social democrats chose first to participate in capitalist governments, then to compromise in order to form majority coalitions (usually with farmer-peasants), and finally to pursue reformist rather than revolutionary goals—largely because voters repeatedly rejected party appeals for socialist purity. Given this historical record, perhaps it is not surprising that Esping-Anderson's effort to contribute to these old debates produces rather meager results, consisting largely of his rejection of the distinction between "reformist" and "revolutionary" goals in favor of an amended definition of social democracy as "a movement that seeks to build class unity and mobilize power via national

legislation" (p. 10). The reference to national policy here seems quite sensible, but it is also apparent from his later chapters that use of this definition would exclude the Danish party for much of the past two decades, and partially exclude the other two parties as well. In short, Esping-Anderson's turgid struggle with the socialist cobweb seems neither productive nor clear, and quite unnecessary for the argument that follows in his text.

Nevertheless, it is a tribute to Esping-Anderson's integrity as a scholar that his analysis provides material to support theoretical interpretations quite different from his own. Voters who variously support social democrats depending on benefits offered and parties that seek majority coalitions by designing programs for different party constituencies both suggest a model of party pragmatism that is quite consistent with the data reported by Esping-Anderson. With voter and organizational self-interest at its core, in fact, such a model might help us to understand various bourgeois and splinter parties as well as the social democrats, and it could clarify prospects for party success in dealing with the new issues that Esping-Anderson believes will determine future voter alignments: job security and quality of working conditions. The pragmatic model is not developed in *Politics Against Markets*, obviously, but the data reported are rich enough to support this and perhaps other interpretations. Whether or not we accept Esping-Anderson's conclusions, therefore, all of us should be grateful for the care with which he develops his argument and the integrity with which he presents his data. *Politics Against Markets* is a powerful stimulus for productive thought about a central issue of our time.

THOMAS J. ANTON

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The Decline of Class Voting in Britain: Changes in the Basis of Electoral Choice, 1964-1983. By Mark N. Franklin. (Oxford: Clarendon Press, 1985. Pp. xii + 194. \$34.50, cloth; \$14.95, paper.)

Franklin's well-crafted and subtle book focuses on two questions: Has class voting in

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Britain declined since the 1960s? If so, why and with what consequences? He answers the first with an emphatic "yes" and the second with an unemphatic "difficult to tell."

The study is the theoretical and empirical child of Butler and Stokes' seminal *Political Change in Britain*. Its departure point is a "rehabilitation" of Butler and Stokes' socialization model of partisanship. Franklin turns their six main social structural variables into a causal model of the vote, and reanalyzes the British Election Studies from 1964 to 1979 to trace their changing impact. He shows that their capacity to shape the vote declined markedly (if unsteadily) over the period and was replaced by a growth of issue voting. In an elaborate analysis he demonstrates that only half the decline arose from the changing shape of the class structure—in particular the growing proportion of the electorate with mixed class characteristics—and that almost none could be attributed to the 1974 surge of minor party voting.

Franklin's main thesis rests on the particularly marked decline in the impact of the "central class" variables of occupation and parental class, as distinct from the merely "supplementary" class variables of housing, trade union membership, education, and parental partisanship. The latter, Franklin argues, can transmit and sustain support for *any* party; only the former *necessarily* socialize the electorate into Conservative and Labour support. The collapse of their influence means that since 1970, the structure of class voting has lost its backbone and cannot be relied upon to sustain the two-party hegemony. Only inertia and accident enable the Conservative and Labour parties to continue precariously as major parties. The future is one of volatility and unprecedented electoral uncertainty; not for 60 years has the party system been so "ripe for change."

In the course of this argument, Franklin challenges the numerous competing accounts of electoral change that have recently appeared. Those that side with him in discerning the demise of class voting—e.g., Himmelweit, Whiteley, and Rose—are gently but effectively admonished for technical weaknesses. (One of the book's side benefits is an exceptionally clear description of the complexities of different statistical techniques.) Those who try to resurrect the primacy of social structure are given much shorter shrift. In a devastating sec-

tion, Franklin demolishes consumption cleavage theory as conceptually vague, inadequately modeled, and lacking in empirical support. The ecological paradox of a strengthening of class influences at the constituency level is also dismissed—less plausibly—as an artifact of trends in the influence and areal distribution of housing.

In seeking explanations, Franklin singles out 1970 as the electoral turning point, but the early 1960s as the social starting point. The crucial change was not the "swinging sixties," pace Beer, or the "silent revolution," pace Inglehart, but the abolition of military conscription between 1958 and 1960. Innocence of the barrack-room class system combined with new-found affluence created a classless new generation within the working class, for whom politics was a matter of government performance, not class loyalty. In 1970 they swung massively against the conspicuously incompetent Labour government. Labour's electoral rot set in under Harold Wilson, not James Callaghan or Michael Foot. It is an original and intriguing argument, even if, as Franklin acknowledges, the evidence is incomplete.

Skeptics will quibble with Franklin's analysis here and there. The argument occasionally seems forced; resting on over-analyzed data or unduly speculative inferences. The dependence on respondents' recall of parental partisanship and class is troublesome, given the notorious unreliability of voters' recall of their own past partisanship. The argument that occupational socialization must prop up the Conservative and Labour parties, whereas housing socialization can sustain any party system, is not fully convincing. The absence of any reference to race as an element in the social structure and as an electoral issue (especially in 1970) is disappointing. Nonetheless, Franklin's contribution to the rapidly growing controversy on the role of class in British elections is the most sophisticated, painstaking, and, in my own view, plausible published so far. Anybody wishing to understand the complexities of the issues at stake will finish the book well rewarded.

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Administrative Politics in British Government.

By Andrew Gray and William I. Jenkins.
(Sussex: Wheatsheaf Books, and New York:
St. Martin's Press, 1985. Pp. x + 259.
\$26.00.)

Six years of the Thatcher Government, with its especially strong view of the proper relationship between elected and permanent officials, have given rise to an outpouring of interpretations of the state of British bureaucracy. Most of these works have focused on immediate questions and lack scholarly rigor, often centering on such hardy perennials of that system as government secrecy and the perverse uses of ministerial responsibility. Recently, the literature has gone in for mapping trends toward a more politicized civil service without giving that concept much specificity. One of the virtues of Gray and Jenkins' book is that they approach the alleged transformation of public administration in Britain from a fresher perspective than most other commentators. Their preoccupation is with organizational theory as a guide to what can be done to change the mainsprings of deeply entrenched attitudes and practices. Eschewing the more ponderous abstractions (and, happily, the terminology) of that genre, they employ a mixture of theoretical concepts, empirical references, and case studies to explore the influence of organization politics on the conduct of the central administration. Followers of Graham Allison will recognize familiar things here, although the authors insist that their version of bureaucratic politics takes in much more, notably, contextual elements that give rise to differing interests from department to department in "village Whitehall."

How well does the mixture work? (It is not really a "model," despite the authors' claims, but is not necessarily the worse for that.) When they can securely connect their well-honed concept of administrative politics with detailed government policy making, their analysis is quite persuasive. For example, they bring insight to the problem of engineering change in administrative outlooks by showing that those governments that take the trouble to understand the underlying organization, culture, and tasks of the diverse units making up the congeries called Whitehall stand a better chance of getting their way than those that opt for an undifferentiated blueprint for change. In

the search for ways to import managerial efficiency into the civil service, reforms of the former sort—characterized by the Rayner scrutinies and Heseltine's Management Information System for Ministers (MINIS)—have had impact and shown staying power, while more sweeping programs—such as the Fulton Committee's ideas and Programme Analysis and Review (PAR)—have been sidetracked rather easily by experienced players of administrative politics. Gray and Jenkins' discussion of such matters puts into perspective Thatcher's obsession with enhancing management at the expense of policy. On the other hand, in those cases where the behavior of politicians and bureaucrats can be fitted into the authors' theoretical framework only tangentially, as in their case study of the Falklands dispute, the result is more commonplace and provides little support for their overall approach. The book, then, like the structure of British administration, is quite variable, with strong chapters juxtaposed with pedestrian ones. Mercifully, when chapters are good they are very, very good, as is true of those on "The Politics of Efficiency" and "The Politics of Administrative Reform."

In part because the authors have theoretical aims and are concerned not to overuse case studies, their work has a "from the outside" flavor. Such an orientation has costs for those who stake much of their argument on the twin concepts of administrative culture and bureaucratic politics, both of which (as Heclo and Wildavsky have shown) should take us directly into the corridors of power. Gray and Jenkins prefer to rely for much of their case material on official reports, as when, in a controlled way, the government allowed the Franks Committee to lift a few veils from the operation of the Falklands war. The alternative use of sources such as press leaks, ministerial memoirs, and interviews with principals, while they carry undoubted methodological costs, would have brought the authors closer to the administrative decisions that they attempt to put into new perspective.

Taken as a whole, the world that Gray and Jenkins depict is a highly political one. It is not that of the clever cynicism of the *Yes, Minister* television series, or of those who see these relationships primarily in clientelist or corporatist terms. Rather, it is a world of shifting power and dependency, in which the central, stable

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fact is that of organizations acting in reference to other organizations. A paraphrase of Michels might do to sum up their position: Who says organization says bureaucratic politics.

JAMES B. CHRISTOPH

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The Politics of Local Socialism. By John Gyford. (London: George Allen & Unwin, 1985. Pp. x + 129. \$22.50, cloth; \$8.50, paper.)

Traditionally, Britain's Labour party has understood local government as a means to implement or obstruct a parliamentary and centrally planned socialism and to provide employment programs and social welfare services. However, town center redevelopment and housing policies and garden city development revealed a party whose administrative and paternalistic politics were remote from constituents and occasionally corrupt. These programs often destroyed pieces of the urban fabric, disadvantaged the less affluent, and generated community opposition. As a result of electoral defeats, changing social and ideological composition, new leadership styles, and reorganized and increasingly problematic center-local relations, Labour has pursued a more decentralist form of socialist politics. This volume examines the democratic socialist potential of the political ideas and practices of newly emerging radical Labour councils in Britain's cities.

Gyford suggests multiple and interrelated sources of leftist Labour strength in urban government and electoral politics: Community Development projects; struggles and movements that develop around community action, women's and environmental issues, and economic and anti-austerity organizations; and a growing number of leftist party activists. Local socialism also has ideological roots: a refusal to accept a socialism that is bureaucratic and centralized, and a conviction that the local state is sufficiently autonomous and accessible to popular pressure.

He contends that the new urban Left's program is both defensive and prefiguratively socialist in nature. On one hand, it defends local autonomy against the Thatcher govern-

ment and opposes its rate-capping legislation and attacks on welfare-state benefits and living standards. On the other hand, it pursues grass-roots industrial policies planning to promote more unionized jobs and new, democratic forms of enterprise, employment, technology, and industrial control. The councils formally monitor police and seek to eliminate police racism and sexism. Also, women's social policies and services are high priorities. As resources for political education and community action, the councils' grant programs support local organizations in the arts, recreation, employment and training, women's services, and community development.

Proponents believe that the local road to socialism is distinguished from other current approaches to decentralization in at least two ways. First, local socialism links the people to the Labour party, opposes the policies of the Thatcher government, and defends working-class communities. Second, it encourages mass participation in the decentralized management of services and popular planning to improve their quality and match social needs with resources. Thus, the local state enables forms of urban political education, and the local state's power of example and of incubating innovative progressive programs educates the nation.

The councils face important and not unrelated political problems. They seek a new policy role for their members and more informal, less staff-dominated and professionalized forms of urban governance. They confront the strategic question of uniting old working-class constituencies with new and diverse cultural and ethnic group supporters. Although they combine an orientation toward constituency benefits with an ideological commitment to radical social change, unions and the Left frequently pressure them. In short, local socialism generates political conflicts over the functional decentralization, organization, and implementation of public services and programs, as well as over its forms of participation. It functions within significant, and in this work probably underemphasized, limits on local power and resources. If it escapes the dangers of party bureaucratization, it may still have slight impact on broader social and power relations. Finally, the strategic dilemma of local socialism is how to balance the desire for decentralization and self-determination and the need for

political mobilization to establish new collective norms and transform the society.

Although some hold that local socialism rests on a consumption-based and irretrievably pluralist and localist politics, incapable of constituting itself as a national force, Gyford concludes that it has already influenced local government and the agenda for socialism. It has demonstrated that cities—governed with much less reliance on bureaucratic and professionalized norms and more responsive to diverse urban interests—can pursue and implement a more progressive range of politics and programs than many believe. Local socialism reintroduces the idea of a self-governing socialism based on the democratic practices, skills, and self-confidence of a participatory citizenry. Democratic socialist practice would encourage a healthy tension between parties and the people, and a variegated and decentralized economy. It would be willing to accept nonsocialist policies as the price of an unswerving commitment to a socialism that rests as much on political empowerment from below as it does on redistribution from above.

The book is intended as a mere introduction to local socialism in Britain. It succeeds only partially in that regard, for it does not clearly define and measure the likely or real, symbolic or tangible, policy accomplishments of local socialism. The broader comparative issues and profound theoretical questions are beyond its scope.

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The Power of the Powerless: Citizens Against the State in Central-Eastern Europe. By Vaclav Havel, et al. Edited by John Keane. Introduction by Steven Lukes. (Armonk, NY: M.E. Sharp, Inc., 1985. Pp. 228. \$35.00. Published simultaneously as vol. 15, no. 3-4 of *International Journal of Politics*.)

The book is the outcome of a joint Czechoslovak-Polish seminar on the nature of the Communist systems of East-Central Europe, and on strategies to promote civil and human rights in both countries. The leading essay was written by Vaclav Havel, a well-known Czech playwright, author, and member

of the opposition. The volume consists of this lengthy piece and what may be considered short responses and elaborations by his Czechoslovak colleagues. The writers range ideologically from Petr Uhl, the founder of the Czech Revolutionary Socialist Party and the only one among the contributors in favor of violent socialist revolution in present day Czechoslovakia, to Joseph Zverina, a Catholic priest and theologian who advocates change through Christian love. All of the contributors are signatories of Charter 77, a document published that year in Czechoslovakia demanding the government respect human rights as guaranteed by its own constitution and by the international treaties to which Czechoslovakia is a signatory.

The essays provide an original and penetrating analysis of totalitarianism, or what Havel calls *post-totalitarianism*, which is bound to revitalize the ongoing debate among Western political scientists as to the usefulness of this term and the importance of ideology in these political systems. This view of totalitarianism is combined with a fascinating theory of how it could be effectively opposed. The originality of the discussion stems from the fact that it is undertaken, not by Western "specialists," who, in most cases, have a limited practical knowledge of these regimes, but by "practitioners"—either those who, like Hiri Hajek, were for many years members of the Communist ruling elite before they joined the opposition, or those like Reverend Zverina, who spent nearly one-third of his life in Nazi or Communist prisons. Both groups know the system from the inside, but write with the considerable detachment so necessary for objective analysis.

The essays view the present-day totalitarianism in Czechoslovakia—and by extrapolation, in other countries of the Soviet bloc—as still ideologically based. This ideology however, instead of preaching the original message of total transformation, now serves as the gospel for the ritual fundamental to the maintenance of the system. The ideology, by its promise of the just society of equals, legitimizes the frozen system of inertia. The rulers and the ruled act as if the society were still faithful to its Marxist commitment to humanism and to the "withering away of the state." Officially only the myth is real, and one lives in a schizophrenic world where the real "real" is unreal. The total

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mobilization of society in early totalitarianism changed to a demand for total passivity and silence. In this system of "ifs" the Soviet Union cannot reform itself without destroying the prop on which the East-Central European "ifs" are built. One is reminded of Christianity, in which the original teachings of Jesus were eventually ritualized into power and corruption in the medieval church. Like the medieval church, Communist totalitarianism can only be destroyed or reformed by the truth about its lie. It has to be overthrown from within, in the tradition of Jan Hus, a Czech religious reformer who, one hundred years before Luther, preached the truth about the medieval church (and was burned at the stake). Today's totalitarianism can be defeated only if people live the truth in a parallel society, independent of government control—a society consisting of culture, media of communication, trade unions, foreign contacts, and even the economy—"a parallel polis." Living in the truth returns the focus from the state to the individual. The few who take the first steps and dare to live the truth may, like the early Christians, be thrown to the lions, but their example will create a multitude that eventually will conquer the empire.

Although the writers challenge the "real socialism" of present-day Czechoslovakia, they remain true to socialist values and, in most cases, to Marx and even to Lenin, because for them, socialism is the only system capable of extending democracy to economics. In their arguments there is a strong sense of the anti-statism of Marx and of Ghandi, in which the state is always viewed as a negative force counterposed against a "positive" society. Thus, the present socialist (Communist) state must be replaced by a loose association of self-governing groups with a minimum bureaucracy. The ideal model might be the Paris Commune of 1871, admired so much by Marx.

For an edited work the book is surprisingly consistent. It is definitely recommended reading for those with an interest in comparative politics, totalitarianism, political philosophy, and Marxism. These words were written by brave, intelligent, and dedicated men. It would be a pity if they were not heard in the West.

JAROSLAW PIEKALKIEWICZ

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Japan's Financial Markets: Conflict and Consensus in Policymaking. By James Horne. (Sydney, Australia: George Allen & Unwin, 1985. Pp. 271. \$33.95, cloth; \$13.95, paper.)

The recent gradual liberalization of Japan's financial markets has provided James Horne, an Australian government official, with material for a series of informative case studies about change processes in an important sector of regulatory policy. Following a brief but interesting overview of the government's relationship to financial institutions, he describes how a market for government bonds developed; the issue of whether banks or securities houses should control it; why certificates of deposit were finally authorized; conflict over interest rates and tax reporting in the postal savings system; the partial loosening of foreign exchange controls; and the growth of the market for foreign yen-bonds. The period covered is the 1970s and the early 1980s, with the most detailed information pertaining to 1980-81, when the author carried out over 100 interviews. There is also a good chapter on how policy might be affected by the Ministry of Finance's personnel system.

The key to Japanese financial policy is the maintenance of a highly regulated and hierarchical structure of low interest rates. Most of these stories are about how various actors try to preserve their own slice of the status quo as this structure is progressively threatened by the growth of government debt and the continuing process of internationalization. Regulated interest rates have some economic justifications, but the author's aim is to isolate political factors. In particular, the inertia he finds in this policy area is ascribed not to a Japanese love of harmony, but to intense conflicts of interest among multiple actors with effective veto power. This explanation leads to something of a contradiction, since both conflict and policy change are seen as increasing over time, even though conflict is supposed to inhibit change.

In general, theorizing is not a strong point in this book. The typology of decision making, crossing "level of agreement" and "level of concentration of control" to produce unity, consensus, dominance, and conflict modes, is largely circular and not very helpful. On the other hand, Horne is a clear-sighted observer

of processes that have confused many political scientists. On the ever-popular question of whether the Liberal Democratic Party (LDP) or the bureaucracy really runs Japan, he concludes that even the Finance Ministry is "relatively hopeless in the face of effective political alliances built within the LDP"; yet in most of the cases described—some quite important—"the LDP had little or no direct involvement" (pp. 112-13). In fact, the majority party normally does not intervene, except when its grassroots political interests are at stake.

For the majority of policy issues, outcomes depend on the delicate relationships among bureaucratic agencies and between each agency and its clientele, and on the rules of the game (jurisdictions, routines) that pertain to particular decisions. Even within a circumscribed policy area like financial markets, decision-making processes get very complicated, and the author explains them well—he is especially good on the divergent interests of the four Finance Ministry bureaus active in this area. These are not always as high-minded as advertised: such motives as avoiding complications in the marketing of government bonds, protecting the profitability of banks, and even maintaining a pool of post-retirement jobs often outweigh economic policy considerations or pressures for change from foreign banks and governments. Horne thus takes a prominent place among the school of Tokyo watchers that tends to cast doubt on the Japanese government's capacity to make rational and coherent policy. Those who take the opposite view should be encouraged to consider his carefully assembled evidence from a crucial area of Japanese public policy.

JOHN CREIGHTON CAMPBELL

University of Michigan

The Geography of English Politics: The 1983

General Election. By R. J. Johnston. (Dover, NH: Croom Helm, 1985. Pp. xvii + 358. \$43.00.)

With the zeal of a missionary among particularly recalcitrant natives, R. J. Johnston wants to alert political scientists to the importance of a "geographical dimension" in British political behavior. More particularly, he wants to banish forever the widely-held image of

England as "a homogeneous place, with no spatial variations in voting behaviour additional to those that can be accounted for . . . by the basic electoral cleavages" (p. 278). The principal cleavage to which he refers is, of course, the traditional socioeconomic division of the vote with strong Labour support among working-class electors and equivalent or higher Conservative sympathy in the middle class. In making the case for localistic behavior during the 1983 general election, the author attempts to demonstrate wide variability in patterns of class-based voting among parliamentary constituencies, and to provide and verify sound theoretical explanations for local deviations from the nation-wide norm.

The homogeneity thesis is disproven quickly by showing how badly "national" patterns of electoral cleavage were reproduced across England's 523 parliamentary constituencies. When survey-based estimates of class voting patterns are applied to data on the social composition of the constituency, the simulated results diverge substantially from the actual election results in 1983. The author reports similar disparities when the individual-level voter transition matrix of 1979-1983 is used to forecast the recorded constituency-level flow of the vote. In a short but important chapter focusing upon the political consequences of voter behavior, Johnston demonstrates that constituency variations from the national pattern have important effects upon the distribution of seats in parliament.

For Johnston, the gap between forecast and outcome means that voters are influenced by constituency-level forces, which he apportions to *sectional*, *environmental*, and *campaign* effects. The first category, sectional effects, refers to local, regional, and other areal differences in partisan socialization and tradition. Environmental influences include the salience of particular issues and the personal qualities of candidates. The third type of local influence, campaign effects, refers to the closeness of previous campaigns and the scale of organized efforts to sway electors. Johnston argues that these forces are better explanations for spatial variations in class/party support patterns than the social interaction and "contagion" processes usually cited in studies of "contextual," "structural," "compositional," or "neighborhood" effects on voting.

The remainder of the book uses multiple

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regression equations to account for spatial variation in several aspects of voting behavior: class-based voting, voter shifts in party preference since 1979, and class-based patterns of voter transitions. Lacking any direct information about such individual behavior within constituencies, and rejecting traditional methods for deriving cell entries from known marginals, Johnston generates the behavioral estimates for each constituency by maximum likelihood methods. Through this innovative technique, he discovers that the two major parties did better in winning and holding onto support from their "core" social class as that class increased its share of the constituency electorate. Moreover, Labour and Conservative candidates did better and held past supporters more efficiently among all classes of electors in areas where they ran incumbents for reelection or where regional traditions historically favored their party; in districts with clusters of certain occupations or characteristic population densities; and in marginal constituencies and constituencies where they spent more money. In addition, Labour gained support among all classes from high unemployment rates. Depending upon the particular dependent variable, support for the Alliance—the coalition between Liberals and Social Democrats—was less clearly predicted by any constituency characteristic.

The validity of the specific conclusions and of the author's brief for geographical influences hinges on the adequacy of the empirical analysis. However sympathetic the reader toward ecological analysis and contextual models, there are some troubling methodological questions left unanswered. The spatial variations Johnston attributes to various contextual forces might reflect measurement error or the differential distribution of certain unmeasured constituency traits. Because of sampling error, the national survey estimates of party support by social groups are approximations. The size of the social groups in each district's electorate is based on somewhat dated census information about the unit's resident population. If multiplying the two percentages produces a poor fit to recorded results, that might be due to the roughness of the estimates. Moreover, if individual voting is significantly affected by factors other than social group membership—as an increasing number of scholars now argue—then spatial variations in class/party relationships may be responses to

variables not included in the model. The possibilities of measurement error and model misspecification should have been addressed. Given the book's five lengthy methodological appendices, I was surprised that the author did not raise these issues.

The pervasive indictment of previous voting research also seems overstated. Social scientists generally have not been as indifferent or naive about contextual influences as Johnston imagines. Recent political science literature provides a variety of contextual models that rely on behavioral mechanisms other than social contagion. Research on British electoral politics has suggested two major factors that might produce significant spatial variations beyond class—nonwhite immigration and "peripheral" nationalism. Racial effects are excluded from the analysis for no convincing reason (see p. 80), while the potential influence of peripheral nationalism is arbitrarily minimized by omission of non-English constituencies. Furthermore, Johnston's major empirical finding, that a party's support in its core class increases with the relative size of that class, is perfectly compatible with the contagion process that the author so readily decries. Until he can find better indicators for his alternative contextual influences and show their statistical impact, the social interaction model remains a plausible explanation for varying class/party relationships across election districts.

Given its theoretical and methodological complexity, the book is unavoidably heavy going. Despite dense prose and occasional howlers like the use of "preciser" (p. 107) and reference to data that was "grossed up" to another level (p. 110), the argument is comprehensible. More important, for all the weaknesses already enumerated, the argument is stimulating. Like other contextual research, the book raises doubts about the adequacy of the individualist model of voter behavior that still pervades much research in Britain and elsewhere. Thus, the book can be read with profit by both students of British politics and experts in the field of voting studies. Even if the magnitude and source of geographical influence on voting remains a matter of debate, its existence warrants the type of sophisticated analysis exemplified in this book.

KENNETH D. WALD

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State-Building Failure in British Ireland and French Algeria. By Ian Lustick. (Berkeley: Institute of International Studies, University of California-Berkeley, 1985. Pp. x + 109. \$8.95, paper.)

Lustick argues that the presence of British and French settlers was "a sufficient, albeit not necessary" condition for the unsuccessful incorporation of the "subject peripheries" of British Ireland and French Algeria into the British and French states (p. 5). The bulk of his evidence for this claim comes from secondary source material on the consequences of "settler political activity for cooptive and participatory native policies pursued by metropolitan cores" (p. 9) for Britain (pp. 17-38) and France (pp. 47-76). Lustick also uses material about the successful cooptation of local elites and hence the successful incorporation of subject peripheries by Britain and France in their earlier history (pp. 9-16). Lustick's explanation of state-building failures in Britain and France is an important and clever addition to the literature on colonialism. He argues that the failure results from the relationship between settlers, natives, and the metropole.

To expand the central state, the metropolises had to overcome the demographic preponderance of the natives. Their solution was to settle large numbers of nonmilitary citizens in outlying territories inhabited by "culturally distinct and antagonistic natives" (p. viii). The British thus planted colonies in Ireland in the 1580s (Elizabeth), from 1607 to 1640 (the Stuarts), and in 1652 (Cromwell); France planted colonies in Algeria from 1830 to the early 1900s. However, in planting these colonies, the metropolises pursued contradictory goals. During tough times, the colonies were viewed as an asset to be exploited—needing the natives in order to consolidate their rule, the metropole accommodated native demands by attempting reforms. During good times, the colonies were viewed as a burden to be ignored—not needing the natives, the metropole accommodated its settler's demands and ignored reforms.

Settlers also pursued contradictory goals. On the one hand, they were numerically weak compared with the natives, and hence wanted protection against potential native mobilization. The settlers thus sought close ties and assimilation to the metropole, so that they

could use its resources to maintain local hegemony. On the other hand, the settlers wanted to dominate the native majority and protect their local privileges. They thus sought autonomy from the metropole, so that they could escape the logic that full integration into the metropole meant equal treatment of all citizens, settlers and natives alike.

Through various mechanisms (pp. 81-82) the settlers were able to undermine the metropole's cooperative policies towards the natives, and hence block reforms. The consequences were that indigenous local elites were not coopted into the decision-making process, and native masses did not enjoy full citizenship rights. These factors led to native unrest, a struggle for political rights, separatist demands, and the prevention of the transfer of loyalty and legitimacy from the core to the periphery.

Lustick's argument is intriguing, but based on two crucial assumptions. He assumes, first of all, that in the absence of pressures from settlers, Algerian and Irish ethnic groups would be reformist, and that native elites would welcome metropole reform policies (pp. 37, 70, 72). In other words, Lustick assumes that settlers disrupt "processes of elite cooptation and expansion of political participation rights to natives" (p. 8) that are sufficient for successful integration. However, would the native elites ever have been reformist and coopted? The argument tends to slight other factors (e.g., geography, culture; see p. 83) that influence separatist and radical ideologies and behaviors among ethnic groups.

Second, the argument assumes that metropole policies would be reformist in the absence of settler pressures, but how generous are metropole elites? Are they ever willingly reformist and accommodative of native demands? The argument tends to ignore other factors (e.g., interelite squabbles) that influence metropole policies. Lustick assumes that accommodative metropole policies are sufficient for successful integration.

To test Lustick's argument, one thus needs a crucial case study: if settler undermining of metropole policies is decisive in preventing successful incorporation of peripheries, then a situation where cooperative natives rebelled against reformist metropolises stymied by hegemonic settlers should be analyzed. Lustick did not establish that, *ab initio*, the natives

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were cooperative and the metropolises reformist. This volume thus establishes only the plausibility of Lustick's argument. It is an interesting and important attempt to "describe, compare and explain the differential success of various state-building efforts" in Britain and France (p. 2), but a convincing test of Lustick's argument has yet to be made.

MARK LICHBACH

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Capitalism and Apartheid: South Africa, 1910-1984. By Merle Lipton. (Totowa, NJ: Rowman and Allanheld Publishers, 1985. Pp. xii + 449. \$19.95.)

The historical and contemporary relationship between capitalism and apartheid is an issue of significant debate among scholars of South African political economy. To cast it starkly, some maintain that apartheid structures, which place severe limitations on the vertical and horizontal mobility of African labor, were the creation of irrational and backward forces of Afrikaner nationalism that obscured capitalist development. Capitalists, therefore, have never really wanted apartheid, and can be expected to press for its removal. Others argue that capitalist forces helped to construct apartheid and benefited enormously from the cheap labor it guaranteed. Thus, the racist structures were not an irrational impediment to capitalist growth and profit, nor can capitalists be expected to be a progressive force to help eliminate apartheid. Lipton engages this debate in a complex and dynamic study that demonstrates the crudity and static nature of some previous formulations.

The heart of Lipton's analysis is contained in the book's middle section, which systematically examines the changing interests of the "white oligarchy." Included in the oligarchy are agricultural, mining, and manufacturing capitals, and white labor. The author asserts that the exclusion of Africans from political and economic power was rationally calculated and vital to white (predominantly Afrikaner) farmers' interests because

it underwrote their possession of the land, shielded them from black competition and pro-

vided them with plenty of cheap docile labour, while not imposing on them the costs in the skilled [white] labour market incurred by other employers. (p. 108)

White labor was the other major economic sector that demanded apartheid in order to protect itself from the higher quantity and lower costs of African labor.

Mining capital, dominated by the English, had a more complex calculation of the costs and benefits of apartheid. In the beginning, white conquest was essential to the operation of the mines, and later mineowners supported the restrictions on Africans' physical mobility. However, these capitalists did not want or like the job bar that inflated the price of white labor by assigning all whites to skilled jobs and all Africans to unskilled jobs. Forced to accept this piece of apartheid, mineowners responded by instituting highly repressive measures against Africans, such as closed housing compounds and very low wages. In addition, the increasingly interventionist role of the state was resented by owners of mining companies, whose surpluses were taxed to pay for the protection of agriculture and manufacturing. Manufacturers generally opposed apartheid because it was incompatible with their need for skilled black labor and a larger domestic market. This sector was compensated for such costs through protectionist policies that, together with repressive measures by the state, helped assure its acquiescence.

In the 1970s, Lipton asserts, the benefits of apartheid to certain sectors of the white oligarchy that had previously supported it began to change. This change was brought about by increasing capital intensity in all sectors of the economy that required even more stable, skilled labor—the kind that apartheid was designed to discourage among Africans. Support for apartheid among capitalists of all kinds has dramatically declined in the last 15 years, Lipton argues, although it has remained high among white labor and the state bureaucracy. The resulting conflict within the oligarchy has produced "a contradictory and confusing mixture of reform (particularly in the socio-economic sphere) and maintenance of apartheid (especially politically)" (p. 49). Capitalists have not forced more changes because of divisions among them, their inability to forge coalitions with the black opposition, their fear that instability and

insecurity would accompany rapid change, and state repression.

In addition to providing an engaging, readable, and lucid analysis of the differing economic and political interests of the ruling elite, Lipton provides an overall history of twentieth-century South African political economy, an analysis of the impact of apartheid on blacks, an examination of the interaction of race and class as variables affecting policy and social change, and an investigation of historical and recent changes in apartheid.

An issue taking on increasing significance, but which is inadequately addressed, is the potential for effective black resistance. Lipton discusses this, but does not devote to it the careful analysis she brings to the examination of South Africa's rulers. Perhaps further elaboration on this issue is too much to ask of one book that is already broad in scope and length. The occasional use of survey data collected by others in a virtual police state, especially to analyze black opinion, is a questionable practice. Another disappointment is the lack of a comprehensive bibliography, although there are extensive footnotes. These quibbles aside, the book is excellent scholarship and a refreshingly sophisticated contribution to the study of South African political economy.

JANICE LOVE

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Economic Policy and Policy-Making under the Mitterrand Presidency, 1981-1984. Edited by Howard Machin and Vincent Wright. (New York: St. Martin's Press, 1985. Pp. x + 293. \$27.50.)

The surprise of the Socialist electoral victories of 1981 inspired much prudence among students of French politics. Only recently has confidence returned to the field in a quantity sufficient to allow observers to commit their appreciations of French politics in the eighties to print. Hence, one welcomes this edited volume on Socialist economic policy making. The welcome is all the warmer because of the book's focus. Economic policy was the one facet of the Socialist victory that aroused the greatest hopes and the greatest curiosity, and it is Socialist economic policy that has provoked the greatest disappointment.

The editors bring to this enterprise a collection of eminent scholars. Primarily an Anglo-French endeavor, its authors include such well-known and well-placed French observers as Paul Fabra of *Le Monde*, Dominique Strauss-Kahn of the *Commissariat au Plan*, Pierre-Alan Muet of the *Observatoire Français des Conjonctures Economiques*, Christian Stoffaes of the Ministry of Industry and Research, as well as academics such as Yves Mény and Yves Morvan. Three articles were authored by respected American observers of French politics: Janice McCormick, Suzanne Berger, and Frank L. Wilson. The British contribution, with the exception of the introductory article by Machin and Wright and a provocative article on the *lois Auroux* by Duncan Gallie, is confined to commentary on the articles. The commentary is both informed and perceptive, however, and provides much of the appeal of the book.

One finds in this volume a diversity of appreciations concerning Socialist economic policy. For some, Socialism represents little more than an abortive experiment in idealism. At best, the Socialist reign has "performed an educational function" by allowing the left, excluded from power for nearly a generation, to learn "how to manage France in an interdependent world economy" (McCormick, pp. 60-61). One encounters ironic continuities between the policies of the current government and those of its predecessor. Jack Hayward points out that "despite the scorn poured on the still-born Eighth Plan, its Socialist successor was often forced to follow it closely" (p. 114). One encounters similar continuities in financial policy, despite the Left's criticisms of the financial strategy pursued by the Giscard-Barre government.

Instances abound in which the best-intentioned policies yielded unanticipated outcomes. For instance, the government sought to restore the Plan to a position of preeminence. However, it actually "accelerated the decline of planning" (Ozenda and Strauss-Kahn, p. 112) by promoting greater *concertation* (cooperation) between the state and interested social groups, thus depriving the planning commission of autonomy. In the same vein, the government's privileged relations with the leftist unions resulted in a loss of influence for the latter, leaving the government with ineffective union support. Similar disappointments have

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sullied the Socialist government's attempts to diminish the influence of the *patronat* (employers), as well as its attempts to reinforce worker support through reforms in industrial relations.

On the other hand, some contributors take care to identify the benefits—actual or potential—that have accrued as a result of France's experiment with "socialism." Christian Stoffaes, for example, points to the reforms that have taken place in the management structures of nationalized firms, and predicts that if such reforms succeed and spread to the private sector, then "the coming to power of the left will have created a new situation in industrial relations in general" (p. 158). Nevertheless, one closes the book pondering the words of David Goldley, for whom "it would not be the least paradox of the Socialist experience in France, if the Government were rejected for its excessive and broken promises of 1981, but in fact left to its conservative successors a much better situation than it itself inherited" (p. 254).

This volume is not characterized by an ambition for anything like theoretical sophistication. It is a book of political analysis, and the level that it achieves within its chosen genre is very high indeed. Nevertheless, its pages contain a number of intriguing insights of theoretical relevance. For example, both Machin and Wright's and Janice McCormick's articles refer to the limits of the state's power to formulate and implement economic policy. Jean-Paul Fitoussi writes in his comment that "today, State power is relatively much weaker for, domestically, it is trapped in corporatist arrangements and externally, it is even more constrained by the rules of international competition" (p. 166). The state theorist will also find provocative the claim made by David Soskice that "the case for state intervention in French industry rests partially on the strength of the State, but also on the inability of French industry to restructure itself, through lack of coordination" (p. 171).

In sum, this is a book replete with both solid analysis and provocative insight. It is certain to reward the efforts not only of the student of French politics or of economic policy, but also of the student of state theory and international political economy.

MICHAEL LORIAUX

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Marxism in Asia. Edited by Colin MacKerras and Nick Knight. (New York: St. Martin's Press, 1985. Pp. 297. \$32.50.)

The stereotype of monolithic Marxism so tenaciously popular in the United States could not survive a reading of this book. Not that all the Marxisms described are attractive or even harmless: it is numbing to read even a short description of North Korea's *Juche* ideology, and the Khmer Rouge slogan of "Spare them, no profit; remove them, no loss" is as brutally simple as their killing fields. However, the ideologies have so little in common with each other and are so tightly interwoven with the political and cultural context of each country that their common allegiance to "Marxism" is more historical than theoretical.

Marxism in Asia is intended to be an advanced undergraduate textbook. Except for its price it fulfills that purpose well, although "Asia" should be understood as East and Southeast Asia. Within that restriction the important Marxisms of the Philippines, Malaysia, Burma, Thailand, and Laos are left out. The book concentrates on the ideology of the respective communist parties. It is not concerned with salon Marxism, and it presents the political history of the parties only insofar as is necessary to understand ideological developments. Generally speaking, the introductory material on Marxism, Leninism, and Stalinism, the extensive treatment of China, and the individual chapters on North Korea, Japan, Vietnam, Kampuchea, and Indonesia provide a solid introduction to the respective ideologies that is as clear as the subjects permit. Some parts of the book are especially good—I would single out the introduction to Marxism-Leninism, the discussion of Mao Zedong, and the chapters on Japan and Indonesia. The weakest chapters are those dealing with post-Mao China and North Korea, but even these are worth reading. Anyone desiring a beginning orientation to Asian Marxisms would find the book useful.

The introductory material on Marx, Lenin, Stalin, and the Comintern provides a clear introduction to the common heritage of Asian Marxisms. The discussion of Chinese Marxism develops into an analysis of Mao Zedong that makes understandable both the connection of Mao's politics with his more abstruse theorizing and also the changes between his Cultural

Revolution leftism and his earlier thinking. The succeeding chapter tends to treat post-Mao Marxism as the denouement of Maoism, which is unfortunate because there have been many interesting and challenging ideological developments. It is unfair, for instance, to say that the Chinese have given up "systematically seeking social equality" and that "they have devised a theory based on Marx to justify present inequality" (p. 136). Equality has not simply disappeared as a political value; it has become a problematic and sensitive one. Rawlsian justifications of dynamic inequality are heard countering murmurings about polarization, and Gini indexes, caloric intake, and other welfare indicators have replaced class struggle as the ammunition of argument. It is a hard task to summarize and simplify post-Mao developments, but there are much better attempts by Schram and Tsou.

The second half of the book is comprised of chapters of from 20 to 30 pages dealing with each of the remaining countries. The problem with the Korean chapter is that Kim Il-sungism is taken too much at face value, and there is not enough analysis of its pathology. It is a wonder to read that "*juche* and Kim Il Sung's thought are indeed a creative development of Marxism-Leninism" (p. 171). Dynastic Stalinism is different, but I am not sure that it is creatively different. The chapters on Japan and Indonesia not only provide a summary of ideological developments, but succeed in conveying the unique spirit of Marxism in these countries. Even though these are the two "parliamentary Marxisms" of the collection, the highly factional and intellectual soul-searching of the Japanese contrasts dramatically with the broad themes of national unity in Indonesia. A very different and chillingly unique spirit of the Khmer Rouge is presented in a chapter entitled "Kampuchea and Stalinism." The portrayal of the Khmer Rouge is useful and convincing, but the comparisons with Stalin and Mao are haphazard. It would have been useful to extend the analysis to the current government of Kampuchea, because it presents a peculiar case of one Marxist government overthrowing another. The treatment of Vietnam is adequate, but the very important reforms since 1980 are ignored. The overall development of politics in Vietnam is more complex and interesting than the chapter suggests, but part of the reason is that the Vietnamese are cautious in

theorizing. Perhaps for good reason. The leading theorist, Truong Chinh, holds some personal responsibility for two major policy disasters.

So what is Marxism in Asia? One could easily collect a stew of contradictory adjectives from this book, and if more countries were studied the pot would more easily boil over. The greatest common denominator would be pitifully small, and moreover, would fit a number of non-Marxist political movements. The ideological connection has become more genetic than generic. *Marxism in Asia* succeeds with its subject only because it starts from the individual context of each country, rather than from a Procrustean bed of stereotypes or formulas.

BRANTLY WOMACK

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State and Opposition in Military Brazil. By Maria Helena Moreira Alves. (Austin: University of Texas Press, 1985. Pp. xiv + 352. \$22.95.)

Brazil has long been considered a paradigmatic case in analyses of bureaucratic authoritarianism in Latin America. In 1985 Brazil witnessed the installation of a civilian in the Planalto presidential palace after a long period of military rule initiated by a coup in 1964. Consequently, Brazil, the eighth most powerful economy in the capitalist world and a leader among the "advanced developing countries," currently figures prominently in the study of democratization in the region. The publication of *State and Opposition in Military Brazil*, therefore, should be welcomed by "Brazilianists" as well as by those interested in the comparative analysis of authoritarian rule and regime transitions.

Moreira Alves's ambitious objective is to examine the "dialectical relationship" between the state and the opposition, in order to understand the forces that have "shaped the complex political and social mechanisms of domination in that country since 1964" (p. 3). This research is informed by skillful use of documentation drawn from governmental sources and the major opposition actors in civil society. Judicious use is also made of personal memoirs and the author's more than 100 inter-

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views with opposition figures. Useful statistical data is presented in a 40-page appendix. Very little has escaped Moreira Alves's encyclopedic coverage, although some topics, such as the "culture of fear" and the key role played by entrepreneurial elites, could have been accorded more extensive treatment.

The central focus is on the "Doctrine of National Security" and its offspring, the "national security state." Four contradictions in the Brazilian variant of the national security state are identified: (1) a tendency toward unchecked expansion of the state's repressive apparatus; (2) the state's inability to eradicate opposition completely; (3) state repression that tends, in the context of mounting social and economic grievances, to propagate opposition from one sector of civil society to another; and (4) the abyss between the language of legitimization premised on the return to democracy and the reality of state repression, which tended to generate recurrent politico-institutional crises.

Following this initial discussion, Moreira Alves turns to a careful narrative reconstruction of three "stages of institutionalization" of the post-1964 military regime. Her goal is to demonstrate that the process of "decompression" and *abertura* (political opening) under military rule stemmed from constant transformations in the state's relationship with a multifaceted opposition movement rooted in civil society. Herein lies the book's most significant departure from the English-language literature on contemporary Brazil. Moreira Alves parts company with the view that liberalization was induced by the post-1974 collapse of Brazil's "Economic Miracle," as well as with those who argue that liberalization was the consequence of the ascendancy of more liberal generals with a moral commitment to return the country to democracy and the military to its barracks.

Eschewing both economic and psychological reductionism, Moreira Alves demonstrates that

after each period of repression (which reflected the view of the "internal security" sector within the military) the state would implement a policy of gradual liberalization in order to lower the level of tension created by the violence of the repressive apparatus. (p. 255)

Analysis of the last two military governments headed by Generals Geisel (1974-1979) and

Figueiredo (1979-1985) indicates that the dynamic of change cannot be accurately described as a linear progression from liberalization toward democracy. Each successive cycle of liberalization was a response to the growing capacity of opposition sectors to resist a logic of state power crystallized in the national security doctrine. Decompression was a deliberate strategy articulated by General Golbery do Couto e Silva, the *éminence grise* of the post-1964 period. First, the regime deployed a policy of carefully targeted selective repression, and second, it simultaneously pursued controlled political liberalization, circumscribed by greater respect for individual rights and limited electoral competition. This two-pronged strategy did not envisage a transfer of executive power to the opposition; rather it was intended to curtail what General Geisel once referred to as "useless violence" and to legitimate authoritarian rule.

While generally persuasive, there are some ambiguities in this thesis. Moreira Alves may overstate the regime's self-consciousness, foresight, and capacity to manipulate alternative political scenarios in a manifestation of sovereign will. In fact, the thrust of the study's insights and original research on elite and mass opposition appears, at least partially, to contradict the author's interpretation. Events subsequent to the book's completion, culminating in the return to civilian rule in March 1985, illustrate the accelerating disorientation of the regime and its loss of control over the tempo and final destination of the *abertura* process. Notwithstanding this quibble, Moreira Alves makes an important contribution to debates concerning the role of the opposition—particularly labor and grassroots movements—as Brazil under the *Nova República* navigates between another episode of elite conciliation from above and the effort to consolidate democratic institutions and foster a participatory political culture.

This commendably detailed study diverges in several respects from existing interpretations. Perhaps most significantly, it questions the current view that Brazil exemplifies a "transition from above," stage-managed by the authoritarian elites themselves. Moreira Alves does not explicitly address this issue, but the primordial role of the opposition in the Brazilian transition suggests a need for a rethinking of current typologies of regime

change. Indeed, *State and Opposition in Military Brazil* is a compelling argument on behalf of the importance of well-done studies of specific historical cases as the only legitimate basis for valid comparative analysis.

WILLIAM C. SMITH

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Politics in Mexico. Edited by George Philip. (Dover, NH: Croom Helm Ltd., 1985. Pp. 223. \$27.50.)

This edited collection of essays came from a two-day conference held in June 1984 at the Institute of Latin American Studies in London organized by the British-Mexican Society. George Philip, lecturer in Latin American politics at the London School of Economics and the London Institute of Latin American Studies, skillfully coordinated a panel at that conference that related Mexico's financial crisis, raging since 1982, to the political pressures especially evidenced in the fraud-tainted elections in 1982 of congressional and gubernatorial candidates of the dominant Institutional Revolutionary Party (PRI). (Though held a year after the conference, the controversial election of *priistas* (members of PRI) in the congressional and gubernatorial elections of July 1985 provides further evidence for the pessimism of some of its political analyses.)

In the opening chapter, "Causes of Mexico's Financial Crisis and the Lessons to be Learned," William Chislett of the *Financial Times* of London orients readers to factors found throughout the volume. He analyzes Mexico's huge foreign debt against the background of the Mexican oil syndrome. Norman Cox, British ambassador to Mexico during 1978-81, succinctly summarizes the Mexican political system, showing how the dominant PRI must deal with the Communist left and the traditional right. Editor George Philip furnishes a chapter, "Politics Under Stress," that shows clearly that the De la Madrid administration has tried to break sharply from its two immediate predecessors under Luis Echeverría and José López Portillo.

Professor Antonio Juárez of the National Autonomous University of Mexico (UNAM) describes Mexico's Marxist left and its background. Economist Barbara Wijngaard pro-

vides economic perspectives. John R. Heath, a research fellow at the Agrarian Investigation Center in Mexico City, traces Mexico's food policy, linking land tenure decisions to other government programs. Diane Stewart, a research student at St. Antony's College, Oxford, untangles the enigma of Mexico's banking system, complicated by the 1982 bank expropriations.

Roman Gío Argáez, research student at Essex University, covers Mexico's foreign policy during 1982-84, taking the Contadora activities more seriously than have any of the Central American nations more directly involved. Other papers on Mexico's stream of illegal migrants into the United States and on agrarian development complete the volume. In light of the several political and economic crises that have occurred one after the other since 1982, this volume is welcome among those trying to keep up to date on Mexican public life.

Reviewing this book in early 1986, right after the announcement that falling oil prices are squeezing Mexico almost to default, this reviewer, living in the Mexican borderlands, has had to try to resist the escalating pessimism about Mexico. However, the once-bright hopes for the De la Madrid term (1982-88) have dimmed. The Mexican government—risking political fallout—has boldly pursued austerity measures, many of them in 1985 after this book went to press. For example, federal spending was cut 15%, and hundreds of political appointees were dropped from token jobs not guaranteed by the civil servant unions.

Some of the De la Madrid innovations are working, so veteran Mexicanists cannot really proclaim Montezuma's malaise. This splendid little British volume will help political scientists reach an evaluation of Mexico somewhere between the bravado of the government's daily newspaper *El Nacional* and the nattering negativism of the *Washington Post's* correspondent in Mexico City. Technocratic leadership can easily be discerned by those attuned to the Mexican mystique. That leadership stubs its toes now and then but does manage to keep governmental and private-sector workers functioning—probably better than counterparts elsewhere in Central America and the Caribbean.

MARVIN ALISKY

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The Evolution of Modern Botswana. Edited by Louis A. Picard. (London: Rex Collings, and Lincoln: University of Nebraska Press, 1985. Pp. 339. \$37.95.)

The importance of Botswana in the study of African politics is all out of proportion to the size of its population and its historical impact on the development of the rest of the continent. Two factors provide the basis for elevating Botswana to the level of a "critical" case study: (1) its status as sub-Saharan Africa's only independent state with an uninterrupted tradition of democratic government; and (2) its strategic location in the heart of southern Africa. As expected, this interesting and useful collection of essays addresses both of these important issues.

This book, which has its origins in a panel at an African Studies Association convention, brings together 11 essays by 10 different authors, all of whom have done extensive fieldwork in the region. The book is divided into four sections, the first two dealing with the historical development of Botswana; the third section treats the political system, with emphasis on rural development and local administration, and the fourth examines the role played by Botswana in the international arena.

The major emphasis in the work is on the issue of local institutional development, a question addressed by 5 of the 11 essays. Especially interesting is the essay by Malcolm O'Dell, which reexamines the role of the village *kgotla*, a sort of combination local court and town meeting. He suggests that these vital institutions have been largely overlooked as the basis for rural development in the country. Robert Hitchcock, in his examination of land tenure (particularly that regarding pasture areas) emphasizes the complexity of the traditional tenure system and its relation to current government development policies in the livestock sector. Picard and Morgan provide interesting insights into the problems associated with policy implementation through local institutions. This is an important addition to the all-too-limited literature on problems of implementation at the local level in African politics. The remaining two essays in this section provide a summary of the functioning of the party system (Picard) and an assessment of the utility of social class theory as an explana-

tion of rural development in Botswana (Holm).

The historical section is composed of a brief introduction to and summary of the historical development of Botswana (Picard) and essays by Q. N. Parsons and Jack Parson. Parsons offers some reinterpretation of several facets of the nation's history and their likely impact on future events. The third historical chapter is a somewhat unsuccessful attempt by Jack Parson to apply dependency theory to the history and development of Botswana.

The last section of the book moves us into the international arena, with good summaries of the problems associated with Botswana's external linkages. Dale's contribution provides a good description of the actors involved, while Polhemus focuses on the situation of the country in relation to the liberation struggle in South Africa. The final chapter provides an interesting, but not surprising, set of conclusions by Weisfelder, who apparently played the role of discussant for the original conference panel.

The main weaknesses of this volume are the lack of a consistent theoretical theme and the fact that there is little new empirical data presented in the various chapters. The book does, however, bring together good summaries of much past research done on Botswana, and should thus be regarded as a valuable addition to the libraries of those with an interest in the region in general, and Botswana in particular.

RICHARD VENGROFF

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Cities and Services: The Geography of Collective Consumption. Steven Pinch. (London: Routledge and Kegan Paul, 1985. Pp. xi + 213. \$29.95.)

British urban studies have been flourishing vigorously during the last decade, chiefly because they stand at the crossing point between two streams of theoretical development. The first and older, established stream includes American-dominated liberal approaches to analyzing cities and urban problems, especially the contributions made by neoclassical economics, formal spatial modeling, descriptive sociology, and the community power debate and its aftermath. The second

stream consists of the newer, French-influenced wave of Marxist and neo-Weberian sociologists, for whom questions about the production of cities have been far more important than their ideologically magnified social effects. The British literature has tried to meld together some behavioral research technologies with modern urban social theory. Nowhere has this empirical drive been stronger than in a new political geography seated firmly in a preoccupation with urban problems, and trying to connect some inherited geographical concerns about the spatial implications of public policy decisions with the substantive analysis of urban social conflicts and their political management. Steven Pinch's book provides the latest in a line of increasingly plausible attempts to make this amalgam work.

Cities and Services marries two liberal and two newer themes. The first is the impact of jurisdictional fragmentation among local authorities (and state governments in the U.S.) on citizens' access to services, which is handled both by conventional geographical mapping of accessibility to services and by comparative output studies of subnational governments' service provision. The second liberal theme deals with externality problems, the tapering of access to locational facilities with increasing distance, and the general problems of locational conflicts between incompatible land uses. The first of the newer themes concerns the institutional determinants of urban managers' behavior, analyzed in largely Weberian terms of the dynamics of bureaucracy. The last theme addresses the involvement of conflicts over urban public services in three contexts: (1) in broader social struggles, (2) between production and consumption imperatives, and (3) between capital and labor. The links between all these theoretical approaches and particular urban policy problems are convincingly made in a way that students especially will find very useful. These core chapters are topped by a short account of the structures of local government in Britain and the U.S., from which all the book's empirical examples are drawn. A concluding chapter focuses on a number of disparate theoretical developments that are supposed to take us "beyond locational analysis and structuralism." While each of these elements is fairly summarized, there is no really effective synthesis here.

Overall, Pinch has written a useful student text that should have a wide readership within courses tackling urban politics or policy analysis. His consistent concern with the spatial patterning of service delivery may put off some of the *Review's* readers, but this is an important theme that connects very closely with the practical concerns of public administrators and policy analysts about territorial equity, and that political science has tended to neglect. Pinch addresses these issues from a variety of theoretical approaches, drawing on welfare economics, public choice theory, and sociology, as well as on political science and geography.

At a deeper level, *Cities and Services* has some flaws. Its wide attention span occasionally induces a feeling of ungrounded knowledge, as when a variant of substantive individualism is described as "methodological individualism" (p. 6), or when pluralism is classed as a subset of public choice theory (p. 34). The grouping of new developments into the last chapter gives an anachronistic feel to some preceding text, as if the author's reading had rather lapsed after 1981, or the publishers had tarried overlong in getting the book into print. The exclusion of the foreign language urban studies literature is also restrictive, given the bridging role of British urban studies mentioned above, and the adaptation to a period of public service cutbacks and deurbanization seems curiously patchy. Despite such limitations, however, Pinch is a lucid, reliable guide to a complex literature.

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Yugoslavia in the 1980s. Edited by Pedro Ramet. (Boulder, CO: Westview Press, 1985. Pp. xvi + 354. \$27.00.)

Many outside observers felt that the death of Josip Broz Tito in May 1980 would unleash a host of cataclysmic events in Yugoslavia, bringing civil war, incorporation into the Soviet bloc, a military coup, or the rise of a new but less charismatic, legitimate, and capable leader. Happily for Yugoslavia, these visions turned out to be fantasies. The collective leadership designed by Tito to manage his succession proved durable and highly success-

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ful in avoiding spectacular catastrophes. Unhappily for Yugoslavia, Tito's success in institutionalizing this decentralized, almost confederal system has left a legacy of substantial problems: a stalemated government that cannot respond effectively to the nation's intensifying economic crisis, which in turn has produced growing social disillusionment. Pedro Ramet's *Yugoslavia in the 1980s* provides an excellent description and analysis of this growing economic-political-social crisis and paralysis. Unlike most such collections, the contributions are well chosen and integrated to provide a comprehensive picture of contemporary Yugoslavia.

Chris Martin and Laura D'Andrea Tyson describe Yugoslavia's stagflation of the early 1980s, and show how the economic and political institutions constructed in the 1970s perpetuate economic irrationalities and prevent effective policy responses. The problems of political organization are examined in more detail in several other chapters. Wolfgang Höpkin discusses the inconclusive debate over how to reform the League of Communists to reverse its fall "from federalization to disintegration." Dennison Rusinow describes the similar stalemate concerning Yugoslavia's manifest nationality conflicts, and notes how bureaucratic self-interest is changing some of the conventional positions of specific national or ideological groups. Robin Remington discusses several issues in civil-military relations, and concludes that the military is, at least at present, well integrated into the governmental system.

Several case studies indicate how federalization and decentralization have brought policy paralysis. Sharon Zukin analyzes the growing problems in and popular disillusionment with Yugoslavia's unique system of self-management in enterprise administration and local government, and Barabara Jancar demonstrates that the self-management system has inhibited attempts to combat environmental problems. The other chapters on domestic politics also indicate the regional variations and or increased questioning of the current system in the role and autonomy of the press (Pedro Ramet), religious policy (Pedro Ramet), and the generation gap and the growth of "feminism" (Barabara Jancar). In the realm of foreign policy, Zachary Irwin describes Yugoslavia's nonalignment policy,

especially her competition with Cuba over the direction of the Nonaligned Movement, and Othmar Nikola Haberl argues that the central facets of Soviet-Yugoslav relations in the post-Tito period are "reciprocal pin-pricks" politically, and Yugoslavia's growing dependence on the USSR economically.

These essays are well integrated by two theoretical overviews. George Schöpflin presents a model of "political decay" in Communist systems, and shows how it applies to the somewhat different situation in Yugoslavia. (As Schöpflin and several of the other authors imply, a significant cause of Yugoslavia's decay has been the adoption of political reforms that remain anathema to orthodox Marxist regimes.) Pedro Ramet argues that Yugoslavia represents an "apocalypse culture" that is "prepared to question the fundamental political and social values of the society" (p. 3) because of obvious political and economic breakdown. These two theoretical perspectives raise broader questions relating to the study of comparative communism that go beyond the book's excellent conception of contemporary Yugoslavia: Will economic stagnation produce crises of political legitimacy in other Communist states (as has, in fact, occurred in comparatively "liberal" Poland)? How will more orthodox regimes respond to the growth of apocalypse culture? Will the current Yugoslav deterioration inhibit political reform in other Communist countries? There is also some grist for democratic theorists in this volume's unstated central theme that the Yugoslav regime instituted the self-management system in the belief that participation can be an "opium of the people," but found to its sorrow that bread is more important.

CAL CLARK

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Soviet Politics in the 1980s. Edited by Helmut Sonnenfeldt. (Boulder, CO: Westview Press, 1985. Pp. x + 247. \$24.00, paper.)

Soviet Politics in the 1980s is remarkable because of the uniformly high caliber of all 16 chapters, each written by a different scholar. It covers the period from 1960 to 1984. More than half of the chapters deal with the economic life of the Soviet Union, and each

chapter presents a rigorous analysis of Soviet economic stagnation and examines the plans and prospects for reform. There are a number of very informative tables and charts pertaining to the growth of the Soviet economy and to trade between the Soviet Union and the outside world. Each chapter has a very up-to-date bibliography to the end of 1983.

Five chapters deal with political and military matters. Of especially high quality are Thane Gustafson's "The Andropov Accession," Steven Rosefield's "Soviet Arms Buildup or US Arms Decline?," Dimitri K. Simes's "Can the West Affect Soviet Thinking?," Wolfgang Leonhard's "Soviet Foreign Policy: Interests, Motives and Objectives," and Fritz W. Ermath's "The United States and the Soviet Strategic Challenge." The final two chapters are really a brief essay by Richard Pipes, "US Policy Opportunities," and a short speech by Lawrence Eagleburger, "US National Interests and the Soviet Union."

This book constitutes a periodic check up of the body politic of Soviet totalitarianism by a highly specialized team of experts at a time of internal erosion and external expansion in the USSR. It should be of particular interest to policy makers in Washington, for whom this work was primarily intended. The authors of the chapters were commissioned in 1982 by the United States Information Agency, the Georgetown University Center for Strategic and International Studies, and the Kennan Institute for Advanced Russian Studies in Washington, D.C. to present papers, which were exposed to vigorous discussion during a series of four one-day conferences held in 1983. Subsequently, the authors revised their papers, which appear in this book in their final form. This whole process was initiated by the policy makers in Washington, who were eager to benefit from experts in the field of Soviet studies. It is very encouraging that government officials and policy makers are anxious to cooperate on an intellectual level with outstanding scholars. This work is of such quality that it will also be useful to academicians at large who want to keep up with the latest developments in the Soviet Union.

The verdict of the experts is that the Soviets have not found a magic solution to their archaic and ailing economic system, and that reforms have encountered resistance from the rigid and inefficient bureaucracy. There ap-

pears to be no prospect that Soviet domestic and foreign policy will change substantially under Gorbachev. None of the authors implies that Soviet dictators will become Jeffersonian democrats. Like his predecessors, Gorbachev undoubtedly will make every effort to eliminate fear of Soviet aggression and to confuse and divide Western sentiment about long-term Soviet intentions. Although there has been a rapid succession of general secretaries since Brezhnev, the fundamental nature of Soviet totalitarianism remains the same. Contrary to wishful thinkers in the West, the Soviet regime does not shed its police-state characteristics; it dies when power is wrenched from its hands.

This reviewer's only regret is that Solzhenitsyn, Bukovsky, or other distinguished refugees from the Soviet Union were not invited to contribute to this book. Their enormous first-hand knowledge of the inside workings of the Soviet Union would have added luster to the high quality of the book. The calibre of Dimitri Simes's chapter is perfect proof that gifted and knowledgeable Russian immigrants have a more realistic grasp of American-Soviet relations and prospects for genuine coexistence than even the most able outsiders.

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Political Tolerance on Context: Support for Unpopular Minorities in Israel, New Zealand, and the United States. By John L. Sullivan, Michal Shamir, Patrick Walsh, and Nigel S. Roberts. (Boulder, CO and London: Westview Press, 1985. Pp. xv + 264. \$20.00.)

In *Political Tolerance in Context*, John Sullivan, Michal Shamir, Patrick Walsh, and Nigel Roberts continue a line of research that Sullivan started with James Piereson and George Marcus in a series of publications analyzing political tolerance in the United States. Readers familiar with that earlier research will recognize the methodology, as well as many of the same conceptual and theoretical arguments, in this latest effort comparing political tolerance in the U.S., New Zealand, and Israel.

The heart of the book is devoted to a careful analysis of survey data collected in the United

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States in 1978; in Christchurch, New Zealand in 1980-81; and in Israel in 1980 (only Israeli Jews are analyzed). The authors used a "content-free" question format to measure levels of tolerance in these countries. Respondents were asked first to identify the groups in their society they disliked the most, and then to indicate if they would concede a number of basic civil liberties to members of these groups. Both New Zealanders and Americans target a variety of left-wing and right-wing groups, while a large majority of Israelis focus their antagonism on left-wing groups, especially Communists and groups that support the PLO. New Zealanders, it turns out, are more willing than both Americans (who rank second) and Israelis to extend basic liberties to the groups they dislike, largely because they feel the least threatened by their outgroups.

Surprisingly, in light of the varying degrees of threat posed by outgroups in these countries, the authors conclude that New Zealand is the most tolerant of the three nations. This conclusion appears to violate the original logic of the content-free method. When Sullivan, Piereson, and Marcus introduced this method to study changes in political tolerance in the U.S., they contended that existing studies of change had erred by re-asking present-day Americans a series of questions about Communists that were originally asked in surveys conducted in the 1950s. Because Communists had become less frightening to Americans, they argued, it was now necessary to ask about outgroups that posed as much of a threat to Americans today as Communists did during the Cold War. However, if we follow this logic, we also cannot say that New Zealanders are more tolerant than Americans and Israelis. Just as current Americans fear Communists less than the 1950s generation of Americans did, so do New Zealanders fear their target groups less than Israelis and Americans fear the respective target groups they identify.

In any event, inconsistencies aside, neither the earlier nor later logic behind the content-free method rescues this approach from being a poor measure of the level of tolerance in a society. Knowing the proportion of citizens in a society that will tolerate their least-liked group tells us little about how much tolerance prevails in that society. For example, if the citizens of one country tolerate a range of groups from A to G on the left-right con-

tinuum, but not least-liked group H, while the citizens of a second country are intolerant of all groups from A to G in addition to being intolerant of least-liked group H, it is obvious that the first country is more tolerant than the second. Yet, according to the content-free criterion, the two countries share similar levels of tolerance. The content-free method falters because it summarizes the level of tolerance in a society on the basis of an extreme data point. Instead, we should examine the range or variety of groups in a society to which people are willing to extend basic civil liberties.

Despite this conceptual and operational problem, Sullivan and his colleagues provide a valuable analysis of the social, psychological, and political factors that contribute to tolerance of the most unpopular groups in the U.S., New Zealand, and Israel. The model they develop specifies that an individual's level of tolerance is the product of four major factors: degree of support for democratic norms, personality, political ideology, and perceptions of the target group. Demographic factors such as education, social status, age, and religion operate largely in the background by influencing the development of political ideology and various personality characteristics.

In their data analysis, the authors show that personality factors have a consistently strong impact in all three nations; flexible, self-confident individuals are more tolerant than those who are dogmatic and lacking in self-esteem. Similarly, the perception that the target group is threatening greatly inhibits the expression of political tolerance.

The effects of ideology and democratic norms vary cross-nationally. For instance, political ideology has a stronger impact on tolerance in Israel than in the U.S., largely because in Israel, the effect of ideology reflects not only the positive effect of liberal norms, but also the somewhat greater sympathy that liberals, relative to conservatives, have for the predominantly left-wing target groups.

General democratic norms and specific legal norms have a much weaker impact in Israel than in New Zealand and the U.S. The authors suggest two reasons for this discrepancy: first, the concept of minority rights has, historically, been less honored in Israel than in either of the other two nations; and, second, the precarious life circumstances of the Israelis makes even those who are strongly wedded to democratic

norms reluctant to tolerate the groups they fear.

All told, the authors develop a strong, well-organized argument about the variety of influences that affect the development of political tolerance. They are attuned to significant differences in the political and social circumstances of the United States, New Zealand, and Israel, and to the effect these variations will have on the structural coefficients of their causal model. The data analysis is sophisticated and well documented. Although it is based on a problematic measure of political tolerance, the book is valuable and instructive.

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Workers, Participation, and Democracy: Internal Politics in the British Union Movement.

By Joel D. Wolfe. (Westport, CT: Greenwood Press, 1985. Pp. xii + 258. \$45.00.)

Ever since Michels argued that democracy was impossible in large-scale organizations, critics have sought to show that it is not. These critics reject Michels's iron law of oligarchy, as well as what Gouldner called the "metaphysical pathos" of pessimism and fatalism that accompanies it. Nor did attempts to save democracy by redefining it as elite competition satisfy these critics. If this was democracy, it was hardly worth defending. Rather, these critics argued that a participatory democracy was possible in which members set agenda and held leaders accountable, instead of being manipulated by them. This is Wolfe's task—to argue that "under certain circumstances, leaders are compelled to act on behalf of their membership rather than to exercise power over them" (p. 17).

Support for the theory of participatory democracy that Wolfe presents in this book is based on his analysis of the policy process within the British trade union movement as it debated two issues toward the end of World War I: a reconstruction program for industry and policy towards the war. In each case, Wolfe claims that the union membership was able to articulate its own position on these issues, and compel the leadership to comply with it.

Wolfe argues that collective interests among workers can form as a result of forces from outside the union, and thus be beyond manipulation by the union leadership. He locates these outside forces in the workplace, which can engender collective interests among workers under certain conditions. These conditions include the ability of workers to exercise job control that permits informal workgroups to form; job security due to full employment, which gives workers bargaining power; increasing differentials between wages and profits, which stimulate feelings of injustice and exploitation; and state repression that politicizes and standardizes workplace grievances. These conditions create shared interests among workers, who then make their support of the union leadership conditional on the leadership's ability to satisfy the externally derived interests of the membership.

This, to a large degree, is what Wolfe found with regard to the relationship between union members and leaders in Britain during World War I. Led by shop stewards that emerged from the direct democracy of workers on the shop floor, skilled workers resisted conscription, fought dilution in which lower-skilled workers entered craft jobs, demanded that profits be stabilized if wages were to be restrained, and supported peace-by-negotiation to end the war. In each instance, members were able to force union leaders to respond to these demands from below.

The context of democratic theory in which Wolfe analyzes the internal politics of British trade unions is original and provocative, but his interpretation of the events upon which his theory of participatory democracy is based troubles me. First, Wolfe claims that British workers experienced a decline in living standards during the war, when his own data (Table 3.2, p. 61) shows this was not the case at all. While average weekly wage rates did indeed slip noticeably behind retail prices during the war, average weekly earnings did not, and it is the latter, not the former, that determines purchasing power. The war also squeezed wage differentials between skilled and unskilled workers. This leads to my second criticism of his analysis: the extent to which Wolfe portrays the shop stewards' movement in Britain as a class-conscious movement rather than a defense of craft privilege and exclusiveness. The shop stewards' movement was based

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among skilled workers, who felt that the way the wages of unskilled workers were catching up was as unjust as the way profits were racing ahead. The dilution issue was theirs, and not the unskilled workers'.

Finally, it is not clear that a special set of workplace conditions is enough to create a shared standard among workers, against which they can measure the performance of their leaders. If the workplace was so determinative of interests, why didn't the shop stewards' movement receive broader support than it did, even among skilled workers? For example, why wasn't the shop stewards' movement among engineers as successful in Barrow as it was on the Clyde? Workplace conditions were similar in each locale, but skilled engineers exhibited different degrees of militancy. Other factors need to be included to account for the militance of some workers that were transmitted up the union hierarchy during the war.

Despite these criticisms, Wolfe's blend of history and theory demonstrates how powerful the two can be when they are used to enrich each other.

ALAN DRAPER

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The Rise and Decline of the Zairian State. By Crawford Young and Thomas Turner. (Madison and London: The University of Wisconsin Press, 1985. Pp. xix + 500. \$37.50.)

The authors' aims are typically discrete:

Through the prism of the state . . . to compose a portrait of politics in the Mobutu era, from 1965 to 1980. . . .

. . . to provide insight into Zaire in the Mobutu era, and [make] a modest contribution to comparative understanding of the contemporary state. (pp. 6, 46)

Nevertheless, readers will recognize this excellent volume as a major contribution to the study of politics in Zaire. Its broad scope, careful research, and meticulous attention to empirical detail make *Rise and Decline* a worthy successor to Young's earlier and equally comprehensive treatment of politics in pre-Mobutu Zaire (*Politics in the Congo*, Prince-

ton University Press, 1965). Clearly and elegantly written, this lengthy book will amply reward those who read it. Its chapters provide a wealth of information and analysis on the Zairian polity, society, and economy during the Mobutu years.

The authors discern two distinct cycles of rise and decline between 1885 and 1980. The first ascendant period coincides with the construction of the colonial conquest state, which begins in 1885 and lasts until 1958. The initial period of state decline starts two years prior to independence in 1960, and ends with Mobutu's seizure of power in 1965. Then follows a temporally telescoped repetition of the cycle under Mobutu. The years 1965 to 1974 witness a period of reconstruction and expansion of the Zairian state along all dimensions, which, in turn, leads to a second era of dissolution and decay (p. 7). Although they review the first cycle, the authors devote the vast majority of their attention to the Mobutu years. Throughout their narrative, which treats not only the rise and decline of the state, but also the state's relations with civil society in each of these phases, the authors maintain that

the state itself is the most decisive single factor in the complex pattern of mutations and reformulations of social consciousness within civil society. The nation-state . . . provides the framework, context, and situation which shape the particular class and ethnic configurations at any given moment. (p. 162)

Young and Turner skillfully weave astute political analyses into their tale. Especially noteworthy is their explanation of why the Mobutu regime has not given way to a major social upheaval, despite its less than exemplary record in managing the economy, its inability (and perhaps unwillingness) to decrease the widening gap between rich and poor, its failure to stem the enormous erosion in the standard of living, its cavalier view of human rights, its inability to guarantee the state's territorial boundaries (Shaba I and II), and its extraordinary corruption. Their response to the paradox of Mobutu's durability entails a discussion of the still-pervasive myth of educational mobility; the widespread belief that external powers guarantee the regime; the increasingly important survival mechanisms of the informal economy (p. 137); and the presence of a system of kinship-clientelage that sus-

tains the hope of eventual social mobility for the lower classes (p. 160). To these, I would also add the very real fear of violent retaliation by the state's various coercive arms.

Despite this work's substantial merits, two critical comments need airing. First, a quibble. I am not persuaded that the second decline begins in 1974. If we accept the authors' main premise concerning the cyclical nature of the state, it is still possible to argue that the decline begins somewhat earlier. To be sure, by 1974 it had become visible for all to see, but I suspect that it actually began in either late 1970 or early 1971, when the relative financial health engendered by the currency reforms of 1967 began giving way to the budgetary and financial manipulations necessary to underwrite Mobutu's grandiose vision of Zairian development. It also became clear at about that same time that Mobutu would not honor his pledge to relinquish power after five years. Moreover, if we reject the main premise, could it not be argued that both periods of postindependence decline result from a single systemic crisis spawned by the nature of the colonial experience and capitalist penetration?

Second—and this is a more serious failing—Young and Turner's theoretical framework does not do justice to their fine political analyses. In chapter 1, the authors review various definitions of the state and find those of Poggi, Skocpol, and Dyson especially impressive. Yet they neither embrace one of these nor offer their own formulation. They maintain that "the complexity of the state as a concept makes impossible its reduction to a single dimension, or its definition in a parsimonious phrase" (p. 12). Instead, the authors list a series of essential attributes, or defining characteristics—territory, sovereignty, nation, a set of institutions of rule, a legal system, and the state as an idea which exists in the minds of officials and citizens (pp. 12–13)—that, when coupled with a list of the state's behavioral regularities—the imperatives to maintain territorial hegemony, uphold and advance state security and independence, maintain some form of political legitimacy, and provide revenue (pp. 15–16)—are supposed to provide a framework for understanding the state.

The difficulty is that these lists of attributes and regularities are functional requisites rather

than a definition. Young and Turner appear to have sought the essence of the state and found an answer based on what it does during periods of ascendancy. While in decline, however, states neither perform all these functions nor display all these attributes. If this is the case—and the authors' subsequent analysis shows convincingly that it is—then which are the key defining characteristics of the state? Which are necessary *and* sufficient conditions for what J. P. Nettl once termed "stateness"? When, according to these criteria does a state stop being a state? And what does it then become? The question of necessary and sufficient conditions also becomes important if one wishes to examine the state in historical perspective. Their list of defining attributes is clearly influenced by European history. While this European orientation complements the importance of the colonial state, it tends to ignore other forms of rule that evolved in pre-colonial Africa. Using these definitional criteria, would the authors have recognized states in precolonial Zaire, where kingdoms and empires also arose and declined? Probably not, and this reduces the utility of their perspective for comparative analysis.

The authors' functional framework successfully handles the state in its periods of ascendancy, but seems inadequate for periods of state decline. This is most unfortunate, because while correctly recognizing that most theoretical visions assume, whatever the functions of the state, that states somehow rationally and systematically relate means and ends, Young and Turner demonstrate conclusively that this is no longer true in Zaire during periods of decline:

In Zaire, the central feature is in the shrinkage of the competence, credibility, and probity of the state. It has progressively lost its capacity to relate means to ends, bringing about a loss of belief, within civil society, that the state can be expected to perform its accustomed functions. (p. 45)

The question of state decline, like others the authors raise, will require more reflection and further reformulation of our understanding of the contemporary state.

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INTERNATIONAL
RELATIONS

Between Washington and Jerusalem. By Wolf Blitzer. (New York: Oxford University Press, 1985. Pp. xii + 259. \$15.95.)

The Future of American-Israeli Relations: A Parting of the Ways? By James Lee Ray. (Lexington: University Press of Kentucky, 1985. Pp. ix + 157. \$16.00.)

Here are still two more works devoted to American-Israeli relations, both readable and useful. Both were published in 1985, but presumably begun a couple of years earlier. Hence, not surprisingly, both reflect many of the questions about the relationship between the two countries that accompanied the 1982 Lebanon war and its aftermath. Aside from that point of convergence, the two works provide a vivid contrast, and reinforce this reviewer's impression that almost all works on the Arab-Israeli conflict must—if they are to make sense—be interpreted according to the sheer viscera of their authors. Yes, there are degrees and gradations, but we are virtually all either for or against Israel, and such sentiments rarely change in response to new facts or circumstances. One can think of few other subjects where sentiments (phobias, obsessions) drive analysis to the same degree, whatever the scholarly or analytical veneer.

Blitzer, author of *Between Washington and Jerusalem*, is the Washington correspondent for the *Jerusalem Post*, an American fluent in Hebrew, and a Washington insider who also has contacts in the highest echelons of Israeli politics. He also claims a personal role on the route to Camp David as the result of a question posed to Anwar Sadat at a news conference. His political sympathies reflect the ideological position of the *Jerusalem Post*, and appear to be those of a Labor Party moderate or of the moderate left in the U.S.: he is very sympathetic to Israel and is concerned about its survival. His book is very well written and laced with interesting anecdotes, personal portraits, and vignettes. His main strength, again, is his intimate knowledge of the Washington scene. He covers the Washington bureaucracy (primarily the State Department and its crucial

bureau of Near Eastern Affairs; the Israeli presence in Washington (primarily the roles of several recent Israeli ambassadors); strategic cooperation; the CIA-Mossad connection; Congress; the political role of American Jews in relation to Israel; the news media and think tanks; business, labor, blacks, and Christians; and the crucial figures of Henry Kissinger, Jimmy Carter, and Ronald Reagan. This is a quite comprehensive volume, a *tour d'horizon* of the past and present relationship between the U.S. and Israel.

Generally, Blitzer sees this relationship—with all its ups and downs and despite the recent trough of 1982—as stronger at present than in the early years of Israel's existence, though also rather consistent since 1967. What stands out amid the blizzard of detail is the stark, vivid portrayal of the division between pro- and anti-Israeli individuals and institutions—you can't tell the players without a program, indeed! The reader gets a real panorama of Israel's friends and enemies—it's all here between the covers of one book. There are few surprises—although this reviewer was surprised to read that Secretary of State Marshall's anti-Israeli animus had extended as far as an open threat to vote against Truman in the 1948 election if the president went ahead and recognized Israel.

Blitzer is not, of course, the first commentator to point out that pro- and anti-Israeli sentiments and/or policy preferences do not measurably correlate with broader political ideology or with party or institutional identification. There are supporters and critics of Israel among liberal and conservative Democrats and liberal and conservative Republicans alike. There are ex-CIA chiefs, flag-rank military officers, black leaders, and even conservative think tanks on either side. However, Blitzer's own, albeit mild, political biases may have resulted in a few shadings of perspective. From this (neo-conservative) reviewer's perspective, he appears to understate the depth of the Black-Jewish problem as it applies to Israel. He also appears overly, stubbornly suspicious of fundamentalist Christians' support of Israel,

and seems to try a bit too hard not to see the powerful animosity against Israel that exuded from Jimmy Carter and much of his entourage. (Ask Mayor Koch!) Blitzer's book was published, incidentally, before the now startling (if it is durable) shift of Senators Jesse Helms and Steven Symms to a pro-Israeli orientation, a switch of potentially major political import.

In contrast, *The Future of American-Israeli Relations* is a more self-consciously academic, though—somewhat paradoxically—also far more prescriptive in tone and intent. It is a lengthy essay in which the author gropes ambitiously for a solution to the core of the Middle Eastern conflict. In the process, he tries to find policy applications for some of political science's empirical theories. Bruce Russett and Paul Huth are invoked in connection with possibilities for a U.S.-Israeli defense alliance, and some theoretical literature on nuclear proliferation is called upon for answers as to whether Middle Eastern proliferation would or would not promote stability or peace.

However, I must admit to some bafflement about exactly what Ray is up to in this book. The author himself confesses dilettantism in conceding that he is not a specialist on the Middle East (there is no information about whether he is well traveled or well connected there). No doubt it is a bit out of bounds to do so, but I'll speculate about some of his purposes.

The tone of the book is unmistakably pro-Arab, though the author often issues—perhaps revealingly—virtual disclaimers. One has the impression that despite its aspirations of being "utilitarian" (do the best we can for both sides), and despite the previously noted efforts at applying social science concepts and frameworks, the book is somehow a very personal statement, even a catharsis of sorts. The author often appears to be addressing himself as audience. I take him to be a long-time, visceral Arabist, but one somehow so uneasy about it as to have written almost by way of expiation, if not of mere explanation.

Ray cursorily but effectively reviews the history of the conflict at the outset. That brief history will make you happy or sad, depending upon which side you are on. The Israeli Gaza raid in 1955 is seen as virtually naked, unprovoked aggression, not as a response to repeated fedayeen raids. Eisenhower and Dulles are applauded for leaning on Israel after

the 1956 war. One would almost never suspect that the West Bank, including East Jerusalem, and Gaza were under Arab control for 20 years preceding 1967, and that peace did not result therefrom. The Israeli lobby looms large, though there is the generous concession that its power may have been overrated. There are the telltale quotes from long-time Arab supporters William Quandt and Seth Tillman, and an acknowledgment of the advice of Arabist Augustus Norton.

Generally—and in contrast to Blitzer—Ray sees American support for Israel as having waned after 1982, or at least having become more fragile or ephemeral. In that connection, however, the meaning of the book's subtitle, *A Parting of the Ways*, is not altogether clear. It is surely not the major theme of the book. Is it, rather, a wish-fulfillment fantasy?

At the heart of the book are its chapters on "Alternative Futures for the Administered Territories" and "U.S. Relations with Israel: What To Do." Under the former are sections on "Autonomy for the Territories," "The Jordanian Option," and "An Independent Palestine on the West Bank and Gaza"; under the latter are discussions about "Forming an Alliance with Israel" (not feasible), and "Introduction of American Troops" (also not feasible), and—curiously so in this context—"A Nuclear Middle East" as an American option. In a way, the author's presentation of the latter options must reveal his unstated assumption that the formation of a Palestinian state would not bring peace; rather, Israel is required to devise dramatic new policies to sustain its survival.

Above all, Ray is very determined that Israel relinquish the West Bank and Gaza for some sort of Palestinian state. In the manner of George ("let's make Israel save itself") Ball, he concludes his book as follows:

Such well-known Israelis as Abba Eban see quite clearly the direction in which Israel is heading: "If we were to hear that some people in Holland were eager to incorporate 4 million Germans into their country against their will, we would conclude not that the proposers of the idea were patriots, but that they were demented. Yet the idea that Israel can permanently impose its authority over a foreign population that constitutes 30 percent of its own size is solemnly put forward as if it were a rational alternative. . . . There are two nations, not one, in that area, so that any unitary political structure is bound to be coercive, artificial, and morally fragile.

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Such an argument might be labeled "pro-Arab," but coming from a person of the stature of Abba Eban it cannot reasonably be called "anti-Israel." For the sake of Israel, and for the sake of the future of American-Israeli relation, I hope that Israelis who share Eban's views on the future of the occupied territories will prevail. They may, with a little help from their friends.

Politically and psychologically, that is the book's gravamen, particularly when it comes to the threatened "help."

ROBERT E. HARKAVY

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NATO in the 1980s: Challenges and Responses. Edited by Linda Brady and Joyce P. Kaufman. (New York: Praeger Publishers, 1985. Pp. xi + 274. \$39.95.)

Most edited volumes on policy issues have a number of characteristics in common. They often are based on papers presented at a conference held a few years back; they frequently suffer from the lapse of time between preparation and publication; and they usually include at least a few valuable contributions. This volume is no exception.

The collective expertise of the contributors is impressive. The group, led by Linda Brady, includes a number of present or former Pentagon officials and a mixture of scholars from outside the government. The volume is organized with good effect into chapters on "challenges" (military, economic, and political) followed by analyses of "responses."

The book opens with two solid, politically mainstream chapters by Linda Brady. Leading to Brady's first chapter, the introduction suggests that NATO faces a "serious, multi-dimensional challenge" in the next several years but that "the challenge can be met, provided that members of the Alliance demonstrate once again the political will that has served NATO so well" in the past (p. 1).

Brady's second chapter focuses on the "military dimension of the challenge." Brady wisely avoids a simple bean-counting approach to the analysis and focuses instead on some political implications of the balance of advantages and disadvantages between the Warsaw Pact and NATO. She sensibly observes that even with all of NATO's deficiencies, the Soviet Union is

not likely to attack "under present conditions" (p. 34). With an equal measure of wisdom, she observes that NATO countries must continue to improve its defensive capabilities in order to maintain the current deterrent standoff.

Three other chapters fill out the "challenges" section of the book. James Blaker's essay on out-of-area issues and NATO burden sharing focuses on an issue that was hot when the chapter was written, but which has cooled in the last few years. Nonetheless, his presentation provides useful background information and analysis. Robert Black's chapter on the economic dimension of the challenge is comprehensive, but is framed by assumptions notably more pessimistic than the tone set by Brady concerning NATO's future ability to deter the Soviets and to maintain economic and social peace within the coalition. Black's relative pessimism is balanced by Martin Edmonds's somewhat more sanguine presentation on European approaches to NATO. Edmonds nicely pulls together a number of themes that emerged in earlier chapters, focusing particularly on the requirement for effective American leadership. In Edmonds's view, this means leadership carefully balanced between attempts to improve East-West relations and NATO defenses.

The section on "responses" opens with Joyce Kaufman's discussion of NATO's force modernization programs. This chapter outlines NATO's strategy of flexible response and the alliance's attempt to deploy nuclear and non-nuclear forces to support that strategy. This is a good review of the relationship between NATO's strategy and force posture, but it never gets very far beneath the surface of the issue. Perhaps due to space restrictions, it does not outline the multiple dilemmas that have made it impossible to "perfect" flexible response in the past, and that will probably continue to do so in the future.

The chapter by James Clay Thompson and A. Grant Whitley is perhaps the most novel in the book. For over three decades NATO leaders have accepted the theoretical value of standardizing alliance weapons systems or at least guaranteeing their interoperability. They have failed more often than they have succeeded in accomplishing these purported objectives. The brief analysis in this chapter, apparently a summary of some much more extensive research, puts some scientific meat

on the bones of the logical assumption that NATO forces would fight more effectively if they had standardized equipment. The case made by the authors deserves close attention if the allies hope to strengthen conventional deterrence in a future that promises serious resource constraints on both sides of the Atlantic.

James O'Leary, in his chapter on responses to economic challenges, argues that "NATO members are economically vulnerable in ways that imperil allied cooperation in diverse strategic and diplomatic arenas" (p. 166). O'Leary's statement of the problems may seem overly alarmist to some readers, and simply objectionable to others. Most Europeans, for example, would not buy his description of their motivations for detente as resting on "flawed" assumptions (p. 185). Nevertheless, his stress on informal consultation and collaboration rather than "grandiose designs" for dealing with economic security issues is a pragmatic approach to dealing with such problems among the allies (p. 186).

John Borawski's chapter on arms control and European security and Richard Eichenberg's on public opinion are both substantial and well-written contributions. Borawski effectively traces developments in the negotiations on intermediate-range nuclear forces, and mutual and balanced force reductions. The author is perhaps too skeptical about the potential value to NATO of these negotiations, but the checkered history of attempts to constrain the military confrontation in central Europe perhaps justifies a cautious approach.

Richard Eichenberg's chapter summarizes the results of his extensive analysis of European and American poll data on security issues. His analysis puts in perspective issues that tended to get exaggerated in the headlines of the early 1980s—spreading "pacifism" in Western Europe, the generation gap on security issues, and the European-American parting of the ways on defense requirements. Eichenberg argues that existing transatlantic differences in perspective will continue to produce frictions over alliance policies, but he finds no cause to believe that the alliance's base of support is in immediate danger.

The editors chose to wrap up the volume with a short chapter on NATO's future. The attempt at summarization is not particularly successful. It is too brief to give fair treatment

to the chapter heading or to reflect in anything but generalities the themes exposed in the volume.

In spite of the inevitable shortcomings of such a volume, this collection includes a number of interesting essays. Those gems make the book a useful contribution to the literature on the Western alliance.

STANLEY R. SLOAN

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The Regionalization of Warfare: The Falkland/Malvinas Islands, Lebanon, and the Iran-Iraq Conflict. Edited by James Brown and William P. Snyder. (New Brunswick, NJ and Oxford: Transaction Books, 1985. Pp. xi + 291. \$34.95, cloth; \$14.95, paper.)

The Falklands War: Lessons for Strategy, Diplomacy and International Law. Edited by Alberto Coll and Anthony C. Arend. (Boston: George Allen & Unwin, 1985. Pp. xiv + 252. \$27.50, cloth; \$12.50, paper.)

The three wars of 1982 (and the Grenada invasion of 1983) have spawned a number of volumes that catalogue a variety of political, diplomatic, legal, strategic, and military lessons, and describe and analyze various facets of these small but significant wars. These two volumes add to this literature, but *The Regionalization of Warfare* suffers from a number of flaws, whereas the more focused *Falklands War* represents a much more valuable addition to the corpus of studies on these subjects.

Generally, the more ambitious Brown-Snyder collection fails to cohere as an integrated work. The three cases are treated very unevenly. For example, the chapters on the Falklands focus primarily on the military lessons, while the Iran-Iraq chapters emphasize the international political, domestic, and broadly defined strategic facets of that war. Furthermore, for some inexplicable reason, the Falklands section contains two chapters on general military precepts, with considerable overlap and no evidence of a meaningful division of labor. The emphasis throughout (with several noteworthy exceptions) is on description rather than analysis, and many of the chapters evince little organization, do not flow well, and make no contributions to an endur-

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ing literature on the wars of the early 1980s.

The weakest part of the book is section 1, on the Falklands/Malvinas. The first chapter, by David R. Segal and Katherine Swift Gravino, offers some useful generalizations about organizational cohesion and other dimensions of military sociology, but then meanders into a recounting of the origins and unfolding of the events of the war (a superfluous section, in view of an introduction that sets the stage for the actual chapters very nicely). Over 9 of the 20 pages of this chapter constitute a description of the war itself. Chapters by Dov Zakheim and John F. Guilmartin, Jr. are fairly well done, but both explore the same subject, and neither will serve as a definitive treatment of military lessons emanating from the Argentine-British battles.

Section 2, on Lebanon, contains two pedestrian but worthwhile chapters, one by Stanley Sienkiewicz on the military lessons of the Israeli invasion in 1982, and the other on the interests and consequences of the war for the many participants in the Lebanese arena. The latter, by William W. Haddad, provides a lucid overview of the implications of the conflict and its outcome for Syria, Israel, Lebanon, and the other actors in the maelstrom.

Clearly, part 3, on the neglected Iran-Iraq conflict, is the most useful of the case studies (although there is an almost complete neglect of the military lessons dimension). Chapters by coauthors Jack S. Levy and Mike Froelich and by Arthur Campbell Turner both zero in on the causes of the war and provide a balanced juxtaposition of social science and more traditional discussions, respectively. Interestingly, however, Levy and Froelich conclude that the Iraq-Iran territorial dispute was a pretext and not a cause of the war, while Turner maintains that control of the Shatt-al-Arab River was the central issue in Iraq's decision-making calculus. John D. Robertson rounds out the section with a comprehensive and sophisticated account of the statics and dynamics of Soviet interests vis-a-vis the two belligerents.

Part 4 features lessons for the United States and the Soviet Union. Chapters by Donald E. Nuechterlein (on U.S. national interests) and Schuyler Foerster (on Soviet perspectives) are both excellent. Foerster, in particular, weaves together in a masterful fashion lessons on technology/tactics, sustainability, survivability,

the Kremlin's view of seapower, the use of Afghanistan as a Soviet "tactical laboratory," and dilemmas for Moscow with respect to clients and conflicts.

The Falklands War traverses a broad range of topics, and does an impressive job of covering the legal, diplomatic, strategic, political, and military lessons and implications (although the military sphere receives less representation in proportion to the others). Actors ranging from the two combatants and the United States to relevant regional and international organizations are profiled in detail. Issues including Antarctica as a potential Argentine-British conflict arena, the problem of management of boundary disputes in current world affairs, the principles of collective security and peaceful change, and the salient facets of the conflict are all discussed trenchantly.

The 15 chapters are generally well written and reflect careful, solid research. They form a coherent volume, in marked contrast to too many edited collections in international relations. While a few of the contributions are sketchy, none of the chapters is weak, and most are lucid and analytically sound. The book progresses well from part 1, on international law (the only section in which there is some repetition), to a second part on diplomacy, a third section on strategic, military, and political dimensions, and a brief but unusually effective concluding chapter by Alberto R. Coll.

Several particularly illuminating chapters should be singled out for special praise. Alberto Coll's chapter on philosophical and legal dimensions provides a lucid overview of the international law dimension, as does Anthony C. Arend's discussion of "the failure of the international legal order." In the section on diplomacy, Inis L. Claude contributes an outstanding chapter on the role and impact of the United Nations. Finally, Dov S. Zakheim's chapter on strategic, military, and technological lessons is a tour de force, and represents the best single published treatment of these issues. Zakheim chronicles an array of lessons, ranging from the strategic (deterrence and defense, naval forces in contemporary crisis management, planning for low probability contingencies) and the military (the role of naval surface forces, the relevance of amphibious missions, logistics) to the technological

(with an emphasis on aircraft, missile systems, and submarine warfare).

GERALD W. HOPPLE

International Information Systems

Cuba's International Relations, The Anatomy of a Nationalistic Foreign Policy. By H. Michael Erisman. (Boulder, CO: Westview Press, 1985. Pp. xv + 203. \$34.00, cloth; \$13.95, paper.)

Students of Cuba's foreign policy continue to struggle with the paradox that while Cuba's influence is felt well beyond the Caribbean Basin, its dependent status and membership in the Communist bloc and in the Non-Aligned Nations Movement (NAM) produces conflicting pressures that Cuban policymakers cannot always escape. Like other actors seeking to maximize security and advance core foreign policy interests, Cuba uses its political, military, ideological, and leadership assets in an integrated fashion. Its principal domestic source of policy is still nationalism. In effect, Erisman's work focuses on the nationalistic dimension of Cuban foreign policy, while the larger goal is "to trace and analyze the overall evolution of the Cuban Revolution's foreign policy, with special emphasis on the globalist phase from 1975 onward" (p. xiv). Changes and developments from 1959 through the early 1980s are examined in four issue areas: Cuban-United States relations, Cuban-Soviet relations, Cuba's Third World policies, and Cuba's activities in Latin America. The book's six chapters are "An Overview of Cuban Foreign Policy," "The Early Evolutions of Cuban Foreign Policy," "Incipient Globalism," "The Maturation of Cuban Globalism," "Contemporary Cuban Globalism," and concluding comments with an assessment of future prospects. Several speeches delivered by Fidel Castro between 1971 and 1981 are included, and are useful for reference purposes.

The book's basic theme is that "nationalistic considerations have been among the most enduring elements" in Cuba's quest for security, in its defiance of the United States, in its efforts to promote revolution in some Latin American countries, and in its occasional

rebuffs of the Soviets (p. 12). Nationalism, rather than orthodox Marxism-Leninism or enduring sympathy for Soviet-style communism, is what drives Cuba's globalism, which is itself tempered by pragmatism and a realization that "objective conditions" constrain Cuba's behavior. Erisman correctly points out that Cuba's advocacy of radicalism in the 1960s can "most generally be deemed a failure" (p. 25), but that the seeds of Cuban globalism "were quietly planted in Africa in the 1960s" through contacts between Cuban operatives with African revolutionaries (p. 32). Persistence is one of the basic features of Cuba's foreign policy, even if tactical considerations mean that long-term goals are not always pursued with vigor and dispatch. In the 1970s, Cuba cultivated radical and anti-imperialist allies among governments and non-ruling but nationalist "liberation movements," hoping to gain leverage if and when some of them came to power. This was the case with the Popular Movement for the Liberation of Angola (MPLA), and the Frente Sandinista de Liberación Nacional (FSLN) in Nicaragua. Ties between these groups and Havana go back over 20 years. In the process, Cuba engaged in what it called "military internationalism" in Angola and Ethiopia—costly commitments on behalf of pro-Soviet dictatorships that are not going favorably for Cuba.

In the 1980s, Cuba's Third World activism has been set back by the Soviet invasion of Afghanistan, by more assertive U.S. policies, and by a shifting balance of interests in NAM itself. Cuba now has a much lower profile, and Castro's ritualistic apology for the Soviet invasion of Afghanistan is a major stumbling block for Cuba's Third World entreaties. Erisman contends that Cuba's "Third World policy has tended to emphasize developmental and diplomatic assets" over military internationalism (p. 132), but this is highly questionable in light of escalating Cuban military involvement in Angola and Nicaragua. In El Salvador, the favorable trends Erisman detected in early 1984, namely, that Havana's "FMLN allies were in fact making major gains on the battlefield" (p. 142), have been reversed, and neither Havana nor the insurgents seems capable of regaining the initiative. Havana has not recovered from political setbacks in the Caribbean proper, and its credibility suffered due to its failure to rescue its Grenadian client in 1983.

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Erismán argues that Havana's warm relations with the Bishop regime were largely due to the fact that Washington spurned the regime, but this view is substantially inaccurate.

Given Washington's assertive posture in the Caribbean Basin and Soviet reluctance to bankroll the Sandinistas or substantially increase support for other revolutionary movements, Havana is expected to weigh future commitments in the region carefully, and avoid "taking major risks in the area as long as the Reaganites control the White House" (p. 186). Erismán is quite critical of U.S. policies towards Cuba and throughout the Caribbean, and somewhat indiscriminately lays much of the blame for the region's troubles on Washington. Still, he recognizes that Havana pays attention to strong signals, and tends to become more prudent in its actions.

The book successfully integrates the various strands that shape Cuban foreign policy, but the emphasis on nationalism overlooks the fusion between Cuba's foreign policy and Marxism-Leninism and its striking anti-U.S. character. In other words, the distinction between ideology and nationalism is somewhat artificial. In addition, much evidence suggests that Havana's nonalignment is dubious, that its willingness to serve as Moscow's junior partner severely limits its vaunted autonomy, and that its record of "successes" has come in partnership with some of the world's most repressive and illegitimate regimes, such as that in Ethiopia. Finally, Havana's challenges to Washington are legitimate exercises of its sovereignty, and are to be understood as expressions of calculated statecraft, not as manifestations of political altruism. The David vs. Goliath approach has many analytical flaws, and part of the argument of *Cuba's International Relations* rests on a less-than-convincing assumption regarding the *fidelistas'* infallibility.

JUAN M. DEL AGUILA

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Politics and Policy in the European Community. By Stephen George. (New York: Clarendon Press, Oxford University, 1985. Pp. xi + 197. \$24.95, cloth; \$10.95, paper.)

Discussion of the European Community on both sides of the Atlantic has tended to be dominated, in one way or another, by the ghosts of disappointed European federalists. Neofunctionalism, the conceptual orientation that has dominated thinking about the evolution of the European Community (EC), has itself been a kind of closet federalism. It has held that the fundamental interdependence within and among advanced technological societies would require the integration of ever wider circles of activity until eventual political integration became irresistible. Even those who have been pessimistic about federal solutions have often appeared to be "reluctant realists," who believe that some sort of federal integration would be rational and would save the future for Europe but that dark forces—mainly nationalism, fragmentation of parochial interests and short-sighted political opportunism—make such a movement toward union illusory. What has been common to nearly all those writing in this tradition has been an impatience with the slow, uneven and occasionally retrograde development of the European Community and a proclivity toward disillusionment.

Steven George has plunged into this troubled discussion with a remarkable and lucid analysis of the experience of the European movement in the light of neofunctionalist theory. Following the well-worn path of scholars and journalists who write about Europe, he poses the central question of his analysis: "Why have more common Community policies not emerged?" He attributes our expectations of more successes in creating common policy to the influence of the neofunctionalist view that there could be no turning back from integration once the logic of functional interdependence made itself felt. Since the history of the Community has proved resistant to that logic, George perceptively argues that a reevaluation and extension of neofunctionalism is needed, rather than a retreat into an empiricism without theoretical focus.

His analysis begins with an apt and admirably succinct summary of the development of

the European Community. In a later chapter, he traces the problems of neofunctionalist conceptualizations of the process and argues for a restructured theory with four main dimensions. First, he believes that the idea of functional (technical) "spillover" derived from the ideas of David Mitrany, Jean Monnet, and others seems to work and should be retained. Second, the concept of political spillover, added mainly by academic theorists like Ernst Haas and Leon Lindberg, does not square very well with experience, according to George. Thus, it should be amended to de-emphasize the importance of interest groups and the executive institutions of the European Community as agents of integration and, conversely, to stress the importance of nationalism and the domestic political pressures that constrain the Community policy of national governments. Third, the economic assumption of neofunctionalism—that growth is the normal condition of advanced economies and that the benefits of growth are evenly distributed over time—George argues must be abandoned in favor of an understanding of economic cycles and of their differential impact upon economies. Finally, neo-functionalism must come to grips with the powerful influence of the international political context upon Europe including the breakdown of the U.S.-dominated global economic system, the rise in the economic salience of the Third World and of Eastern Europe, and the changed balance of power between the U.S. and the European Community.

George's analysis of specific policies of the European Community occurs against the background of the analytical approach he has developed. He selects six major policy areas: energy, agriculture, economic and monetary union, regional policy, enlargement and external relations. In each case, he brings to bear a discussion of (a) domestic political forces, (b) economic changes and the uneven impact of economic stresses, (c) the economic interests structured by the world economy, and (d) the opportunities and constraints imposed by the shifting balance of power. In spite of a tendency to offer his interpretations as uncontested facts, George succeeds in making a strong case that the shortcomings of neofunctionalist explanation are largely overcome when domestic and international political and economic factors are systematically combined in the analysis.

The curious thing about this excellent study

is that, having pointed the way toward a restructured neofunctionalism, it does not actually return to the theoretical problem of accommodating the empirical dimensions it surveys. George's analysis appears to suggest the possibility of a "situational theory" of functionalism in which the operation of functional logic is located within a structure of key influences. But he does not develop the idea and may not even intend it. Instead, the focus is upon the impediments to integration imposed by conflicting national priorities. Occasionally international pressures are seen to force a common approach, as with the European Monetary System or the Mediterranean Enlargement, but the theoretical significance of such change is not developed nor is the total constellation of changes, so ably described in the study, assessed theoretically. The emphasis remains fixed upon the important difficulties that stand in the way of further progress. Perhaps the ghost of European federalism makes it difficult to find significance in anything except the uncompromising and rapid development of European Community policy.

An alternative glimpse of the same reality is suggested by scholars like Donald Puchala, Robert Keohane, and others, who have stressed the importance of converging expectations around a "regime" of values and procedures. Similarly, Robert Axelrod and other students of bargaining behavior have written about the durability of successful systems of cooperation. When applied to the European Community, such conceptual orientations emphasize the significance of the steady elaboration of institutional arrangements, the routine accumulation of mutual experience, and the anticipation of a shared future. Through such analytical prisms, the array of partial common policies in the European Community and the development of its institutional capacities look like more substantial accomplishments. At least, they suggest the possibility of a different balance in the evaluation of the European Community.

All this does not diminish the importance of this valuable analysis. It is a major contribution to the literature on the European Community and one likely to be read with delight because of the clarity, economy, and sharpness of organization with which it has been written.

WARREN L. MASON

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Banks, Petrodollars and Sovereign Debtors: Blood from a Stone? Edited by Penelope Hartland-Thunberg and Charles K. Ebinger. (Lexington, MA: Lexington Books, 1986. Pp. xii + 193. \$27.00.)

Synergy, a word of the 1970s, has gone out of fashion. This volume does nothing to rekindle the concept. Each of its seven essays has something to say, but together they are an ill-matched lot. The book consists of an introductory review essay, two suggestions for international economic reform, and four case studies of three countries' economic problems. The essays do not hang together, and few patterns and no theory emerge. In short, the essays are better than the sum of the parts. Even the title is misleading. "Petrodollars" are mentioned twice in the introductory essay, and then never again.

Economist Penelope Hartland-Thunberg starts with a concise review of the causes and the consequences of the world debt crisis. Debt problems are blamed mostly on the recession that followed the "totally avoidable" second oil price shock. The developing countries were devastated by higher prices for their oil imports, by sharply rising interest rates, and by sagging export income.

Before launching into the case studies, two old pros suggest ways to defuse the debt bomb. Nathaniel Samuels, an investment banker and former undersecretary of state, harks back to Keynes's original vision of a strong IMF and World Bank working together "to provide the leadership necessary for encouraging sustained and consistent economic and financial policies on the part of the member countries" (p. 37). Specifically, he urges that the best way to deal with the international debt crisis would be to authorize a new allocation of special drawing rights to provide new liquidity to debtor countries. He suggests that the dangers of reigniting inflation and undermining the resolve of debtor countries to take stern domestic steps are manageable. Next, long-time congressional staffer Jane D'Arista places the debt problem in a monetary context. She contends that the current monetary system is outmoded and inadequate. She argues that to escape the debt crisis, the monetary system needs to be revised to retain a reserve currency system while expanding the number of usable currencies. Both of these essays provide interesting sugges-

tions, but neither writer focuses on the problems of implementing their ideas.

The next three essays focus on Mexico and Brazil. First, Hartland-Thunberg and energy expert Charles K. Ebinger focus on Mexico's recent debt woes. Hartland-Thunberg then assesses Brazil's recent economic reversals and Ebinger contributes an analysis of the role of the energy sector in Brazil's economy. The first two of these essays provide thorough, blow-by-blow histories of the events leading to debt showdowns and the maneuvers and responses by the countries, their creditors, and the IMF. They provide excellent introductions to the unfolding dramas through early 1985, based almost entirely on reporting from the financial and economic press. No new data is provided, and there is minimal explicit comparison of the two cases. The essay on the Brazilian energy sector is informative, but only loosely tied to Brazil's debt problems. (Obviously, greater domestic energy production would reduce Brazilian dependence on imports and improve its debt situation.)

The final essay by Middle-Eastern expert Joyce R. Starr focuses on Israel's economic problems and what might be done to minimize them. An interesting analysis is provided that stands well on its own, but bears little relationship to the other essays. Israel's external debt situation is only one of many topics covered, and differs from the situation in Brazil and Mexico in that only about 25% of its external debt is held by commercial banks. Starr's essay and the book conclude with a famous Ben-Gurion punchline, "I always counted miracles in my calculations," and a retort that future economic miracles for Israel and presumably other debtor countries will have to be man-made. This is true, but we need better-integrated studies to solve these problems.

JONATHAN D. ARONSON

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Small States in Europe and Dependence. Edited by Otmar Holl. (Boulder, CO: Westview Press, 1985. Pp. iii + 341. \$25.00, paper.)

For some American political scientists it has become fashionable to view approaches to comparative politics and international rela-

tions that emphasize dependency matters as being "worn out," suspicious leftist carping, or methodologically soft analysis. For others, the issues of primary concern in the international political and political economy arenas have generally to do either with military/strategic issues or with the multifaceted interactions between the superpowers. To those who have not forsaken an interest in dependency questions or those for whom the world is larger than crisis-generated and powercentric analysis, *Small States in Europe and Dependence* (also referred to as the "Laxenburg Papers") edited by Otmar Holl are a needed breath, perhaps even a gust, of fresh air.

The papers that form the volume are revisions of those presented at a session of the Austrian Institute for International Affairs held at Laxenburg in 1981. In his introductory essay, Holl points out that big states and "big brother" analysts were not included, so that the focus of attention could be kept upon the issues of smaller states (European, "Second World" ones). Holl and his associates are definite in their opinion that the directions and subject matter of research in big states overlook both the existence of the problems of the small state political economy, and the important lessons to be learned by studying its dynamics. While excluding what are perceived to be the necessarily hegemonic foci of "First World" analysts does not in any sense mean that what the contributors have to say is correct, it does present us with an important challenge. Perhaps a bit of introspective reflection might just turn up the sort of biases these folks have asserted. Certainly, we ought not allow a mix of nationalistic pride and professional hubris to prevent us from thinking about things!

I appreciated this collection's concern with political economy issues. The numerous essays address the vitally important matter of the degree to which economic dependence influences the political options available to decision makers in small states. The overall conclusion is that the impact of dependency is considerable.

However, there is more here than material for students of the several smaller Western European states. The spirit of the essays is captured nicely by Otmar Holl and Helmut Kramer in their discussion of the process of internalization and Austrian policymaking.

First, they make the point that internalization is the absorbing condition of most contemporary political economy activities. Second, internationalization implies the dominance of the capitalist social mode. Finally, internationalization has rendered "international politics more dependent on economic processes while on the other hand politicizing international economic relations on a global scale" (p. 209).

For those who find the continuing description of difficulties and problems too hard to take, the volume is rich in prescriptions. For example, Benno Signitzer and Hans Heinz Fabris address themselves to various possibilities that exist as counterdependency strategies pertaining to the Austrian communications industry. Notwithstanding their pessimism about the necessary changes being implemented within their specific arena of concern, the authors represent the volume's uniformly tidy manner of blending the description of structural factors and purposive policy dilemmas.

The individual essays are written well and the arguments are developed into cogent statements. The reader will benefit from the volume's extensive documentation and the inclusion of considerable bibliographic materials. This volume is a valuable contribution to the literatures of both international relations and the politics of development. It is made the more valuable by the vantage points of its contributors.

CARL F. PINKELE

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World Futures: A Critical Analysis of Alternatives. By Barry B. Hughes. (Baltimore & London: The Johns Hopkins University Press, 1985. Pp. xii + 243. \$25.00, cloth; \$10.95, paper.)

Authors are not always responsible for the titles given their books. I have no idea who decided to call this book *World Futures: A Critical Analysis of Alternatives*, but the title, *World Futures*, has already been used for a similar, but generally superior, volume produced by the Science Policy Research Unit (SPRU) at the University of Sussex and published in the U.S. in 1978. Even so, the title did

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not prepare me for the actual contents of Hughes's book. Rather, as one might expect knowing the author's previous work, the book is primarily a summary and discussion of several very well-known reports based more or less directly on recent global modeling efforts: namely those commissioned by the Club of Rome—*The Limits to Growth*, *Mankind at the Turning Point*, and the Bariloche model—Wassily Leontief's *The Future of the World Economy*, the Organization for Economic Cooperation and Development's (OECD) *Facing the Future*, and Gerald Barney's *The Global 2000 Report to the President*. Some attention is also given to Lester Brown's *Building a Sustainable Society*, Alvin Toffler's *The Third Wave*, Herman Kahn's *The Next 200 Years*, and Julian Simon's *The Ultimate Resource*.

While the first six of these volumes may be a fairly good sample of books derived from attempts at computer-based global futures modeling in the West during the 1970s, I would be hard-pressed to say that, along with the remaining four volumes, they are representative of serious thinking about the future of the world by different groups and cultures around the globe—and that is what the title led me to expect. True, a few other sources are mentioned in passing, but even important existing critiques of global modeling, such as the SPRU work mentioned above and many others, are essentially ignored.

Some of the chapter headings are similarly misleading. The introduction, "The Proliferation of Future Studies," is in no way at all a discussion of the many examples of serious academic attempts at future studies that have emerged around the world over the past 30 years, or of basic or applied futures research. I can assure the reader that such efforts are found throughout Eastern as well as Western Europe and in many parts of Africa and the Middle East; that the subject is impressively well developed in many Asian countries; and that there are perhaps a dozen good academic programs and scores of reputable research and consulting groups focusing on global as well as other futures issues throughout North America. Almost none of these is mentioned, much less "critically analyzed," in this volume.

Similarly, chapter 2, "Competing Images of the Future," with its purported "Review of Recent Futures Studies," reviews only the ten

works listed above. The works reviewed are evaluated along two dimensions. One is labeled *political ecology* and comprises two categories: modernist and neotraditionalist. The other is *political economy* and has three categories: liberal, internationalist, and radical. When these are arrayed orthogonally against each other (and modernist and neotraditionalist are relabeled "no limits" and "limits," respectively), the studies reviewed fall within the resulting Cartesian space. For example, the Bariloche model thus lies in the "radical/no limits" sector, and Lester Brown in the "liberal/limits" sector.

The seven chapters that comprise the bulk of the book are derived from the "components of the global development system" of *Mankind at the Turning Point*: population, economics, energy, food, technology, the environment, and values and political structures. In turn, each summarizes the methods and substantive findings of the various global models relative to that component. They are as well done as the data they are based upon permit, but have a dry textbook flavor that makes for rather uninteresting reading for a person looking for information about "world futures."

The final chapter, "What Are We To Think?" is a bigger letdown. Only four and a half pages long, it does not go far in answering its question, which presumably refers not to thinking in general, but to what we are to think about the future of the world. Yet how could it do more, when the author ignores most of the futures literature and focuses on such a narrow, if admittedly important, part? What *are* we to think, indeed?

JIM DATOR

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Autonomy and Interdependence: U.S.-Western European Monetary and Trade Relations, 1958-1984. By Thomas L. Ilgen. (Totowa, NJ: Rowman and Allanheld, Publishers, 1985. Pp. x + 166. \$34.95.)

This book provides a sweeping historical interpretation of postwar trade and monetary relations between the United States and Western Europe. Thomas L. Ilgen offers an often-compelling interpretation of the paradox presented by America's simultaneous pursuit of

interdependence and autonomy. Immediately after World War II, the United States used its power to develop liberal trade and monetary regimes intended to foster economic interdependence. Yet it also wanted autonomy in domestic economic management. When autonomy and interdependence conflicted, the U.S. acted to change trade and monetary regimes in order to insure the maintenance of American autonomy. As a result, America's relative economic power declined, and an international political economy founded on economic interdependence is replacing one based on hegemonic power.

Ilgen presents a great deal of historical detail in support of his thesis, and this well-written book includes discussions of domestic developments in Britain, France, Germany, and the United States. He describes a wide array of international trade and monetary negotiations, and offers powerful discrete observations throughout.

The book is also theoretically eclectic. It draws from the literature on hegemonic stability, from arguments about the international division of labor, and from arguments in comparative politics about the importance of domestic economic arrangements and institutional relationships. Ilgen synthesizes existing work to develop an important and compelling historical thesis.

Ilgen presumes that autonomy and interdependence are incompatible objectives, and that the American struggle to maintain autonomy in the face of interdependence has worked to its economic disadvantage. Yet he does not define autonomy precisely, and in none of the senses in which he uses the term does autonomy necessarily conflict with interdependence. At times, Ilgen equates autonomy with protectionism. Elsewhere, he uses it to describe America's disregard of other nations, and in still other places it seems to mean the U.S. pursuit of domestic macroeconomic policies without concern for balance of payments considerations. However, autonomy can be pursued either to prevent the workings of interdependence or in full recognition of it. The protection of textiles in the early 1960s, for example, can be interpreted as the pursuit of autonomy in the face of interdependence, or as the domestic price for the further liberalization of trade and the growth of interdependence. Moreover, autonomous policies can be used

either to improve American competitiveness or to shelter industries from structural adjustment.

The problem with portraying autonomy and interdependence as incompatible and with viewing the pursuit of autonomy as harmful to the United States is evident in Ilgen's analysis of the last decade. The Carter administration self-consciously embraced coordination and interdependence rather than autonomy, but the failure to achieve consensus on coordinated macroeconomic stimulation resulted in an unintended autonomy for the United States. Ilgen sees the Reagan administration, on the other hand, as returning to earlier attempts to maintain autonomy—as ignoring international consequences and concentrating solely on domestic ones. Yet it has not attempted in the name of autonomy to shelter domestic industries from structural adjustment, as had earlier administrations. Its insistence on market solutions has led it to reject both coordination and most protection. The incompatibility of autonomy and interdependence (and the presumptive need to coordinate in an interdependent world) depend on the nature of autonomous policies; autonomous policies can improve adjustment and competitiveness, just as coordination can fail.

The strength of this book is its interweaving of domestic and international economic policy and of monetary and trade policy. The historical interpretation is provocative and often compelling.

ARTHUR A. STEIN

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Interdependence on Trial: Studies in the Theory and Reality of Contemporary Interdependence. Edited by R. J. Barry Jones and Peter Willetts. (New York: St. Martin's Press, 1985. Pp. x + 237. \$29.95.)

I first heard the term *interdependence* as a child in a Catholic elementary school in the 1950s. Following daily Mass we would pray with the priest for the conversion of Russia, and then go off with the nuns to have the effects of our prayers for U.S. hegemony undermined by indoctrination in the concepts of interconnectedness and brotherly love. Dur-

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ing those years of American confidence in the power of our dollars and our weapons, the Franciscan sisters were out of step with the "realism" of Cold War Catholics and their ideological brethren who dominated the way most of us viewed the international system.

As Jones and Willetts tell us in the introductory chapter of their provocative collection of essays on interdependence, events in the late 1960s and early 1970s undermined the confidence of both lay and official observers in the realist paradigm. It gave way to a world view that focused on the limits to power and the proliferation of entangling ties that prevented its rational exercise in increasingly complex and even internally contradictory national interests: "Interdependence, in this vein, may thus be viewed as a rationalization of failure in Vietnam and the enforced abandonment of dollar convertibility into gold" (p. 2). As realism makes its comeback on the coattails of the elections of 1980 and 1984, it is easier to see the flaws in both perspectives when they are used reflexively, as blunt instruments of foreign policy making and analysis.

The first four essays in this collection demonstrate very nicely that interdependence is far from being a single concept. A major analytical problem is the lack of agreement about whether interdependence is a property of states or of systems, a problem that has bedeviled those who try to measure interdependence and its changing levels over time. Like most of the measurers, Jones, in his essay, settles for interconnectedness as at least an indicator of interdependence among states, but along with Willetts and Steve Smith in their contributions, he also recognizes the empirical and perceptual complexity of the concept to both the people who study international relations and those who engage in them. Other ways to understand interdependence are also discussed, such as the equation of interdependence with a perception of a common fate, the rationale for oligopoly behavior among firms, and deterrence behavior between superpowers. The latter model informs Little's argument that realists believe in interdependence too.

The link between situation and ideology and the choice and application of an analytical framework is clearly drawn throughout the volume. It is treated theoretically in the essays already mentioned, and empirically in John

Vogler's chapter on the World Administrative Radio Conference and in John MacLean's chapter on UNESCO. Michael Smith's essay on Atlanticism and interdependence in the North Atlantic is an elegant analysis of the sometimes tenuous connection between empirical reality and perceptions of system structure. All these essays emphasize the role of subjective factors in determining the content and interpretation of analytical models.

An element that I believe is insufficiently explored in this collection as a whole is the difference between the organizing principles of interdependence and hierarchical frameworks such as realism and dependency. Even though interdependence in its ideological context has been used by powerful countries to justify cooperation by less powerful actors to uphold regimes that benefit the powerful (see the arguments made by Vogler and MacLean), the quality of the relationship as viewed through the interdependence model is different than it would appear from a realist perspective. Interdependence takes sovereignty, rather than hierarchy, as its organizing principle. Bargaining rather than force is seen as the legitimate means of resolving conflicts. This is why the interdependence model can explain empirical situations such as international relations during periods of declining hegemony, or where participants perceive themselves as sharing a common fate, better than realism. If interdependence is "on trial," it is because it can't explain why bargaining breaks down or is bypassed altogether in favor of the application of force. The priests of power and the sisters of cooperation are still unable to reconcile their divergent views into a comprehensive model of how the world works.

MARY ANN TETREAULT

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European Peace Movements and the Future of the Western Alliance. Edited by Walter Laqueur and Robert Hunter. (New Brunswick, NJ and Oxford: Transaction Books, 1985. Pp. xxi + 450. \$45.96.)

West European Pacifism and the Strategy for Peace. Edited by Peter van den Dungen. (New York: St. Martin's Press, 1985. Pp. viii + 218. \$29.95.)

History has demonstrated time and again that no alliance is eternal. The political, economic, and military environment that gave birth to NATO in 1949 has undergone a transformation so dramatic that elites and masses alike have begun to question the alliance's prospects for survival, at least in its present form. Mass protests against the deployment of Pershing II and cruise missiles in the early 1980s may have highlighted the need for a reevaluation of prevailing alliance strategies, but the erosion of consensus among American and European security partners commenced in the late 1960s and early 1970s. These years were marked by a declining confidence in the United States' capability and/or willingness to protect the Continent at the expense of its own survival, as well as by changing perceptions of the Soviet threat, based on different experiences with détente.

A 35-year emphasis on the military dimensions of the Atlantic Alliance has precluded a search for a new consensus grounded in common political purposes that would serve to rejustify its existence and lend coherence to its strategies. The search that lies ahead is fraught with complications unknown to defense planners of earlier eras, owing to the growth of an attractive, activist, antinuclear public, as well as to a reassertion of national interests among European states. In the long run, attempts to reform NATO or to replace it with an alternative security system will succeed only if three conditions are met: (1) policy makers structure their defense systems around a positive, operational definition of peace that addresses the sources of global and East-West conflict; (2) European and American partners find a concept of security that ensures an equitable distribution of costs, guarantees, and risks among all members; and (3) leaders in Western democracies undertake steps to inform, reintegrate, listen to, and learn from the successor generations constituting a major segment of

the critical, mobilized, protesting public. Direct dialogue among the political establishment, defense strategists, peace researchers, and a public terrified by possible nuclear annihilation is long overdue.

These two volumes appear to offer an ideal forum for such a dialogue, but they fail, for the most part, to deliver the syntheses promised in their titles. In *European Peace Movements and the Future of the Western Alliance*, editors Laqueur and Hunter argue that Western Europe has sufficient manpower and resources, but not the political-psychological will to provide for its own defense. The link between this missing will and peace movements is not as "obvious" as they assume, however. Hyland speaks to European leaders' unwillingness to provoke an open nuclear debate, but sees strategy as nonetheless having been abdicated in favor of politics, and politics as sacrificed to public relations. Kristol's (reprinted) critique of the nonexistent nuclear umbrella and untestability of a graduated deterrence strategy ends with the warning that U.S.-nationalistic unilateralism is likely to wreck the alliance. Kissinger's (reprinted) plan for reshaping NATO stresses that an alliance incapable of agreeing on fundamental political premises cannot survive by adhering obliviously to military arrangements adopted under radically different circumstances. He recommends a European planning and command role. Draper (reprint) holds that what is new about the current crisis besetting the alliance is the questionable reliability of the West Europeans now being called upon to aid the U.S. in pursuing its own security needs. Hassner sees a potential German neutro-nationalist lurking behind every peace banner, willing to risk the division of the alliance to advance its own rapprochement cause.

Portrayers of the individual peace movements—Haagerup on the Netherlands, Ceadel on British nuclear disarmers, Gnesotto on the French front, Norman on the pacifist contributions of British churches, von Kielmansegg on the origins of the German movement—offer few new insights against the backdrop of the many campaign "autobiographies" already in print. Altling von Geusau's polemical, "Communist-plot" assault on the Dutch and Belgian movements, along with de Maizière's facile dismissal of German pacifists' efforts at least to introduce alternative security concepts

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detracts significantly from the quality of the debate. Herf's acerbic essay equating neutralist aspirations with moral confusion and fractured liberal democracy in the Federal Republic of Germany (FRG) attends more seriously to the sources of protest than does Thomas's characterization of the American freeze campaign as naive, emotional, and Orwellian. Neither the more interesting propositions advanced by Scharrer on the search for new ethical standards and political activism among the German churches, nor the more concrete proposals for restructuring NATO covered at the book's outset, receive further notice—there is no critical-analytical concluding chapter.

West European Pacifism and the Strategy for Peace, on the surface, admits to an analytical distinction between the idea of pacifism, peace movements directed against specific security policies, and integrated strategies for peace. Andreski relates the search for peace to differing conceptions of human nature, and thus to different explanations for the origins of war, rejecting the idea of a killer instinct among humankind. Editor van den Dungen identifies the theoretical origins of pacifism, along with various schools and types ranging from heroic and Christian to imperialist and cultural pacifism. Comments by Shtromas quickly degenerate into an anti-Communist tirade. He equates unilateral disarmament steps with unconditional surrender, claiming that "the West's surrender to the Soviets is even more lethal than nuclear war itself" (p. 40).

The anti-red histrionics gather momentum in subsequent chapters. Unlike the essayists in the Laqueur-Hunter volume, however, these authors cite little evidence of ever having read the works of those they denounce as ideologically obdurate, "normal machiavellians" (p. 61). Clesse charges the peace movements with trying to impose neutralization through the "language of the streets" and media manipulation. Radnitzky employs a sociobiological framework for judging whether movement supporters themselves—said to prefer moralistic aggression and spiritual self-mutilation to rational discussion—constitute "pitfalls for peace." Shtromas returns to assess peace politics in the Soviet Union, where the population gives its "dissenting assent" to false promises and pseudo-initiatives in exchange for a minimum of physical security. Harmel posits that Communists use peace/pacifism to their own

advantage, but also use war when necessary to advance the revolution. In addition to expanding their own propagandistic organizations, they exploit and infiltrate any and all spontaneous movements that "bleat for peace" (p. 168). Gallois writes of asymmetries threatening Western security, including the "often absurd intervention of the public" (p. 175), and urban vulnerability in advanced industrial Europe.

At least three voices of reason emerge. Tromp stresses the importance of the "paradigm shift" in security thinking that has been brought on by the paradox of deterrence: mutual assured destruction (MAD) threatens such a horrible global end that no rational actor would resort to it, thus undermining the credibility of the threat. The key to effective security policy rests in the recognition of interdependence among all states. Von Baudessin holds this to have been the aim of détente; political cooperation and binding crisis management procedures should take precedence over military capability. Morton Kaplan outlines concrete steps for reducing the risk of war through conventional "transarmament," nuclear-armed Quick Reaction forces, "dissuasion" tactics applied to the East, and parallel command rights for each ally. Klein judges this model to be more satisfactory than Mutual and Balanced Force Reduction (MBFR) proposals made thus far in Vienna and less radical than Reagan's "zero option," yet sees disarmament as but one component of a general peace policy. Von Baudessin objects to the prescription for turning "flexible response" into "flexible responsibility."

The "extraordinary array of talent" (book-jacket claim) ostensibly includes one woman among the 26 authors represented in these two books, and no activist peace researchers. The "surprising consensus" that emerges is therefore not so surprising to this reader. In matters of war and peace, "it is political power that counts; in politics power dominates arguments or rationality" (p. 69). Despite their success in raising arguments critical to the survival of the species and in posing questions as to the rationality of deterrence, the peace movements are a long way from threatening or transcending the power-political structure of the Atlantic Alliance.

JOYCE MARIE MUSHABEN

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Superpower Involvement in the Middle East: Dynamics of Foreign Policy. Edited by Paul Marantz and Blema S. Steinberg. (Boulder, CO and London: Westview Press, Inc., 1985. Pp. ix + 301. \$21.50, paper.)

This volume is divided almost equally between Soviet and United States policy in the Middle East. The first three chapters deal with the recent evolution of Soviet perspectives on the utility of force in the Third World (S. N. MacFarlane), Islam as a political force in the Middle East (Carol R. Saivetz), and U.S. goals in the region (Herbert L. Sawyer). These are followed by two case studies of Soviet policy: one on the Lebanese crisis of 1982-84 (Robert O. Freedman), and one on the apparent Soviet decision of 1972-73 to aid and abet Sadat's decision to go to war (Dina Rome Spechler).

Thereafter come four chapters on U.S. policy and perspectives: one on the role of oil as a factor in U.S. calculations (David Haglund); one on the assumptions of State Department Arabists about the character of the PLO (Martin Indyk); one on general U.S. policy assumptions about the nature of society and conflict in the Middle East and Persian Gulf (James R. Kurth); and one chapter that attempts a general evaluation of the Reagan administration's Middle East policy during its first term (John H. Sigler). The final section of the book offers an analytic framework for assessing the impact of the U.S. and USSR on the historical evolution of the Arab-Israeli conflict (Shmuel Sandler). Finally, a concluding chapter briefly summarizes the theme of each contribution to the volume (Blema S. Steinberg).

The virtue of the volume is that it brings together an impressive array of talent whom it allows to present research findings (more typical of the Sovietological chapters), or to make evaluative statements (more typical of the chapters on the U.S.) about the aspect of superpower involvement in the Middle East in which they are most interested. The drawback of the volume is that it has a topic, but no theme. The contributors were apparently not given a common problem to address or puzzle to solve. Consequently, the volume lacks coherence; not surprisingly, the final chapter reads more like a preface than a conclusion.

Some questions that could have been addressed by almost all contributors are the

following: What would be the political feasibility in Moscow and Washington of joint action to deescalate the Arab-Israeli confrontation? Could Moscow and Washington have controlled their clients even if they had agreed on joint action? Given the turmoil in the region, to what extent is the superpowers' competitive relationship a zero-sum game? To what extent has it turned into a negative-sum game? To what extent would superpower collaboration be a positive-sum game? How have perceptions and perspectives on these matters evolved over the years in each capital? Each chapter in the book, however partially and indirectly, has something to say about some of these matters. As presented, however, there is nothing explicitly additive about the contributions.

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Guests Come to Stay: The Effects of European Labor Migration on Sending and Receiving Countries. Edited by Rosemarie Rogers. (Boulder, CO: Westview Press, 1985. Pp. xiii + 344. \$25.50.)

The Unwanted: European Refugees in the Twentieth Century. By Michael R. Marrus. (New York: Oxford University Press, 1985. Pp. xii + 414. \$24.95.)

Guests Come to Stay is a carefully organized and skillfully edited volume addressing an issue that has received remarkably meager scholarly attention in the United States: the political, social, and cultural challenges spawned by Europe's experiment with temporary worker programs. The volume is ambitious, in that it attempts to be comprehensive by offering both a retrospective and prospective look at the problem, and by incorporating a discussion of an often-ignored and always controversial component of the issue: the impact of emigration on the sending societies. Yet, such laudable goals notwithstanding, *Guests Come to Stay* delivers only meager results. In spite of a commendable introduction by the editor, in which she sets out very clearly the multitude of issues and tensions to which "guestworkers" give rise, there is no apparent attempt to tie the many presentations

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together either theoretically or substantively. As a result, the volume stands naked as a set of mostly descriptive essays that cover a number of key issues associated with European temporary labor programs, but also allow other issues of equal or greater importance to go unattended.

An example might suffice to demonstrate this point. The volume highlights one of the key challenges to receiving societies posed by the institution of foreign workers: the second generation problem. The discussion of the problem, however, is largely mechanical and often seems to get its cue more from the receiving governments' preferred perspective of "damage control" than from a genuine analytical interest in the challenges and opportunities that second-generation migrants pose for key receiving societies. That only one of the contributors to the volume, Hans-Joachim Hoffmann-Nowotny, writes of "immigrants" rather than "migrants" or "foreigners," and that none of the contributors speak of second-generation Germans, Swedes, or Swiss, highlights both the short-sightedness of Europe's policy responses to its de facto immigration situation and the lack of analytical foresight and initiative on the part of most authors. With this serious problem in mind, one must wonder if this volume is, analytically, a second-generation effort in the understanding of the phenomenon of foreign labor in Europe, or simply an updated first-generation version following in the footsteps of earlier mediocre works on the topic.

While the essays by Friedrich Heckman and Tomas Hamar attempt to go beyond the traditional questions of the literature on the topic, the lack of an organizing principle or a *mise en scène* weakens the volume even further. So do a number of other more minor but equally puzzling problems: the editor's unquestioning adoption of an explanatory framework ignoring the substantial structural forces for emigration in favor of an approach that views the decision to emigrate as "voluntary"; a drawn-out discussion of the permanence/temporiness question that ignores the view of virtually all immigrants, and even many refugees, that their emigration is essentially temporary; the positing of Austria, rather than France, as the archetype of a country that allowed foreigners to seek and obtain work and then apply for work permits; and a far too sanguine view of

the degree to which "administratively enforced departures" were adhered to and became a disruptive force in foreign worker communities in the 1970s—particularly in the Federal Republic of Germany.

These problems notwithstanding, *Guests Come to Stay* is a worthwhile and relatively up-to-date account of the status of foreign workers in Europe. It contains generally competent analyses both of the situation of foreign migrants in advanced industrial Europe and of the consequences of emigration in the sending countries.

While the Rogers volume virtually ignores the entire question of refugees in Europe, Michael Marrus's *The Unwanted* focuses explicitly on that type of population movement. Marrus brings a historian's training and interests to the topic, and delves deeply into the causes of refugee generation and the responses refugees received where they resettled. *The Unwanted* tells a tale of genocide, war, mass expulsions, exchange of populations, prejudice and human suffering, dislocation for those seeking refuge and those offering it (often with great reluctance), marginality, and official ambivalence and confusion about refugees. It tells of the persistent tension between politics and humanitarianism and the frequent surrender of the latter to the former, of nation building and of the cruel calculus that accompanies radical systemic change.

Remarkably, although the volume focuses on the first half of the twentieth century, little has changed either in the urgency of the problem and the consequent need for safe havens, or in the ambivalence of the reactions of governments and people toward refugees. The birth of the predecessor to the current United Nations High Commission for Refugees (UNCHR), the League of Nations High Commission for Refugees, is well documented by Marrus, as are the financial and political limitations of the office and the commitment of the first commissioner, Norwegian Fridtjof Nansen.

There is a veritable thesaurus of information and historical insights in this volume. It covers not only the responses to and the reception of refugees by the destination governments, but also the antecedent and proximate causes of refugee flows as disparate as the many Jewish exoduses since the 1880s, the Armenian genocide, and the Greek catastrophe in Asia Minor,

with its consequent mass exodus of ethnic Greeks from Turkey and the Greek-Turkish exchange of populations.

Throughout this fascinating book of government excesses and human suffering, Marrus manages to offer an even-handed, superbly documented, and clearly written analysis of each episode, while simultaneously unraveling the web of another story: the evolution of the international procedures and institutions that would act as occasional buffers, but more frequently as impartial but concerned middlemen, in refugee-generating crises. In this parallel tale one finds the best and the worst about international initiatives—the meeting of the basic survival needs for most refugees and the financial and political constraints of transnational institutions, as expressed in their inability to prevent, defuse, or ameliorate situations that create refugees. Yet, in view of the considerable successes of both the League and the United Nations High Commission for Refugees in assisting with the moving and relocation of perhaps as many as one hundred million refugees, one must be both humbled by the enormity of the task and thankful for the response of the international community in establishing and continuing the institution of the UNHCR, in spite of the extreme political sensitivity of its mission.

These two volumes are somewhat uneven in quality and are intended for different audiences, but they are quite similar in the intensity of their messages. Both deserve the attention of serious students of twentieth-century Europe. Both address important problem areas. Finally, both are eloquent advocates of the need for European states to develop systematic (preferably joint) initiatives to address continuing challenges, namely, coming to terms with their own unmistakable ethnic pluralism (the Rogers volume) and the persisting need to anticipate, and thus plan for, the orderly resettlement of refugees (the Marrus volume).

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New Nationalisms of the Developed West.

Edited by Edward A. Tiryakian and Ronald Rogowski. (Boston: Allen & Unwin, 1985. Pp. xii + 394. \$35.00.)

The avowed purpose of this volume is to address the "anomalous phenomenon, that nationalist movements seeking radical decentralization or emancipation from state authority arise at an advanced state of modernization" (p. 4). The theoretical foundation for the anomaly is the paradigmatic expectation that modernization and mass participation in industrial democracies would tend to eliminate ascriptive solidarities as the basis of political cleavage in favor of class or interest-group activity. The empirical basis for the anomaly is the apparent rise in "ethnonational" political activity in "advanced industrial states" during the late 1960s and the 1970s.

Indeed, it was in the late 1970s that the group whose work is represented in this book was formed. Most of the contributions are based on work that was done at the beginning of the 1980s. It would appear that by that time the nature of the "anomaly" had changed. Most of the 16 individual contributions, as well as the concluding chapter by Ronald Rogowski, are devoted to explaining why ethnonationalist movements in the developed world—especially in Britain, Canada, Spain, and France—have experienced decreasing success, and/or why theories developed to explain their emergence and growth were flawed.

Rogowski suggests a general theoretical approach to nationalist movements—he prefers not to distinguish "new" from "old" nationalism—couched entirely within a rational choice perspective. As Rogowski emphasizes again in his concluding remarks, he sees the need to reject "primordialist" as well as reactive explanations of ethnonationalism in favor of a broader approach to the study of "political cleavages" in terms of changing incentives to adopt alternative political identities.

The judgments implicit in this line of analysis—that identities are plastic and self-selected and that they are usefully considered as instruments employed by individuals to accomplish larger social and economic ends—are common to many of the essays in this volume, including both of Michael Hechter's contributions. Hechter retreats even further

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than he has previously from his "internal colonialism" explanation of contemporary Celtic nationalism. Adopting an Olsonian rational choice perspective, he explains the declining fortunes of Scottish and, with Margaret Levi, Welsh nationalist parties by identifying shifting selective benefits, available to Scotland and Wales from participation in the British state and to individual Scotsmen and Welshmen for supporting non-ethnonationalist parties. Jack Brand's response to Hechter emphasizes the incorrectness of the latter's original predictions about Scottish nationalism, and also takes issue with his amended version as advanced in this volume. According to Brand, Scotsmen have had even less reason to vote nationalist than Hechter realizes, since the Scottish National Party has had no patronage to offer its followers.

Mario Polèse evaluates the variables that determine the economic rationality of ethno-regional separatism. His argument, illustrated in the Canadian context, is that on balance, separation is probably more likely to be costly than profitable for peripheral regions, but that too much indeterminacy exists in the trade-offs that must be made along different economic dimensions for a strictly economic analysis to be conclusive. Polèse's analysis is consistent with that of Oriol Pi-Sunyer, whose theoretically and historically balanced contribution on Catalan nationalism in Spain refers to the protectionist policies that central states can adopt, affecting the incentive structures of inhabitants of peripheral regions and thus reducing the appeal of ethnonational or ethno-regionalist parties.

Jürg Steiner's discussion of modes of decision making in Switzerland with respect to separatist sentiments in the Jura also draws attention to the strategies and techniques available to central states in their efforts to keep peripheral regions with separatist tendencies within the ambit of central state authority. Phillip Rawkins's analysis of the cruel dilemma facing Welsh nationalists, as they seek minimal electoral success at the expense of purity in the appeals the party makes to nationalist values and ambitions, substantiates the general point that the political mobilization of ethnonationalist sentiment is in large part a function of central incentive structures controlled by the state.

The longest and the best contribution is Juan

Linz's study of Basque, Catalan, and Galician political sentiments. Linz uses carefully gathered attitudinal survey data to show that ethnonational political movements, as well as individuals, adjust their thinking to changing incentive structures. His argument, largely corroborated by his data, is that as ethnonationalist movements seek more radical, political, and separatist objectives, their appeals shift from primordialist notions of what the salient identity is to regionalist, nonascriptive ideas. Only in that fashion, in relatively developed "peripheral" regions such as the Spanish Basque country and Catalonia, can the sense of exploitation felt by so many of the new immigrant workers be tapped in order to achieve political or electoral success. He shows that definitions of identity as determined by ancestry or language—as opposed to beliefs and/or place of residence—are more likely to be associated with loyalty to the central state than to an ethnoregionalist political movement.

The book is marred by numerous typographical errors. Several of the contributions are thin, and read more as impressionistic reports of research in progress than as carefully considered contributions to the theoretical and empirical problems at hand. Nonetheless, the volume as a whole is valuable and stimulating. That the answers provided to the original organizing question tend to refute its premises reflects substantial intellectual development in the political analysis of nationalism, new and old.

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Leaders Under Stress: A Psychophysiological Analysis of International Crises. By Thomas C. Wiegale, Gordon Hilton, Kent Layne Oots, and Susan V. Kiesel. (Durham, NC: Duke University Press, 1985. Pp. xvi + 228. \$32.50.)

Leaders Under Stress adds a new dimension to the study of international crisis behavior. It focuses on the state of mind of leaders during crises. However, instead of using the traditional method of content analysis of the written word, it uses what the authors term "signal leakage" in the words spoken by the president

during the crisis. This is done by taking three international crises—the 1961 Berlin crisis, the 1965 Dominican crisis, and the 1970 Cambodian crisis—and subjecting all presidential speeches and press conference remarks pertaining to them to voice stress analysis. This data is then analyzed for thematic stress and psycholinguistic structure.

The authors come up with some interesting results. Concerning the Berlin crisis, they discovered that the theme of reservists was more stressful to Kennedy than the themes of freedom for Berlin or of the need for a military buildup. They also report that, due to a lack of severe stress themes in remarks made after November 29, the "cognitive saliency of the crisis for Kennedy had declined significantly" after that date (p. 97).

The authors found two surprising results in their analysis of the Dominican crisis. First, they found a lack of any severe stress themes in any of Johnson's remarks during the early stages of the crisis, April 27–30. It is only with the May 2 speech, after the original wave of American troops had landed and additional troops moved in, that themes begin to register at the extreme stress level. Furthermore, the authors found that the most stressful theme during the crisis was neither concern over a Communist takeover nor concern over the introduction of American troops, but rather concern for U.S. civilians.

The analysis of the Cambodian crisis offers additional surprises. The most stressful themes were the U.S. relationship to Cambodia and the South Vietnamese role in the operation. The most surprising finding was the lack of stress associated with the theme of domestic turmoil—i.e., Kent State and its aftermath.

Although the book is, in my opinion, well worth reading, it does have some major flaws. The authors maintain that they have developed a psycholinguistic profile for the three presidents during international crises. Therefore, they argue, "if analysts find deviations from one or more of these characteristics, they should be alerted to the possibility that some type of psychological dysfunction has probably taken or is taking place" (p. 165). How can one construct a "normal" profile on the basis of one case? Additional data from other crises during the same administration are needed to make a secure claim of a valid psycholinguistic profile.

This leads to a related point. If inferences are to be made on the basis of changes in psycholinguistic posture, one must be able to ascertain what the "normal"—i.e., noncrisis—psycholinguistic pattern is for each president. Thus another major flaw in the book is the lack of a control group of noncrisis data. The authors explain the lack of such data by stating that "noncrisis video and audio tapes are made infrequently, and they are not kept for long" (p. 18). Having done research at both the Kennedy and Johnson libraries, I know this statement to be inaccurate. A quick call to the two presidential libraries confirmed the existence of audio tapes of presidential press conferences and such noncrisis speeches as State of the Union addresses that could have been used as a control group. Although similar data on Nixon would have been more difficult to gain access to, a call to the CBS News archives confirmed that such tapes on Nixon are available there.

Another flaw in the book is in its interpretation of data. At one point, the authors attempt to explain the differing patterns of fluctuation in levels of stress over time. Their explanation of why the pattern in the Dominican crisis is different from the other two is that the other two were "embedded" crises, meaning they were embedded in a larger issue. The Berlin crisis was embedded in the Cold War and Cambodia was embedded in Vietnam. In contrast, according to the authors, "the Dominican crisis is a discrete event for President Johnson" (p. 140). This explanation seems to ignore that it was exactly during the Dominican crisis period that what many analysts have called the major decisions on the escalation of the Vietnam war were made. Therefore, anyone looking at that period as a whole would be hard-pressed to argue that the two events could be psychologically separated in Johnson's mind.

Finally, the issue of the extent to which voice stress analysis is useful in explaining crisis behavior must be discussed. The authors are the first to admit that voice stress analysis has its limitations, the greatest of which is that the stress recorded by the machine can be caused by "deception, uncertainty, centrality to desires or anticipation" (p. 12). Thus, voice stress analysis tells us what themes, if any, registered psychological discomfort. It does not and cannot tell us what caused the discomfort.

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For example, one of the major findings in the book is the high stress associated with the theme of protection of U.S. civilians during the Dominican crisis. The authors explain the cause of this stress in terms of Johnson as "a leader whose major political interests were in the domestic realm" (p. 114). However, it is equally plausible to argue that the stress was caused by the centrality of the theme to Johnson's paternalism, or that it was caused by an attempt to convince his audience that concern for the safety of U.S. citizens in the Dominican Republic was indeed a major reason for his decision to intervene. A third alternative explanation is that the stress was caused by Johnson's attempt to deceive his audience as to the importance of the issue. The point is that voice stress analysis cannot, on its own, explain stress, and thus must be used in conjunction with other methods, like research on presidential belief systems, in order to explain crisis behavior adequately. To their credit, the authors readily admit this.

When judging this book, one must keep in mind that explaining foreign policy crisis behavior is like putting together a 1000 piece jigsaw puzzle. Wiegale and associates have given us a new and tantalizing piece to be fitted into the puzzle. This book does not explain international crisis behavior. It does, however, give us an additional tool with which to explain crisis behavior. On that basis, it is well worth reading.

J. PHILIPP ROSENBERG

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Ripe for Resolution: Conflict and Intervention in Africa. By I. William Zartman. (New York: Oxford University Press, for the Council on Foreign Relations, 1985. Pp. x + 260. \$24.95.)

Twenty-five years after the "year of Africa"—the proliferation of new states in 1960—the most marginalized and divided of all the continents is once again the focus of international attentions. The "discovery" of Ethiopia's drought and South Africa's struggle by Doonesbury, Live Aid, and Artists United

Against Apartheid is evidence of the unlikelihood of foreign actors ever getting "Out of Africa" for very long. Contrarily, just when the new international division of labour has diminished external economic interests in Africa, the new Cold War has revived extraneous strategic issues. William Zartman's comprehensive and catholic case studies of the latter largely exclude the former. This magisterial mix of his two fortes—negotiation and Africa—come together well in this major study, which is both pragmatic and powerful. Yet Zartman's aversion to political economy lends a certain unreality and superficiality: Africa's developmental disasters, from droughts to the encroaching of the desert to indebtedness, hardly intrude as the backdrop to the dramas of diplomacy and strategy. The continent will be forever ripe for resolution so long as it remains starved of development.

Zartman's informed investigation of interstate discussions and coalitions is thus diminished in its salience and relevance by its failure to treat the economics as well as politics of underdevelopment: What are the social as well as strategic bases of conflict? Can protracted negotiation change patterns of inequality either between or within states? Would diplomatic interventions be less necessary if economic expansion had not been so short-lived in the first flush of independence?

Zartman's purpose in this always stylish and sometimes humorous monograph (e.g., "The more it develops, the more Zaire—now a vacuum of attraction—will become a major pole of attraction and reaction in central African relations" [p. 126]) is to describe, compare and evaluate patterns of African and American involvement in four crucial and continuing regional conflicts: the Sahara, the Horn, Shaba, and Namibia. His historical, analytical, and linguistic skills are best displayed in the discussion of the Sahara, but all four are convincing, comprehensive, and cohesive; the author's well-established Africanist competence is employed to good effect.

The shorter introduction and explication/prescription are neither as sustained nor as satisfactory, despite being based on Zartman's well-regarded negotiation investigations. Instead, early definitions of and distinctions among crises and conflicts (pp. 8-15)—how local?—are compounded in the end by belated

typologies of patterns, dimensions, and phases (pp. 228–38). The juxtaposition of African and American interests is sustained throughout, yet the reconciliation of “Africanist” and “globalist” forces in successive U.S. regimes cannot be so readily reconciled: “America needs—and Africa deserves—a policy that recognizes the importance of Africa to the United States within the context of its global obligations” (p. 244). The logic of super- and great-power antagonisms compounds intra- and extra-African differences in complex and changeable coalitions—*renversement des alliances* (pp. 30 and 241)—and America’s greater “interest in conflict resolution than in seeing one side win” (p. 220) is surely a subtle statement of preference for the status quo.

Zartman concludes by asserting that “it is in both American and African interests to provide assistance in regional peacekeeping and peacemaking functions now” (p. 251). Yet throughout, the prescriptions are tactical (p. 250) rather than structural, and the costs of containment miniscule compared with the price of development. The social bases of conflict cannot be forever constrained by negotiation, no matter how professional.

Nevertheless, the limitations (no political economy) and contradictions (globalist and/or Africanist preferences) of Zartman’s framework, most apparent in the 20% of the volume’s endpieces concerned with context and contrast, do not seriously detract from the utility of his excellent case studies. In these definitive analyses of continuing regional conflicts, Zartman recognizes a variety of actors, interests, periods, and connections, and he relates geography to strategy, and personality to process. He also touches on history—e.g., the alternative, antagonistic Ethiopian (p. 77) and Somali (pp. 74–76) “empires”—economy, and ecology, as well as on some remarkable “indigenous” defensive measures such as Morocco’s desert wall (pp. 39–40) and South Africa’s Nkomati Accord (pp. 200–201). Yet the state- and Americo-centric perspective is present throughout, tending to exclude either socialist powers or proletarian forces.

Aside from the juxtaposition of continent and context, the major contribution of Zartman’s treatise surely lies in the notion of the “ripe moment” (pp. 9 and 220):

Moments, when ripe, do not fall into one’s hands; they have to be taken with skill. . . .

Thus, for the conciliating power, it is a question not only of correctly identifying the right times to move but also of moving the times with skill. (p. 237)

However, such conjunctures are quite rare (e.g., they occur in Zimbabwe but not yet in Namibia), and reflect economy and polity as well as diplomacy and strategy.

Moreover, although the author adopts a common structure for each substantive chapter—from background and conflict to conflict resolution—he fails to contemplate whether the Sahel and Shaba, and the Horn and Namibia really are comparable in terms of history, intensity, centrality, or sustainability. In particular, the potential regional military-industrial complex of South Africa may put it into a distinct category in terms of destructive potential. Furthermore, the complexities of such relatively industrialized or urbanized political economies inevitably generate internal differences and disagreements over foreign relations—e.g., the *verligte-verkrampste* stand-off in South Africa (p. 205) or the *mestiços*—black fight in Angola (p. 194). Thus, there could be reasons other than ripeness for the successful resolution of the anticolonial struggle in Zimbabwe—e.g., decolonization—compared with continuing tension on the Horn—e.g., different definitions of state, nation, and territory.

Despite the historical insights and tactical aspects, Zartman’s policy proposals are more modest than his analytic frameworks. Although negotiation skills can be enhanced, he concludes that “only time resolves conflicts, but time needs some help” (p. 237). Yet, moment, or conjuncture, is not defined by diplomacy or strategy alone; it can also be determined by ecology and economy. Zartman occasionally recognizes such structural constraints or determinants—e.g., the relentless encroaching of the desert in Mauritania (p. 35) and the Horn (p. 91), or the low cost of South Africa’s in-depth defense of Namibia (p. 166). Yet the six causes of continental conflicts clearly have social as well as strategic roots, such as “leftover liberation movements” (pp. 12–16). Already the real cost for South Africa of regional destabilization and national repression is greater than the author calculates. Thus, Zartman’s welcome sense of changing

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time and place in terms of interstate negotiation fails to extend to intrastate contradiction. Nonetheless, this is an important if not seminal contribution to both African and conflict studies, supported by wide-ranging sources

and attractive maps. The continent may yet be ripe for a conceptual revolution.

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POLITICAL ECONOMY

Forecasting Political Events: The Future of Hong Kong. By Bruce Bueno de Mesquita, David Newman, and Alvin Rabushka. (New Haven: Yale University Press, 1985. Pp. x + 198. \$22.00.)

The search for a theoretically based model that will enable analysts to make accurate forecasts of events and policies has, to date, yielded quite meager results. This stimulating and provocative book presents and applies a mathematical model that, its authors claim, enables us to make such predictions with a high degree of precision. This claim must be taken seriously, because their approach is an extension of the expected-utility theory that Bruce Bueno de Mesquita developed and tested in his important book *The War Trap* (Yale University Press, 1981).

The components of the model, the mathematics involved, and the assumptions upon which the model is based are explicitly and lucidly laid out in chapter 2. Policy choices are the outcome of conflicts between groups headed by strong leaders who act rationally. Hence, the leader of any group will select the policy with the greatest net gain. Decisions are influenced by such factors as orientation towards risk, the anticipated impact of the actions of other groups, and the perceived importance of the issue. A set of equations that incorporates these considerations enables the analyst to calculate the expected utilities of the major actors, and thus to predict the likely outcome of any policy conflict. The remainder of the book is devoted to applying the model to the case of Hong Kong. The analysis suggests that, even though the Chinese government will try to maintain the special position of Hong Kong, domestic political pressures

will inexorably lead to the erosion of differences and to important policy changes that are explicitly spelled out.

The approach has strong appeal, for several reasons. The model is parsimonious, flexible, and can be used for simulations that permit the analyst to consider different assessments of any situation and to evaluate negotiating positions. Most importantly, it seems to work: it accurately predicted the outcome of the negotiations between the Chinese and the British on sovereignty, the economy, legal rights, and currency, and came very close on property leases. Moreover, the authors are sensitive to its limitations, which include the need to refine the model so that it can handle such elements as log rolling and tradeoffs.

The model did not achieve complete accuracy because "a key alternative was not identified. . . . The experts did not anticipate" (p. 133). In other words, the accuracy of the forecasts ultimately depends, as in any model, upon the reliability of the data that are utilized. Expert judgments concerning the identity, attitudes, and capabilities of major domestic and external actors are required, but specialists have often differed in their analyses of domestic political configurations, and even a consensus may be erroneous. The authors recognize but tend to minimize this problem. It remains to be seen just how serious it will turn out to be in practice.

Nor is it at all clear why the model should work, given the nature of its assumptions concerning rational behavior. A large body of theoretical and empirical literature has demonstrated that the "rational actor" model does not accurately describe how decisions are actually made in a large variety of situations. Rather, it suggests that leaders engage in "satisficing" behavior, and that bureaucratic and

other factors severely constrain the "rationality" of the decision maker.

That such a theoretical conflict exists is testimony to the quality of the work reported in this book. Although persons familiar with the expected utility theory will probably find little that is new here, others will find this short volume a useful and important introduction to a new way of carrying out political analyses.

JOSEPH SZYLLOWICZ

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Paradoxes of Rationality and Cooperation: Prisoner's Dilemma and Newcomb's Problem. Edited by Richmond Campbell and Lanning Sowden. (Vancouver: The University of British Columbia Press, 1985. Pp. 366. \$15.95.)

The value of a paradox is that it raises issues. Although it cannot be concluded from Richmond Campbell and Lanning Sowden's collection that Prisoner's Dilemma and Newcomb's Problem are paradoxes of the traditional sort (many contributors argue that they are not, for a variety of reasons), there can be no doubt that questions of importance and difficulty are addressed in these two problems.

This volume concentrates on philosophical questions. It focuses on the foundations of decision theory, but begins with ethics and ends with a discussion of the social contract and "tit-for-tat." Its aim is to show the development of its chosen issues, mixing reprints of "classics" with original papers that are often developments or rebuttals. The mixture is good and the objective is achieved in large measure.

The material is organized into four sections—on one-shot Prisoner's Dilemma, on Newcomb's Problem, on the connection between them, and on iterated Prisoner's Dilemma—that are preceded by a general introduction. The introductory section by Richmond Campbell provides adequate background and connecting material for the entire book.

Immediately following, articles by Lawrence H. Davis, John Watkins, David Gauthier, and Edward F. McClennan explore the nature of play in one-shot Prisoner's Dilemma, raising moral questions relating to the choice of cooperation as well as some corresponding

issues in decision theory. The next section, on Newcomb's Problem, contains an introductory article by Robert Nozick, a resolution proposed by Allan Gibbard and William L. Harper, and considerable debate involving Terence Horgan, Ellery Eells, and Frank Jackson and Robert Pargetter. It concludes with Isaac Levi's broad criticism of decision theory.

A player of Prisoner's Dilemma who attaches a high probability to his opponent's matching his strategy choice faces a decision problem that is (arguably) essentially Newcomb's Problem. This situation is examined in the subsequent section by David Lewis, Lawrence H. Davis, and Jordan Howard Sobel.

The concluding segment contains discussion and analysis of various extensions of Prisoner's Dilemma, including repeated play, and more than two players. Reprints of articles by David Braybrooke, Jordan Howard Sobel, and Robert Axelrod, and a new contribution by Russell Hardin, raise questions about the nature of cooperation, the development of the social contract, and the extent to which Prisoner's Dilemma has thrown and can throw light on these questions. The need for an analysis of the development of cooperation in multiplayer Prisoner's Dilemma is a common theme.

Campbell and Sowden's book is resolutely philosophical in its material and approach. It will therefore only be of marginal interest to many political scientists (such as formal theorists) and others who use Prisoner's Dilemma as a basic modeling tool. Nonetheless, this collection is well organized and well introduced, and like a good paradox, succeeds in raising issues.

D. MARC KILGOUR

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Modeling Metropolitan Economies for Forecasting and Policy Analysis. By Matthew P. Drennan. (New York: New York University Press, 1985. Pp. xx + 242. \$40.00.)

Using annual data from 1958 to 1978, Drennan develops econometric models for the New York-Northeastern New Jersey Standard Consolidated Area (SCA) and New York City (NYC). He estimates income equations for 53

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industries, which is much more than the average for regional models. Such a high degree of disaggregation has a price, however. The model is recursive rather than simultaneous, with the results of the export sector equations used to drive most of the local industry income equations. National value-added-per-employee equations for each industry are developed in order to translate industry income forecasts into forecasts of industry employment.

In order to demonstrate the generality of this approach, a model of the Baltimore Standard Metropolitan Statistical Unit (SMSA) using annual data from 1958 to 1976 for 28 industries is also presented. Drennan then extends the results of the SCA and NYC models. Utilizing industry-occupation matrices developed by the New York and New Jersey State Labor Departments, the occupational breakdown for each industry is obtained. In order to make short-term forecasts, quarterly versions of the SCA and NYC models are developed for seven sectors. In addition, Drennan identifies the major sources of revenue for NYC, and develops a model of the NYC fiscal sector. Finally, the annual version of the SCA and NYC models are used to generate long-term forecasts for 1987 and 1990. The tax revenue equations of the NYC fiscal model are used to project revenues for fiscal years 1986, 1987, and 1988. When these results are compared with the expenditure forecasts in the New York City Financial Plan of January 1984, substantial deficits result.

The book begins with a brief review of regional econometric modeling and a good discussion of export base theory with an explanation of the identification of the export and local sectors of a regional economy. In describing the data, Drennan provides a summary of historic economic trends showing the decline of manufacturing and the rise of the service sector. Consequently, the book is tractable for the novice in regional modeling.

The book could be improved by including more detailed discussion of alternative model specifications. While space limitation in journal articles often precludes such detail, this should not be a problem in a book. Drennan states that "all estimated equations with serious multicollinearity were rejected" (p. 42), yet he neglects to include the condition number for each equation. Given the lack of policy

handles among the explanatory variables, an objective measure of multicollinearity is desirable. This absence of potential policy handles becomes quite clear in the latter part of the book, where Drennan deals with public policy questions. While he correctly points out that local government can stimulate the economy by lowering relative costs, one certainly does not need an econometric model to arrive at this conclusion. He then estimates a new NYC model using a relevant city tax as an independent variable in each equation. One wonders why this approach was not taken originally. In summary, Drennan attempts nothing new. Nevertheless, he provides a case study of regional modeling techniques that is of use to those with an interest in these particular places.

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The Social Production of Urban Space. By M. Gottdiener. (Austin: University of Texas Press, 1985. Pp. viii + 318. \$27.50.)

Gottdiener approaches urban analysis as an issue of social theory. His book will be of interest, therefore, to a larger audience than those particularly concerned with urban issues. Indeed, the heart of his argument centers on his contention that social theorists' indifference to spatial relations renders current theoretical approaches ineffective for understanding and explaining contemporary social and political change. Similarly, the lack of attention to changes in social organization leads to deterministic, superficial explanations of modern urban development by both mainstream and Marxian urban analysts. To rectify these gaps in both social theory and urban analysis, Gottdiener proposes a new paradigm, based on Henri Lefebvre's work, that focuses on the dialectical relations of state, society, economy, and space. He calls this the "production of space" perspective, and adopts a "structuralist" approach to describe how structural imperatives and human choice interact to produce contingent, rather than deterministic, outcomes in conflicts over the use of space. The normative implications of this emphasis on the social processes underlying urban devel-

opment place sociospatial conflicts alongside economic struggles as necessary elements in political transformation.

Gottdiener characterizes metropolitan de-concentration trends (into "polynucleated metropolitan regions") in the United States and a concomitant "revitalization" of some central city areas as "a qualitatively new form of settlement space" indicative of profound changes in deeper social relations. The inappropriateness of "outdated" analytic paradigms based on the bounded central city for explaining these trends prompts his reassessment of urban theory. He systematically reviews "mainstream" urban theory, based on ecological models of urban development, as well as Marxian political economy, Marxian structuralism, neo-Weberianism, and models of sociospatial dialectics used by European and American social scientists. In a detailed critique, Gottdiener presents—and finds wanting—the contributions of the Chicago school of social ecologists as well as those of David Harvey, Allen Scott, and Manuel Castells, among others. To political scientists unfamiliar with the nuances and subtleties of "the debate on the theory of space" that has raged since the 1970s, Gottdiener's pungent epithets will seem fascinating, sometimes amusing, elements of an arcane academic dispute. They lay the groundwork, however, for his turn to Lefebvre's theory of the social production of space, an approach Gottdiener claims more effectively integrates social and spatial change than the models reviewed. Although Lefebvre's argument is not as unfamiliar to American social scientists as Gottdiener supposes, the argument is complex enough to warrant some detail.

The author's advocacy of a new paradigm stems from limitations he sees in Marxian political economy's explanations of economic change and spatial form. In contrast to approaches associating spatial forms with specific phases of capitalist development (e.g., "the post-industrial city"), he contends that changes in urban form connote the contradictory relationship between investment in the primary circuit of industrial production and investment in the secondary circuit of real estate. Gottdiener distinguishes his analysis of these circuits from other theorists' approaches by his emphasis on the direct wealth-creating capacity of the secondary circuit, its contradic-

tory role in capital accumulation processes, and its generation of new social cleavages and territorially based interests that reflect sociospatial conflicts rather than displaced economic conflicts.

These investment circuits are seen as complementary: the secondary circuit "siphons off" capital from the primary circuit, and thus offers a means of combatting falling rates of profit and avoiding accumulation crises. In contrast to political economy perspectives, both Gottdiener and Lefebvre contend that real estate investments are a direct means of capital formation, but they argue that this complementary relationship is "essentially unhealthy." Investment in real estate, though a source of capital formation, means that less capital is available for primary production. Once invested in "the build environment," capital is not easily shifted to investment in new production activities. Furthermore, real estate investment appears to be increasingly attractive, relative to investment in the primary production circuit. Gottdiener attributes this increasing desirability to several factors, but notably to the wealth generated by seemingly unlimited transformations in the use of a land parcel. In the United States, social values and institutional arrangements protect private profits gained by these land investment activities. They also ensure that state intervention consists of indirect policy support for restructuring activities, rather than direct planning of the use of space. At the local level, in particular, they encourage local officials to facilitate real estate investment activities. With this weak state role and an essentially uncoordinated real estate investment circuit, there is a tendency towards diverting either too much or too little investment from the primary circuit. This exacerbates crisis-prone economic swings and, as Gottdiener notes, results in development processes that are not necessarily functional for capitalism or logical for capital accumulation processes.

This is an ambitious effort and a complex, compelling argument. It links uneven local development patterns and processes to global economic changes, and describes the spatial equivalent of conflicts of use and exchange values. It portends a local politics increasingly dominated by conflicts over the uses of land and by groups' efforts to renegotiate the distributional and environmental impacts of

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development activities. It directs political scientists' attention to an urban research agenda centered on the "politics of spatial production," particularly the actions of class fractions such as finance capital and other interests organized around land investment, and the state. Finally, it argues that, to the extent that political interests are identified with these sociospatial concerns rather than with class interests, conventional political economy approaches to urban analysis will be inadequate in addressing this new research agenda.

Whether Gottdiener's paradigm offers a useful alternative is somewhat problematic. Its explicit focus on the American case is understandable but limiting, and there is surprisingly little discussion of the extent to which this paradigm is useful in explaining non-American development patterns. Unfortunately, the final chapters discussing the political articulation of these sociospatial dynamics are not as cogently argued as the initial critique. In particular, the vague specification of the secondary circuit reduces this promising insight to a familiar emphasis on real estate and finance interests and a surprisingly narrow consideration of the local state. Gottdiener's notion of a pro-growth network is a weak derivative of the concept of a pro-growth coalition figuring in many recent analyses. It does not suggest a new or distinctive understanding of these relationships. Similarly, his pat portrayal of the local state as a virtual "marriage" of public power and private real estate interests veers away from his structurationist perspective by ignoring contingent community responses to restructuring pressures. It fails to consider the conditions under which such variations occur, and overlooks the tensions stemming from local officials' needs to accommodate electoral and economic constituencies. Finally, Gottdiener gives scant attention to political scientists' contributions to urban theory. While not necessarily an egregious omission, incorporating evidence from the many empirical analyses of sociospatial politics would strengthen his arguments and deepen our understanding of their implications for political life.

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An International Political Economy. Edited by W. Ladd Hollist and F. Lamond Tullis. (Boulder, CO: Westview Press. Pp. viii + 300. \$32.85, cloth; \$14.85, paper.)

An International Political Economy is the inaugural edition of a new series to be published under the auspices of the International Political Economy Section of the International Studies Association. Its avowed purpose is to explain the structure and evolution of the international economy, and to detail the economic and political impact of change on members of the system. As the editors note, we are living in a time of dramatic change, when the orthodox modes of looking at the world no longer suffice to help us make sense of the changes. The problems that rapid structural change impose are normative, empirical, and policy-political (p. 5), as well as theoretical. The series is intended to be an open forum for discussion of these problems.

This, the first volume, is an overview of the field gathering together 10 essays divided into three broad categories: (1) defining international political economy (Susan Strange, and Ladd Hollist and James Caporaso); (2) exploring the evolution of the system (Christopher Chase-Dunn, Robert Solo, and Stephen Resnick, John Sinisi, and Richard Wolff); and (3) exploring outcomes in the specific issue areas of international debt (Jonathan Aronson), industrial crisis (Robert Walters), multinational corporations (Robert Kudrle), commodity trade (Jock Finlayson and Mark Zacher), and rural development (F. LaMond Tullis).

In the first part, Susan Strange finds international political economy (IPE) to be the dubiously legitimate offspring of both international economics and international relations. Arguing that the former has concentrated on efficiency, treating politics as an exogenous variable, and the latter on peace, treating economics as a mere handmaiden to the more important task of preserving the system, Strange finds neither sufficient to address the problems of the interaction of politics and economics. While IPE carries the genes of both parents, it wholly resembles neither. She finds fruitful sources for concepts and theories in economic history, development economics, and international business. Similarly, Hollist and Caparaso, noting that

"derived economic and political concepts are of disappointingly little help in understanding political-economic issues" (p. 37), argue for greater attention to economic and social history and comparative social history, along with greater attention to how the state shapes and is shaped by interaction with the market.

Both Chase-Dunn and Solo adopt historical perspectives on the evolution of the international system and the state, respectively. Both find the territorial state to be incapable of dealing with problems posed by the globalization of the economy and interdependence. Both argue that the nation-state must be transcended, either by some confederation that would make the pursuit of "collective rationality" possible (Chase-Dunn), or by some international civil service to manage the problems that have escaped from the grasp of national governments (Solo).

The most interesting and most original attempt to deal with the structural aspects of the international political economy is the rereading of Resnick, Sinisi, and Wolfe of Marxist class analysis focusing on the question of foreign exploitation. They argue that the analysis of aggregate capital transfers is an inadequate indicator of exploitation. The key to their argument is the disaggregation of capital transfers into that produced by the creation of surplus value and that which is a product of the redistribution of surplus value. Only the former qualifies as an indicator of "foreign exploitation," and it could be nil even in the face of high levels of direct transfers from the periphery to the core. Nationalism may halt foreign domination, replacing it with national exploitation or social revolution, and yet high levels of aggregate payments would still flow northward, though now in the form of distributed surplus, and by definition not part of the mechanism of exploitation. They suggest that there is no necessary relationship between capital transfers and foreign exploitation, or between nationalist revolutions and the end of exploitation and foreign dominance. If we accept the logic of their complicated theoretical analysis, then much of the theorizing about peripheral dependency rooted in transaction analysis must be revised. Conclusions about the exploitive or beneficial effects of integration into the capitalist world economy drawn from radical and orthodox analysis are suspect. Many, however, will find their

narrow focus on exploitation to be of minor importance when compared with the larger questions of the impact of foreign investment on development.

The policy section contains five empirically oriented chapters detailing how policy makers have and ought to respond to the sweeping changes confronting them. Aronson, for example, examines the complex state of financial interdependence and the "wild dance on the chasm of calamity" by banks, creditor countries, debtor countries, and international institutions. Creditor countries, particularly the U.S., need to pay closer attention to the intimate links between trade, development, and finance if a collapse of the system is to be averted. Walters explores U.S. patterns of reaction to change in steel, automobiles, and semiconductors, and argues that the least costly and most likely policy alternative—protectionism—is not likely to succeed in remedying the underlying production problems. He joins others in calling for a national industrial policy to supplement trade policy. Robert Kudrle applies public choice theory to Multinational Corporations (MNCs) in an attempt to account for the differences in response to multinationals between Less Developed Countries (LDCs) and developed countries. He argues that a public choice approach helps identify issues about MNCs but little support for this argument is found in the analysis. Finlayson and Zacher analyze the negotiations over International Commodity Agreements in order to understand the divergences between normative goals of general commodity regulation and the outcomes in specific commodity regimes. Lastly, Tullis reviews the current thinking on rural development as an alternative to the urban-centered development policies of the past. He advocates extending massive credits to small-scale, labor-intensive agriculture as a way of shifting from export-oriented agriculture toward production for domestic consumption. An analysis of the political impediments to such reform movements in Latin America ends on a pessimistic note.

The yearbook is off to a good, if somewhat uneven, start. Though we can all concur that neither politics nor economics is sufficient to address many problems, the definitional section too easily overlooks the contributions of rational choice models and the literature on

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regime formation and maintenance, and gives scant attention to hegemonic theory. Much of the literature that the authors suggest needs to be incorporated into the field has already been incorporated by way of world-systems analysis and dependency analysis. Furthermore, many of the pieces seem compilations of existing material, with only a few (e.g., Resnick et al., Kudrle, and Tullis) bringing new theoretical insights to bear on the literature. However, these are minor qualms, and the editors indicate that they will be addressed in future volumes.

This IPE sampler stands as an interesting introduction to the field—more useful for novices than old hands. Yearbooks and annuals in the social sciences have come and gone, and few have left any lasting mark on their respective fields. With vigorous editorship and the selection of material that both challenges and informs, this one could break the cycle.

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The Limitation of Conflict: A Theory of Bargaining and Negotiation. By L. N. Rangarajan. (New York: St. Martin's Press, 1985. Pp. 327. \$32.50.)

In this volume, L. N. Rangarajan, Ambassador of India to Tunisia, attempts to construct a theory of bargaining based in large part on his experiences in multilateral negotiations. While it is refreshing to read a theoretical work written by a practitioner, the book fails to provide many useful new insights.

What Ambassador Rangarajan puts forth as a "theory" is in fact a series of loosely connected essays on various aspects of bargaining and negotiation. To his credit, the author addresses topics not usually covered in the standard texts on this subject, such as the importance of time and memory in the negotiating process. Unfortunately, so many areas are covered that the author is largely unable to devote sufficient attention to any one of them. As a result, an uninitiated reader will be pumped full of information and anecdotal evidence on a variety of subjects, but will likely

reach the end of the book without a clear idea of how it all fits together.

Ambassador Rangarajan's desire for a new approach to the study of bargaining is based, in part, on his belief that game theory is "inadequate" for the purposes of analyzing the topic (p. 7). However, his objections—that the theory is "static" (p. 50) and "concerned only with those negotiations which are assumed, *a priori*, to end in agreement" (p. 7)—simply do not hold water. Indeed, recent work in the field on the topics of sequential bargaining and sequential equilibria answer many of the questions raised by the author in a much more rigorous, if somewhat less widely understood, fashion.

It is perhaps the case that the author's confusion on the usefulness of game theory results from his desire for unique solutions to bargaining problems. As an example of the difficulty of applying game theory to *n*-person negotiating situations, Ambassador Rangarajan puts forth a game in which three men (A, B, and C) are asked to divide one unit of currency between them, the division to be determined by majority vote. As in the related game of Couples, the imputation (1/2, 1/2, 0) can be countered by player C with (0, 2/3, 1/3), which can be countered by player A with (1/2, 0, 1/2), etc., and a unique equilibrium does not exist. The author concludes the description of this example by observing that "having fallen into the trap of circular reasoning, game theory cannot find a rational and optimal strategy for the three players" (p. 144). Of course, that this situation does not have a unique equilibrium should not lead anyone to the conclusion that game theory is useless. Indeed, what the author seems to ignore is that many games, and many "real-life" bargaining situations, have many possible outcomes.

To confuse matters even more, the ambassador's solution to the game is an equal division of the unit of currency—"may be giving the odd left-over penny to the first who propounds this unique solution!" (p. 145). This agreement will be arrived at because it satisfies the condition of "equality of dissatisfaction" among the players—a solution concept invented by the author. However, this imputation, (1/3, 1/3, 1/3), is dominated by any number of other divisions, and hence, cannot be included in the set of possible solutions. The ambassador's solution obtains only if all par-

ties desire to be "fair" or place a very high value on reaching a unanimous agreement. Rarely do the participants in negotiations have these preferences.

It is also disconcerting that the author devotes a chapter to coalition formation, size, and disintegration without citing Riker (*The Theory of Political Coalitions*, Yale University Press, 1962), and a chapter to bargaining power that addresses weighted voting but ignores the Shapley power index. Indeed, the gaps in Ambassador Rangarajan's coverage of existent works on bargaining are so large that I cannot recommend this book to advanced undergraduate or graduate students.

Finally, it should be noted that the final chapter, "World Order and Conflicts: The Next Fifteen Years," is largely unrelated to the rest of the book. Instead it contains the Ambassador's personal assessment of the current international environment. Unfortunately, these observations are limited to the bashing of Ronald Reagan, Margaret Thatcher, *The Economist*, and proponents of stable nuclear deterrence. It should go without saying that writing such as this is better suited for the editorial pages than an academic publication.

WALTER J. PETERSEN

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Social Science and Social Policy. Edited by R. Lance Shotland and Melvin M. Mark. (Beverly Hills, CA: Sage Publications, 1985. Pp. 375. \$29.95.)

There is a bit of irony in this excellent book. It is dedicated to the memory of Carolyn Wood Sherif, "who was firmly committed to the use of the social sciences to improve the human condition and to further social justice," yet one comes away from the book feeling that Harold Lasswell's vision for the "policy sciences," in which researchers and practitioners work together to improve public programs, is still a long way off and may never be reached.

The principal message of most of the 19 authors is that the role of social science is primarily technical: it cannot and should not enter into value questions. For example, in their chapter on gun control, Rossi and Wright contend,

A society in which social scientists play crucial policy roles through their research is a society in which human values have been subordinated to technical considerations. . . . In a truly democratic society, social science must be content with an advisory but not dominating role. (p. 331).

However, the book also takes some steps away from this strictly positivist position concerning facts and values. In the opening chapter of part 1, which deals with general issues, Thomas D. Cook argues that positivism is dead and should be replaced by "multiplism," by which he means multiple operationalism and multi-method research. Cook rejects determinism, belief in perfect prediction, attempts to reach generalizations, belief in value neutralism, and insistence on operationalism. He does retain the positivist assumption that there is a real world that we can know to a useful, though imperfect, degree through observation.

The remaining chapters in part 1 point out obstacles to ideal research. Sechrest notes that almost all of the data on which we depend are biased and distorted, leading to the "garbage in-garbage out" syndrome. McLaughlin writes that because implementation choices dominate outcomes, and implementors pursue multiple and often competing goals, it is impossible to prove a program works as we do a principle of physics. Pettigrew says that social science research is filtered through the news media, and that this leads to great distortion, since the media do not know how to cover social service.

Part 2 of the book focuses on policy areas. Here again, the authors give social science a narrow, technical role. For example, Taylor argues that social science research cannot resolve the difficult issues in affirmative action: "Who should lose, and who should gain, and who should shoulder the burden of righting society's wrongs—these questions are philosophical, not scientific" (p. 139). Paula Cain's conclusions about comparable worth in the next chapter are similar; real progress in this area was not due to breakthroughs in scientific knowledge (although the development of job evaluation systems helped); it was due to judicial interpretation of an obscure amendment to Title VII of the Civil Rights Act of 1964. The final chapter in this section by Henry Levin reaches identical conclusions: Social scientists cannot answer the question of what the level of unemployment should be,

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only questions about the causes of unemployment (and these not very well). This kind of knowledge, he says, is "quite marginal to the political process that determines normative reality and political strategy" (p. 173).

In the next section of Part 2, St. Pierre, who was involved in evaluations of follow-through and compensatory education, describes the controversies and misinterpretations these evaluations generated. The follow-through evaluation, St. Pierre found, was faulty and misinterpreted, but compensatory education evaluations were used by Congress because the evaluators took the time and effort to answer the questions of interest to Congress. Still, most evaluations are not used because the programs they study do not have big impacts.

Crain and Carsrud are much harder than St. Pierre on social science in their discussion of desegregation. Researchers in this area did not focus on the questions that would be helpful to policy makers—questions pertaining to the best ways of implementing desegregation—but instead looked at whether or not desegregation helped blacks. Why? Because academic institutions do not value applied research. As a result, policy makers have one set of questions and evaluators answer a different set. In the last chapter in this section, Jay Belsky argues that although there is considerable policy-relevant information available about day care, "day care policy seems to develop out of a political process that has little if anything to do with scientific inquiry" (p. 240).

In the final section of Part 2, Charles Tittle looks at research about deterrence and concludes that we know almost nothing about it, even though a good deal of research has been done:

There is now no compelling reason to believe that social science-based policies concerning sanctions would be more efficient, useful, or morally justifiable than those that emerge from ordinary policy debate. (p. 290)

In the same vein, Jeanne C. Marsh finds that rape legislation in Michigan was helpful, but that social science research did not have much to do with it because decisions in the policy process are influenced by interest groups and events, and only minimally by relevant knowledge. As I noted above, in the final chapter on this section Rossi and Wright reach the same conclusion: Even in the best of circumstances,

social science's ultimate impact on policy formation will typically be modest, and this is all it can do.

One of the principal reasons why social science plays such a limited role in policy, according to many of the book's contributors (none of whom is a political scientist), is that political factors are far more important than knowledge. The authors are psychologists and sociologists. If politics are so important, then perhaps political scientists should become more heavily involved in policy analysis. Perhaps this would lead to a more effective role for social science knowledge, since political scientists understand more about the political conditions under which social science knowledge might become more useful. In any event, this is an excellent book that would be a useful text in courses on public policy evaluation.

DENNIS J. PALUMBO

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Taxes, Loans, and Inflation: How the Nation's Wealth Becomes Misallocated. By C. Eugene Steuerle. (Washington, D.C.: The Brookings Institution, 1985. Pp. xii + 205. \$26.95, cloth; \$9.95, paper.)

This book is a very detailed study of the uneven tax and market treatment of capital income produced by the tax system in the United States. It specifically emphasizes the distorting effects of the tax system on capital income flows and loan markets, and the resulting disparity between taxable capital and labor income.

The book accomplishes four separate tasks, which are intertwined in the chapters. The first, in the best Brookings Institution tradition, provides carefully calculated estimates of: capital income from individuals and corporations; the amount of capital income that is taxable, and the effective tax rates broken down for individuals by income groups and for corporations by type of investment. Although tax experts have discussed at length the differential tax treatment of capital income versus labor income and the uneven tax treatment between types of capital investments and sectors, Steuerle provides the best and most comprehensive data to date on these subjects. This section of the book is a gold mine for

opponents of the current income tax system. The author's data provide unqualified critiques of the current system based on grounds of both equity and efficiency.

The second part of the book, which will be new to most political science readers and many public finance specialists, provides a fascinating tour of the world of tax and financial arbitrage. Arbitrage is based on the ability to make financial gains by borrowing to acquire tax-preferred assets. The leverage that makes this possible is the tax-exempt status of some assets (e.g., municipal bonds) combined with the deductibility of interest payments on borrowed money. My more highly paid colleagues may recover the purchase price of this book many times over by putting into practice Steuerle's detailed description of arbitrage in chapter 5. Perhaps most interesting is the prospect he opens up in chapter 6 by outlining in theory a "reverse arbitrage" procedure, whereby futures markets in financial assets would allow those in lower tax brackets to borrow to purchase taxable assets, and still realize after-tax gains. Thus our tax system has realized yet another perverse equality by opening up the world of arbitrage to the poor and middle-class as well as the rich.

The third objective of the book is to assess the implication of the tax treatment of capital income, with special emphasis on the effects during periods of high inflation. The major implication, according to Steuerle, is that the tax system is separating a true measure of economic income from taxable income, financial income (as reported on financial statements), and cash flow accounting. This leads to misallocations of capital, distorted and ineffective incentives for savings, and artificial changes in organizational structures (mergers, divestitures, etc.). In a separate chapter, Steuerle convincingly argues that these distortions are enhanced by inflation. In addition, because most macroeconomic models assume "neutrality" of inflation and inconsequential effects of the tax system on capital markets, macro policy is also adversely affected, and must try to cope with constant instability in financial markets.

The final, and weakest, section of the book briefly outlines the theoretical effects of various tax reform proposals on problems of capital taxation. Steuerle reviews only the most straightforward efficiency effects of base-

broadening combined with rate reduction, a flat tax, indexing proposals, and a comprehensive expenditure tax. Based on the reduced distortion that would result from each, Steuerle seems to applaud all these efforts, despite glaring controversies that arise when other criteria are introduced in the debates over these proposals. Because a separate book or series of books might be required to compare these approaches to tax reform adequately, his omissions are understandable. However, it does leave the reader with a sense that, for example, a base-broadening approach is roughly equivalent to an expenditure tax as a solution. Although this may be true for problems of financial market distortion, there are major equity issues between an approach that treats all income as taxable and one that exempts all capital income.

This book is essential reading for political scientists involved explicitly in tax policy and politics. I would also strongly recommend the book for those generally interested in political economy. For the latter group, the effort required to read and comprehend the book will be repaid through acquisition of a detailed understanding of real-world capital markets. For that group of political economists whose forte is "economist baiting," Steuerle's work provides ammunition both by exposing the simplicity of capital market assumptions incorporated in economic models, and by further shattering images of efficient market flows—in this case the flow of capital.

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The Politics of Industrial Change: Railway Policy in North America. By R. Kent Weaver. (Washington, D.C.: The Brookings Institution, 1985. Pp. vii + 291. \$30.00.)

Industrial policy will undoubtedly reemerge as a political and economic issue in the next U.S. presidential campaign, just as it will continue to be debated within the Canadian parliamentary system. Those who seek a deliberative and balanced cross-national assessment of how public enterprise has been, and might be, pursued in the United States and Canada as an

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instrument of industrial policy will find much that is worthwhile in this study. Rooted in his 1982 doctoral dissertation, Weaver has generated an interdisciplinary work of policy analysis sensitive to economic, political, and public administrative forces.

Why did Weaver select railroads as an object of analysis in the domain of industrial policy? He indicates that any study of government's role in facilitating industrial adjustment must consider methods and tools government can use to intervene in specific industries. There are many instruments of intervention, but public enterprise via nationalization may be the most far-reaching and controversial tool available to government. The U.S. government has been historically reluctant to use nationalization in addressing problems of domestic industrial adjustment. However, major recent exceptions are Conrail, a consolidation of bankrupt northeastern freight-dominated railroads, and Amtrak, a nationwide intercity passenger rail service firm. Both Amtrak and Conrail exist as U.S. versions of public enterprise. Canada has a long tradition of use of public enterprise corporations, and has proliferated a great many Crown corporations. The Canadian National Railway, which competes throughout Canada with private freight-hauling railroads, and the newer VIA Rail Canada national passenger service firm are both public enterprise institutions. These four railroads represent the major empirical data base for the study.

A paramount assumption of the work is that "the industrial adjustment problem should not be viewed primarily as a trade problem" (p. 2). Weaver argues that many industries that are not subject to fierce international competition—railroads, for example—"face the same adjustment dilemmas as trade-threatened industries and will experience many of the same protectionist outcomes." Therefore, the rail sector affords, according to Weaver, an excellent arena for comparative examination of industrial adjustment policy and public enterprise in the United States and Canada. He concedes that "the reader must be convinced that the case is not idiosyncratic but generally applicable" (p. 27). This reviewer remains somewhat skeptical. The railway case has little in common with the plight of current North American steel, textile, and auto manufacturers. A central stimulus to debate about

industrial policy has been concern about failing industries threatened by foreign competition. The decline of U.S. and Canadian railroads can be attributed to many factors (competing modes of transport, high labor costs, huge maintenance and tax costs, etc.) but not to foreign competition.

Nevertheless, Weaver's thorough analysis of the problems of U.S. and Canadian railroad public enterprise operation generates a sufficient body of theory, generalizations, and testable propositions. The cross-national comparison is so sound that this reviewer found significant merit in the study, even if the case is rather idiosyncratic.

In addressing types of industrial policies, Weaver presents market-oriented policies geared toward general macroeconomic and social policies and triggered by market signals (unemployment compensation, education and training programs, monetary policy credit controls, etc.), protectionist policies intended to provide sector-specific avoidance of concentrated costs for any domestic group (tariffs, import quotas, regulation), and accelerationist policies designed to speed up transitions when political markets overwhelm economic markets, or when private markets appear unlikely to produce efficient outcomes. Weaver seems most enthusiastic about use of accelerationist policy. He indicates that the policy that may be most appropriate for a given set of circumstances must take into account whether the firms or sectors addressed are occupied by sunrise, mature, or declining industries. Railroads in North America are a declining industry that, according to accelerationist policy, must reduce excess capacity, promote exit of inefficient firms, and restructure the industry to be more competitive.

Weaver concludes that the major limitations in formulating, legitimating, and implementing industrial policy are domestic and political. Variations in political structure exist from one country to another, such that it makes little sense to pattern U.S. or Canadian industrial policy after Japanese or Western European models. In spite of its industry-specific focus, the analysis and findings of Weaver's study place it among more general economic analyses of industrial policy by Robert Reich, Ira Magaziner, Lester Thurow, and Amitai Etzioni. The political and public administrative environment profoundly affects both the

management and economic viability of public enterprise.

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The Economic Institutions of Capitalism: Firms, Markets, Relational Contracting. By Oliver E. Williamson. (New York: Free Press, 1985. Pp. 450. \$27.95.)

In 1937 economist R. H. Coase published "The Nature of the Firm," a short paper that, like much of Coase's work, has had enormous implications. Typically, Coase took nothing for granted and reinvestigated fundamental questions: Why are there multifunctional firms? Why should not markets prevail in every activity? Why, for example, does a large company like General Motors produce internally some things that it employs in automobile production and acquire still others in market transactions? In attempting to answer these questions, Coase invented a discipline—law and economics—and a set of concepts that are as applicable to the arena of political organization as they are to the activities of economic life. Administrative organization and markets were now viewed as alternative means to attain goals. The most important concept employed in the new discipline was transaction costs—the costs incurred in planning, negotiating, contracting for, and policing tasks. Coase's theoretical answer to the central question posed was that the comparative transaction costs at the margin will determine whether a firm chooses the market or internal organization to fulfill a task.

Like much ground-breaking work, Coase's triggered an enormous body of literature designed to clarify and amplify the original insights. The most important problem Coase raised but did not answer is the central focus of Oliver E. Williamson's brilliant new book: "Unless the factors responsible for transaction cost differences could be identified, the reasons for organizing some transactions one way and other transactions another would necessarily remain obscure" (p. 4). Williamson attempts to resolve the difficulty by providing first an elaborate theoretical framework within which the concept of transaction costs is central, but with which a considerable body of organiza-

tion theory, law, and economics has been integrated. Second, the theoretical framework has been applied to a large number of particular issues such as vertical integration, the organization of work, the structure of corporations, and the problem of natural monopoly.

Before proceeding further I must address a preliminary question. Why should political scientists read a book that is mostly concerned with discussions of such economic or management topics as backward vertical integration and the advantages of the M-form corporate structure compared to the U- and H-forms? The simple answer is that the law and economics framework provides as powerful a tool for examining political organizations as it does for examining economic organizations. Indeed, the leading journals in the law and economics discipline have published papers that have examined the U.S. Senate and administrative agencies as "firms." Using such concepts as transaction costs, information costs, and incentives, it is possible to gain considerable insight into the behavior of political organizations. Bargaining and logrolling, for example, are types of transactions in which legislators and others incur costs and about which we can spell out incentives. For political scientists whose primary focus is on public policies, the dichotomy between market transactions and internal governance has an obvious parallel in the dichotomy between government regulation and market mechanisms as alternative means to achieving certain results.

Williamson explicitly disclaims that transaction cost arguments sweep all others away. Rather, they are a powerful tool capable of generating considerable insight, and are often used most effectively in conjunction with other approaches. Nevertheless, the book's fundamental focus is on transaction costs, and—the ecumenical assertion notwithstanding—other approaches, when they are considered, are usually compared adversely to law and economics. However, the comparisons are almost always made to alternative economic approaches to human behavior that are based upon behavioral assumptions similar (although not identical) to those Williamson makes. Thus, Williamson explicates two important behavioral assumptions: bounded rationality and opportunism. *Bounded rationality*, taken from Herbert Simon, assumes a maximizing posture. However, because all of

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the relevant costs are not known, since information is costly, one undertaking a course of action incorporates risk and uncertainty. In contrast, neoclassical economics assumes that all pertinent costs are recognized. The second behavioral assumption, *opportunism*, is "self-interest seeking with guile. This includes but is scarcely limited to more blatant forms, such as lying, stealing and cheating" (p. 47).

It is here that Williamson's model, like so many in economics, manifests its limitations. To take one extreme case, transaction cost economics is inappropriate for studying the behavior of the insane, since the two behavioral assumptions do not apply. This raises the interesting problem of delineating the political realms in which transaction cost economics is useful alone, useful in conjunction with other approaches (for example, Marxism), and not useful at all. Is transaction cost economics useful in understanding the destructive (and, indeed, self-destructive) inclinations of regimes as diverse as Stalin's Russia, Hitler's Germany, Pol Pot's Cambodia, or Qadaffi's Libya? I suspect that Freud, Le Bon or the social anthropologists provide more powerful insights to understanding such important political phenomena.

I do not wish to take anything away from Williamson's remarkable achievement when I

point out that the political applications of transaction cost economics are apt to be most fruitful in constitutional settings of the type that prevail in the Western democracies and Japan. Costs are imposed upon business firms in such settings because the legal and political structures explicate and enforce rules defining loss and gain. Safeway does not literally bomb Kroger when it loses clientele to the latter, as it theoretically could in an unconstitutional regime. Bounded rationality is virtually compelled because costs can reasonably be enforced. The same dynamic applies to the House of Representatives or the Federal Communications Commission. To that extent, the second behavioral assumption—opportunism—must be modified. Bounded opportunism would be more appropriate.

When we recognize the settings in which transaction cost economics can be most fruitfully applied, and modify the second behavioral assumption, Williamson has provided a powerful mode of analysis that I hope will be more frequently applied to politics than it has been in the past. This is an extremely important book, both for what it says and for what it suggests.

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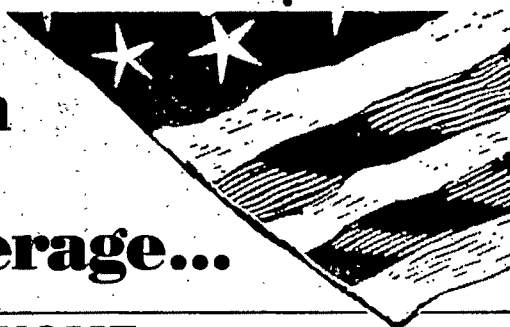
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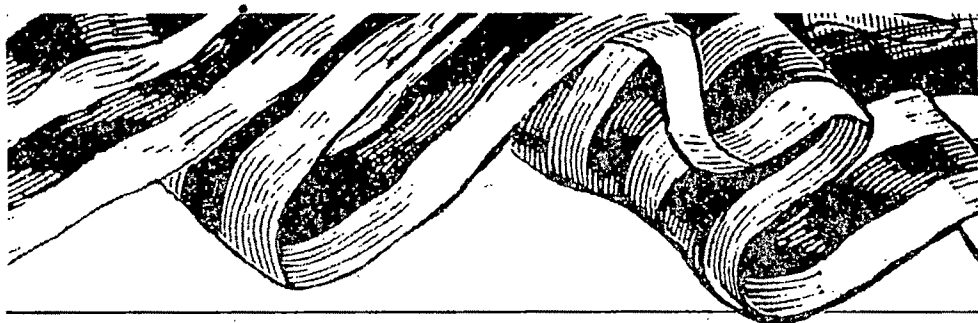
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